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**PROCEEDINGS of the XIIIth INTERNATIONAL CONGRESS
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THE XIIIth INTERNATIONAL CONGRESS OF LINGUISTS

Organized under the Auspices of the CIPL
(Comité International Permanent des Linguistes)
Nippon Toshi Center, Tokyo
August 29—September 4, 1982

Promoted by: The Linguistic Society of Japan

Assisted by: The Ministry of Education, Science and Culture
The Science Council of Japan

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Hiroyuki UMEDA

ICL 1982 Office: Gakushuin University, Mejiro 1-5-1, Toshima-ku, Tokyo, Japan-171

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History of the International Congress of Linguists in synopsis

		President	Number of Participants
I. 1928	Den Haag	C. C. Uhlenbeck	311
II. 1931	Genève	Ch. Bally	243
III. 1933	Roma	P. E. Pavolini	195
IV. 1936	København	O. Jespersen	411
V. 1939	Bruxelles	G. van Langenhove	(suspended)
VI. 1948	Paris	J. Vendryès	541
VII. 1952	London	R. Turner	436
VIII. 1957	Oslo	A. Sommerfelt	526
IX. 1962	Cambridge, Mass.	E. Haugen	944
X. 1967	Bucureşti	I. Iordan	1524
XI. 1972	Bologna	G. Devoto	983
XII. 1977	Wien	W. Dressler	1431
XIII. 1982	Tokyo	S. Hattori	1448

Synopsis of the XIIIth International Congress of Linguists (Tokyo 1982)

第13回国際言語学会議 (東京 1982) 概要

Date: Sunday, August 29 through Saturday, September 4, 1982

Place: Nippon Toshi Center, Hirakawa-cho 2-4-1, Chiyoda-ku, Tokyo, Japan 102

President: Shirō Hattori, Professor Emeritus, University of Tokyo

Secretary-General: Kazuko Inoue, Professor, International Christian University, Tokyo

Number of Participants: 1448 (from 48 countries)
plus 50 accompanying persons

Number of Papers:

27 plenary papers (invited); 26 working groups (out of 102 proposals); 223 principal papers and 55 alternate papers out of 365 section papers were selected by the 15-member Reading Committee.

Registration Fees:

- | | |
|---|-------------------|
| (1) US \$ 60 or ¥ 15,000 (before 31 May 1981) | 443 persons |
| (2) US \$ 90 or ¥ 22,000 (from 1 June 1981 to 31 May 1982) | ... 637 persons |
| (3) US \$ 120 or ¥ 30,000 (after 1 June 1982) | 401 persons |
| Cancellers | 33 persons |
| (Half price for students for all periods; 3,000 yen for an accompanying person) | |

Congress Materials Prepared:

1. First Circular (2 pp.; October 1980)
2. Second Circular (19 pp.; October 1981)
3. Preprints of the Plenary Session Papers (311 pp.; March 1982)
4. Third Circular (70 pp.; 31 May 1982)
5. Congress Program (60 pp.; 29 August 1982)
6. *Abstracts of Section Papers and Working Groups (296 pp.; 29 August 1982)
7. List of Participants (60 pp.; 29 August 1982)
8. Proceedings of the XIIIth International Congress of Linguists (1514 pp.; published in December 1983)

* *Abstracts of Section Papers and Working Groups* are still available. Those interested should write to the ICL 1982 Office.

PREFACE

by Shirô Hattori

The present volume of the *Proceedings of the XIIIth International Congress of Linguists* (Tokyo 1982) contains 8 plenary summaries, 27 plenary papers, 194 section papers and 25 reports of working groups presented at the Congress, a list of participants, index of authors, and pictures at the Congress. The following materials are appended: excerpts from the circulars and congress programmes, and photoprints of the title pages of the Proceedings of the previous Congresses.

* *

The XIIIth International Congress of Linguists was held in Tokyo from the 29th of August through the 4th of September, with 1448 participants from 48 countries.

When I look back upon the events related to the Congress, I realise that it originated from the Ninth Congress held in Cambridge, U.S.A. in 1962. The President was Professor Einar I. Haugen of Harvard University, who invited me to chair a section meeting on structural semantics, giving me a generous grant for transportation and accommodation. There I found how important and significant an international congress was.

At the Bucharest Congress held in 1967, I was elected a member of the Executive Committee of the CIPL, although I did not attend the Congress. I accepted the invitation, because I had been given some explanation about the CIPL by the late Professor Roman Jakobson. However, I did not fully realise the importance of the position.

Several months before the Bologna Congress in 1972, I was requested by Professor Einar I. Haugen, a permanent member of the Executive Committee of the CIPL, to sound out about the possibility of organising the next Congress in Japan. I made some investigation hastily, without full conviction of success.

During the week when the Bologna Congress was being held, I received some suggestion but no formal request to hold the next Congress in Japan.

After having returned home, however, I made all possible endeavour to hold it in our country, until the deadline, i.e. the end of June 1974. After all, however, I was obliged to answer "no" to the Secretary-General, due to the terrible inflation caused by the "Oil Shock", and for some other reasons, of which I would not like to go into details.

At the Vienna Congress held in 1977, no decision was made, as usual, concerning the place for the next Congress. When I met Professor Wolfgang U. Dressler, the President, in one corner of the hallway, he whispered to me, "This Congress should have been organised by you." However, I did not take it seri-

ously, because it was only an informal suggestion. I felt rather freed from the burden of organising the Congress in Japan, because no formal proposal was made at the meetings of the Executive Committee.

Several months after coming back home, I received a letter from Professor Eugenius M. Uhlenbeck, the Secretary-General of the CIPL, in which he requested me to do some sounding concerning the possibility of organising the next Congress in our country.

Hereupon at last, I finally realised the grave importance of the expectations directed towards me, and made up my mind to take all the responsibility for the Congress. I started the activity secretly and very carefully, because of the bitter experience of the last failure.

I will not describe the various events after my decision, except for important ones. I only mention that they were an alternating succession of good luck and ill luck, just like the usual way of life.

Inasmuch as I was planning to organise an ad hoc committee as had been suggested to me formerly, I was greatly embarrassed when we found in October 1979 that, in terms of the Japanese laws, only permanent academic societies are entitled to organise international congresses. In our case, the Linguistic Society of Japan was the pertinent one.

After this event, I still had to overcome various difficulties, and it sometimes seemed almost hopeless. However, it was a great luck for us that the late Professor Shigeo Kawamoto, who deceased on August 1, 1983, was elected President of the Linguistic Society in 1981, and took over the position for two years beginning from April 1981, according to the statute of the Society. With his strong support and cooperation, a very powerful Organising Committee for the Congress was established immediately. Thanks to the endeavours of several members, a "Supporters' Association" was also established at the end of October 1981, in order to financially aid the Congress, because our financial outlook still seemed very gloomy. However, members of the Committee had started the activity in their own given capacities, and as for the business of the organisation everything began to get along smoothly. I was so lucky that I only had to play the rôle of the coordinator of the sincere and devoted team, who worked with a beautiful division of labour. I am deeply grateful to all the Committee members, together with their wives and husbands, and also to the students and others who helped us very faithfully under their guidance. Meanwhile, our financial state kept on improving, and in the middle of August, just before the Congress, it miraculously attained the best condition.

During the last three difficult years, I myself was constantly led by a firm conviction that to hold the Congress in Japan means to aid the realisation of the ideal of the CIPL, one of the thirteen suborganisations of the CIPSh, whose ideal is in close relation to that of the UNESCO.

On behalf of all those who participated in the organisation of the Congress, I express my most cordial gratitude to

Their Imperial Highnesses the Crown Prince and Princess,
to the Linguistic Society of Japan,
to the Ministry of Education, Science and Culture,
to the Science Council of Japan,
to the six academic Societies of Japan, that cooperated with us,
and to all the members of the Supporters' Association.

Before concluding this preface, it is also my great pleasure to express here our heartfelt appreciation to the following organisations, who financially supported us:

The Commemorative Association for the Japan World Exposition 1970 (Osaka),

The Hōsō Bunka Foundation (Tokyo),

The Kajima Foundation (Tokyo), and

The Comité International Permanent des Linguistes (The Hague).

September 1983

LIST OF PREVIOUS PROCEEDINGS (1930-1978)

The titles of the Proceedings of the Ist to the VIth Congress are printed from pp. iii-iv of the *Proceedings of the Seventh International Congress of Linguists* (London 1956). See also pp. 1431-1448 of this volume.

- I. *Actes du Premier Congrès international de Linguistes, à La Haye du 10-15 Avril 1928*. A. W. Sijthoff's Uitgeversmaatschappij. N.V.—Leiden [1930]. viii + 198 pp.
- II. *Actes du Deuxième Congrès international de Linguistes, Genève 25-29 Août 1931*. Librairie d'Amérique et d'Orient. Adrien-Maisonneuve, 5 rue de Tournon, Paris (6^e), 1933. 254 pp.
- III. *Atti del III Congresso internazionale dei Linguisti, (Roma, 19-26 Settembre, 1933)*. A cura di Bruno Migliorini e Vittore Pisani. Firenze, Felice Le Monnier, 1935. xv + 449 pp. [The following French title also appears on p. ii: *Actes du IIIème Congrès international de Linguistes.... Rédigés par Bruno Migliorini et Vittore Pisani.*]
- IV. *Actes du Quatrième Congrès international de Linguistes, tenu à Copenhague du 27 Août au 1^{er} Septembre 1936*. Einar Munksgaard, Copenhague, 1938. 305 pp.
- V. Owing to the deterioration in the international situation the Vth Congress was not held. The following preliminary material appeared:
V^{me} Congrès International des Linguistes, 28 Août-2 Septembre 1939.
 1. *Première publication Réponses au Questionnaire*. Imprimerie Sainte Catherine, Tempelhof 51, Bruges [1939] 104 pp.
 2. *Première publication Supplément Réponses au Questionnaire (Suite)*. [1939] 53 pp.
 3. *Deuxième publication Rapports*. [1939] 147 pp.
 4. *Résumés des Communications*. [1939] 66 pp.

[The following document was prepared by a Committee appointed at the IVth Congress, and it was to be submitted at the Vth Congress: *Dokumente zur Interpunktion europäischer Sprachen*. Elanders Boktryckeri Aktiebolag, Göteborg, 1939. xlii + 57 pp.
 It is not a document sponsored by a Congress though it should be included for the sake of bibliographical completeness.]
- VI. *Actes du Sixième Congrès international des Linguistes, publiés sous le patronage du C.I.P.L. avec le concours de l'U.N.E.S.C.O. par les soins de M. Michel Lejeune*. Paris, Librairie C. Klincksieck, II, rue de Lille (VII^e), 1949. lxxi + 608 pp. [The following English title also appears on p. ix: *Proceedings of the Sixth International Congress of Linguists*, published under the auspices of C.I.P.L. with the assistance of U.N.E.S.C.O. Edited by Professor Michel Lejeune.]

- VII. *Proceedings of the Seventh International Congress of Linguists, London, 1-6 September 1952*. Published under the auspices of C.I.P.L., with the assistance of U.N.E.S.C.O. General Editor: F. Norman, Assistant Editor: P. F. Ganz, London 1956. Distributed by International University Booksellers Ltd. London. lxxii + 575 pp. [The following French title also appears on p. i: *Actes du septième Congrès international des linguistes*]
- VIII. *Actes du huitième Congrès International des Linguistes*. Presses Universitaires d'Oslo. General Editor: Eva Sivertsen, Editorial Committee: Carl Hj. Borgstrøm, Arne Gallis, Alf Sommerfelt. Oslo 1958. xxxi + 885 pp. [The following English title also appears on pp. i and iii: *Proceedings of the Eighth International Congress of Linguists*]
- IX. *Proceedings of the Ninth International Congress of Linguists, Cambridge, Mass., August 27-31, 1962*. Edited by Horace G. Lunt. Mouton & Co. London, The Hague, Paris 1964. xxii + 1174 pp.
- X. *Actes du X^e Congrès International des Linguistes, Bucarest, 28 août-2 Septembre 1967*. Editions de l'Académie de la République Socialiste de Roumanie, Bucarest 1969-1970. Rédacteur en chef: A. Graur. Tome I (1969): xcvi + 773 pp. Tome II (1970): 1158 pp. Tome III (1970): 779 pp. Tome IV (1970): 1184 pp.
- XI. *Proceedings of the Eleventh International Congress of Linguists, Bologna-Florence, Aug. 28-Sept. 2, 1972*. Edited by Luigi Heilmann. Società editrice il Mulino, Bologna 1974. Vol. I: xiv + 966 pp. Vol. II: xiii + 1213 pp.
- XII. *Proceedings of the Twelfth International Congress of Linguists, Vienna, August 28-September 2, 1977*. Editors: Wolfgang U. Dressler, Wolfgang Meid, Assistant Editors: Oskar E. Pfeiffer, Thomas Herok. Innsbruck 1978. xv + 832 pp. [The volume was published as a Sonderband der *Innsbrucker Beiträge zur Sprachwissenschaft*, herausgegeben von Wolfgang Meid. The following German and French titles also appear on p. ii: *Akten des XII. Internationalen Linguistenkongresses, Wien, 28. August-2. September 1977*; *Actes du XII^e Congrès International des Linguistes, Vienne, 28 août-2 septembre 1977*.]*)

*) Still available from: Institut für Sprachwissenschaft der Universität Innsbruck, A-6020 Innsbruck, Innrain 30, Austria.
Price: 2160 Austrian Schillings per copy.

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OPENING SESSION

CLOSING SESSION

OPENING CEREMONY

Greetings by *Shirô Hattori*, President of the Congress

本日、ここに皇太子殿下ならびに同妃殿下の御臨席を仰ぎ、第十三回国際言語学会議の開会を宣することが出来るようになりましたことは、私の無上の光榮とする所であります。

Your Imperial Highnesses the Crown Prince and Princess.

Ladies and gentlemen! Dear friends and colleagues!

I esteem it a great honour and pleasure to declare now the opening of the Thirteenth International Congress of Linguists, on behalf of all those, who have cooperated with us, encouraged us, supported us, and assisted us.

This Congress was organised under the auspices of the CIPL, i.e. le Comité International Permanent des Linguistes (the Permanent International Committee of Linguists), promoted by the Linguistic Society of Japan, assisted by the Ministry of Education, Science and Culture, and by the Science Council of Japan, in cooperation with the six academic Societies of Japan.

The aforementioned CIPL is a non-governmental international organisation founded to assist in the development of linguistic science. It is one of the thirteen international academic organisation unions, federations, committees, etc., which are affiliated to the CIPSh, i.e. le Conseil International de la Philosophie et des Sciences Humaines (the International Council for Philosophy and Humanistic Studies). The ideal of this CIPSh is, to my understanding, in close relation to that of the UNESCO.

The First International Congress of Linguists was held in 1928 in The Hague, the Netherlands. This Congress is the Thirteenth, but, as a matter of fact, the Twelfth, because the Fifth Congress was never held due to the World War II. Out of the eleven Congresses in the past, only one, the Ninth was organised in 1962 in Cambridge, Mass., the United States of America. All the other ten Congresses were held in Europe. Therefore, this is the first Congress in Asia. We have endeavoured to comply with the ideal of the CIPL, and the CIPSh.

The distinctive feature of this Congress, compared with those of the previous Congresses is the fact that it has eight Plenary Sessions on the general theme of "linguistics in the 1980s." Besides, Section Meetings and Working Groups will abundantly be held.

I cordially hope for the success of this Congress, while expressing my heartfelt gratitude to all those, who devotedly contributed to the organisation of this Congress, advised us, supported us, encouraged us, cooperated with us, and assisted us.

Thank you.

Greetings by *Shigeo Kawamoto*, President of the Linguistic Society of Japan

Your Imperial Highnesses the Crown Prince and the Crown Princess, Ladies and Gentlemen.

It is a great pleasure as well as a privileged honor for me to pronounce in the name of the Linguistic Society of Japan a few words of welcome, good wishes and thanks on the occasion of the opening of the XIII International Congress of Linguists today here in Tokyo.

First and foremost, let me extend my cordial welcome to all those who were kind enough to get together for the Congress, the first to be held in Asia, those who traveled from afar and those who represent the various parts of this country. I hope the visitors from other countries will take advantage of their being in Japan to witness for themselves various facets and phases of this country and will return home with a renewed conviction that, in spite of differences in cultural traditions, human beings, *homines loquentes*, are of one and the same family of man and can precisely because of such differences, be enriching to one another.

Secondly, good wishes. Since we are assembled here for academic purposes, and not just for a euphoric exchange of amiable words, we might at the outset agree to disagree. Thus, frank opinions and views will, I hope, freely circulate and, in that way, contribute to leading the Congress to a successful and fruitful conclusion.

Last but not least important, I would like to thank the officers of the Congress who have contributed with self-abnegation to the implementation of the Congress. I thank Professor Shirô Hattori for assuming all responsibilities of planning and realizing the Congress. I thank also Professor Kazuko Inoue, the Secretary-General of the Congress and the members of the Secretariat under her leadership. Thanks are due to those individuals and corporate bodies who came generously to support the Congress. My gratitude goes further to the Comité International Permanent des Linguistes, to its President Professor Robins and its Secretary-General Professor Uhlenbeck and also to every one of the congressists.

So I end my greetings by saying *Good luck* to the Congress and its participants. Thank you.

Greetings by *Robert H. Robins*, President of the Comité International Permanent des Linguistes

Kôtaishi Denka narabini Kôtaishi Hidenka, minasama. Dai Jûsankai Kokusai Gengogakusha Kaigi wa kotoshi Tôkyô de okonawarete orimasu. Kono Kaigi o Tôyô de okonau no wa, kondo hajimete de gozaimasu. Watakushi wa minasama no daihyôsha toshite, Nihonjin no yûjin kara omaneki itadakimashita koto o kokoro kara kansha itashimasu. Arigatô gozaimashita.

皇太子殿下並びに皇太子妃殿下、皆様。第13回国際言語学者会議は、今年、東京で行なわれております。この会議を東洋で行なうのは、今度初めてでございます。私は皆様の代表者として、日本人の友人からお招きいただきましたことを心から感謝いたします。ありがとうございます。

Your Imperial Highnesses, ladies and gentlemen. This is the first International Congress of Linguists to meet in the East. And the contribution made by Japanese scholars in all branches of linguistics makes Tokyo the proper place where such a first meeting should be held. The importance of this congress is emphasized by the gracious presence with us of the Crown Prince and the Crown Princess at the reception yesterday, and at this ceremony today. All of us wish to express our thanks to our Japanese hosts for their invitation, and for all the care that Professor Hattori and the other members of the organizing committee have taken for the success of this congress. We, all of us, must now ensure that our work at the congress justifies all the work that has already been done on our behalf. Thank you.

(English translation)
It is an honour to be at the opening ceremony of the XIIIth International Congress of Linguists and to meet participants gathered here from all over the world.
Language is one of the property traits characteristic of human beings. Study of man's language since the past has been mentioned in the modern world by course of language. It is not too much to say that we owe the continuous development of man's culture to language.
The study is interested by various people, engaged with various languages.

The Address of His Imperial Highness the Crown Prince of Japan

皇太子殿下お言葉

第13回国際言語学者会議が、世界各地から参加された皆さんを迎え、ここに開かれることを誠に喜ばしく思います。

言語は、人間の人間たるゆえんを示すものであり、人類の過去の遺産は、多くこの言語を通して現代に伝えられております。あるいは、人類文化の継続的發展は、言語に負うものといっても過言ではないでしょう。

世界には、人間活動の所産としての文化を受け継いだ様々な人々が住み、様々な言語を話しています。世界の人々が互いに精神的安らぎを持つ社会を築いていくためには、相互の理解とともに、それぞれの人々が背負ってきた文化への、相互的理解と尊重がなければなりません。そのために言語の研究が果たす役割は、極めて大きなものがあると思います。私はかつて、日本の南の島沖縄県の人々が大切にしているものを少しでも理解したいと考え、琉球中山王国時代の琉歌の幾つかを読んだことがあります。その時非常に助けとなったのは、この度の組織委員会会長服部四郎博士が編集の指導をされた沖縄語辞典でありました。日本語の一分枝である沖縄語の文法に関する説明も含むこの辞典により、琉歌への理解を深めることのできた喜びを、今もつて記憶しています。

しかしまた、言語の研究の意義は、決してこのようなものに限りません。この度の会議では、言語の本質と機能に関する理論的研究から、言語障害の問題や、コンピューター使用による言語情報処理の問題を扱う研究まで、様々なテーマについて、発表や討議がなされると聞いておりますが、参加した皆さんにとってそれが興味深く、意義あるものとなることを願っております。

終わりに、今回の国際会議が、参加者の学術的見解や情報の交換を行う場となるにとどまらず、参加者の間に友情が深まる場ともなり、ひいては今後の言語学的发展に資するものとなることを祈り、開会式に寄せる言葉といたします。

(English translation)

It is my pleasure to be at the opening ceremony of the XIIIth International Congress of Linguists and to meet participants gathered here from all over the world.

Language reflects the property truly characteristic of human beings. Most of Man's heritage from the past has been transmitted to the modern world by means of language. It is not too much to say that we owe the continuous development of Man's culture to language.

The earth is inhabited by various peoples, endowed with various languages,

and each inheriting their own culture, the products of human activity. To build a society in which all the peoples of the world live together, sharing spiritual comfort with one another, we have not only to understand one another but also to mutually understand and respect the cultures borne by one another. For the attainment of this end, the study of language will play an extremely great role. In order to understand as much as possible what the people of Okinawa Prefecture, the southern islands of Japan, dearly cherish, I read some time ago several pieces of Ryuka (Ryukuan poems) composed in the era of the Ryukyu Chuzan Kingdom. At that time I relied heavily on *A Dictionary of the Ryukuan Language* compiled under the guidance of Dr. Shirô Hattori, president of the Organizing Committee of this congress. I still remember the pleasure I felt when I gained some understanding of Ryuka with the help of this dictionary, containing even grammatical explanations, of the Ryukuan language, a branch of Japanese.

The study of language, however, has a wider significance. I hear that the lectures, papers, and discussions prepared for this congress cover various topics, ranging from theoretical study of the nature and function of language to problems of language disorder and processing of linguistic information by means of computers. I hope all these academic activities will be both interesting and meaningful to the participants.

I would like to conclude by expressing my very best wishes for the success of this international congress not only as an occasion for the exchange of academic views and information but as a place for deepening friendships among the participants, while contributing to the development of linguistics for the future.

Congratulatory Message by Heiji Ogawa, Minister of Education, Science and Culture

本日ここに皇太子殿下、ならびに同妃殿下の御臨席をあおぎ、第13回国際言語学会議の開会式が挙行されるにあたり、御出席の皆様にお祝いの言葉を申し述べる機会を得ましたことは、私の最も喜びとするところであります。私はまずこの会議のために遠路はるばる来日されました世界40か国の教育・研究者のかたがたに心から歓迎の意を表したいと思ひます。国際言語学会議は、1928年にオランダのハーグで第1回会議が開催されて以来、半世紀以上の歴史と伝統をもつ会議であります。このたびその歴史上はじめて欧米以外の地域で会議が開催されますことは、まことに意義深いものと考えます。これを契機として言語学の分野における研究が全地球的な広がりをもつてますます発展するとともに學術の国際交流の推進にも大きく貢献することを願うものであります。この会議においては、世界各地で意欲的に研究にとりくんでおられるかたがたにより言語の本質と機能に関する問題をはじめ、言語の時間的・空間的变化に関する問題、言語の学習や運用に関する問題などについて、すぐれた成果が発表され、将来への研究の展望を含めて活発な討論が展開されるものと思ひます。そして、このような発表・討論を通じて人間をもっとも人間らしくさせている特性の一つである言語の構造等にさらに多くの光が与えられ、ひいては、人間の英知が深まる結果となりますよう心から期待をいたしております。また各地からの参加者各位におかれましては、これを機会に日本語のみならず、それを生みだした風土、社会、文化等の人間的背景についても理解を深めていただき、日本での滞在をみのり多いものとされるよう心から願うものであります。最後に、本国際会議の開催にあたり御尽力いただいた関係各位に深甚の敬意を表するとともにこの会議が所期の目的を成功裡に達成することを希念して御挨拶といたします。

昭和57年 8 月30日

文部大臣 小 川 平 治

(English translation)

It is a great pleasure to me to have this occasion to offer a message to the participants of this important gathering, congratulating you on the opening of the XIIIth International Congress of Linguists, in the honored attendance of their Imperial Highnesses the Crown Prince and Princess.

First of all I would like to express a hearty welcome to you scholars who have come from distances all over the world representing more than forty countries. As the first International Congress of Linguists was held in the Hague in 1928, you have a history and a heritage of more than half a century.

It is significant indeed that this time the Congress is meeting outside of Europe and America for the first time in its history. I hope that with this Congress contributing to international academic exchange, it will set a new momentum, so that researches in linguistics will make further progress.

In your Congress I anticipate that excellent results will be presented of researches by your scholars working in various places on the earth, researches concerning problems of the property and the function of language, problems of temporal and spacial variation of language, and problems of language learning and its use. There will also be lively discussions on these topics, including the future prospects of researches. I look forward to seeing new light shed on the structure and function of language, truly a definitive characteristic of Man, and to finding Man's wisdom deepened as a result.

I sincerely hope that you participants of this Congress from abroad will find opportunities to make your stay in Japan fruitful not only in terms of linguistics but also for witnessing something of our country, the people, their surroundings, society, and culture. In conclusion I would like to express my profound appreciation for what everyone concerned has done in making this international congress possible. My very best wishes for the successful attainment of your aims.

Congratulatory Message by Koji Fushimi, President of the Science Council of Japan

第13回国際言語学者会議開会式祝辞

本日、ここに皇太子殿下ならびに同妃殿下の御臨席を仰ぎ、第13回国際言語学者会議の開会式に際しまして、同会議を後援する日本学術会議を代表して、お祝いの言葉を申し述べ、機会を得ましたことは、私の光栄とするところであります。

我が国における言語の研究は、江戸時代の国学者たちによる上代語の仮名遣いや、文法に関する研究という形で、外国の学問的影響を受けることなく独自の発達を遂げたと言われておりますが、明治時代以降は、西洋の言語学や音声学の影響を受けて日本人本来の言語研究の能力が次第に開花し、研究がますます隆盛になってきたと伺っております。こうした日本における言語研究の発展とともに、アジア、中近東、アフリカ、中南米などにおいても、古い伝統と新しい科学として言語学が次第に融合に向いつつあると聞いております。このような時にあたって、第13回国際言語学者会議が初めて欧米以外の地である、我が国で開催される運びとなりましたその意義は極めて大きいものと存じます。これまでの関係者各位の熱意と御努力に深く敬意を表する次第であります。

今回の会議には40数ヶ国、約1,400名にのぼる言語学者達が一堂に会し、それぞれの専門的立場から「1980年代の言語学」をテーマとして研究討議が行われるとのことですが、言語は、人間生活および人文・社会・自然の諸科学にとって極めて重要な意義と機能を有しておりますから、私は自然科学の分野に携わる者であります。今回の会議の成果に深い関心を抱いております。

第13回国際言語学者会議が、言語学そのものの進歩発達を促がすとともに、参加された言語学者各位の相互理解を深め、ひいては国際的、学際的、コミュニケーションの改善を通じて、世界平和確立に貢献する有意義な役割を果たされるよう期待してやみません。

また、海外よりはるばる来日された研究者の皆様には研究に関してはもとより、日本の風土や文化についても、より多く知る機会を持たれ、日本に対する理解を深めていただくよう希望いたします。私の祝辞とさせていただきます。

昭和57年8月30日

日本学術会議会長 伏見 康治

(English translation)

It is my great privilege as president of the Science Council of Japan to deliver these words of felicitation to the XIIIth International Congress of Linguists at its opening ceremony, honored by the attendance of their Imperial

Highnesses the Crown Prince and Princess.

According to my understanding, up to the beginning of the Meiji era, the study of language in Japan was promoted without foreign influence by Japanese philologists carrying out researches on the grammar and the uses of Kana in Ancient Japanese. Since the Meiji era, however, the influence of linguistics and phonetics developed in the West has brought out the inherent capacity of the Japanese people to do research in linguistics, and triggered the ever growing research activities in this field.

The development of linguistic researches in Japan is said to have been in parallel with a new trend in Linguistics in Asian, Near Eastern, African, Central and South American countries, that is, in the direction of integration of philological traditions and modern linguistics. It seems to me that the significance of this international congress held at this time is very great, for it is the first time after twelve congresses for the site to be in other than the Western countries. I would like to express my sincere respect to all of you who by your efforts and enthusiasm have made this Congress possible.

I am told that there are some 1400 persons from more than 40 countries gathered here, and you are to discuss "Linguistics in the 80s" from various points of view according to your different specializations. Language has great significance, playing an important role not only in everyday life but also in researches of human, social and natural sciences. This is why a person, such as myself, in a field of natural science, feels deep interest in the outcome of this Congress.

It is my sincere hope that the XIIIth International Congress of Linguists, while stimulating the progress and development of linguistics itself and helping to deepen the mutual understanding among the participants, will play an important role in the establishment of world peace by contributing to the improvement of international and interdisciplinary communication. I hope that all you scholars who have come long ways from abroad will have ample opportunities to deepen your understanding of your own specializations. Finally, I hope also that you will gain some insight into Japan, our people and our culture.

Closing Address by Shirō Hattori

Ladies and gentlemen!

As the President of the Thirteenth International Congress of Linguists, I would like to express my cordial gratitude to all of you for your kind co-operation in making this Congress successful in every respect.

I hope that those of you who have come from long distances have had ample opportunities to satisfy your academic curiosity and to know this part of the world a little better.

On my part, I have heard very explicit expressions of appreciation from some of you, and am very grateful for that. I was, in fact, very fortunate to have been assisted by a team of devoted, enthusiastic and very capable linguists for the organisation of this Congress. I would like to ask you, all the members of our Organising Committee, to stand up, please. (Applause) Thank you.

Now, I would like to be permitted to ask some of you to come up on the stage, when I call your name. Incidentally, . . . I am sorry I have to restrict the number to "seven", because the stage is not so spacious . . . and "seven" is my favourite number. I don't know why. . . Maybe . . . I learned some Mongolian, and the Mongols like the number "seven", and the influence . . . might have come from that experience.

I call the seven names, but it does not mean at all that the others who will not be called don't deserve coming up. Now, I call . . . , but the order is at random:

Mrs. Kazuko INOUE, the Secretary-General

Mr. Tsuyoshi NARA, the chairman of the subcommittee for public relations, and the vice-chairman of the subcommittee for fund raising

Mr. Tadao SHIMOMIYA, the Associate Secretary-General

Mr. Yoshio NAGASHIMA, the Associate Secretary-General

Mr. Hajime KITAMURA, the chairman of the subcommittee for financial affairs

Mr. Takao OOE, who faithfully assisted Mr. Kitamura

Mr. Nobuhisa TSUJI, the vice-chairman of the subcommittee for public relations

May I ask you to applaud for them? (Loud applause.)

Address at Closing Ceremony by *R. H. Robins*

Minasama. Ladies and gentlemen.

The chairmen of the Plenary Sessions have given us their reports, and there is little left for me to say at the conclusion of this Congress.

We retain many pleasant impressions and many very happy recollections. As I said in my opening address, this Congress has been especially important since it is the first International Congress of Linguists to meet in Asia, and we must first of all express our warmest thanks to our Japanese hosts, and by our hosts I mean not only those immediately responsible for the Congress itself, but those many citizens of Tokyo who in different ways have shown us courtesies and kindnesses as we made our way around this delightful city.

A special acknowledgment must be made of the wonderful reception that this Congress has been given. To be accorded one visit by their Imperial Highnesses might, perhaps, be taken as a gracious recognition. To have had the pleasure of their presence on two successive occasions must be regarded as the highest honour and one that we must hope has been justified by our work in this past week. We must also express our thanks to all those who have contributed financially to the running of the Congress, whether as private individuals or as national and local institutions.

Professor Shirô Hattori, Professor Kazuko Inoue, and all the other members of the Organizing Committee have laboured long and hard for the past two years for our benefit. We have seen the results of these labours and we thank them all most sincerely.

We came here specifically to review the state of linguistics in the 1980s. My own dominant impression, derived from our meetings here, has been the healthy diversity and vigour of our subject. No one theory or methodology has been allowed to dominate our sessions. It has been expressly admitted that in the present state of our subject several different theories are best placed to throw light on different areas of study within both descriptive and historical linguistics. The insights of transformational-generative grammar are being properly exploited in several branches of language study, but it is clear from this Congress that the days have now gone when this one theoretical viewpoint claimed prominence over everything else; and the excessive concentration on abstract syntax, often too much confined to English, and sometimes disfigured by political slogans and propaganda, has passed away.

A speaker referred to the renewed attention being given to morphology as a distinct area of grammatical analysis. This is most welcome. The post-Bloomfieldian structuralists, though not Bloomfield himself, tended to treat syntax as an extension of morphology, the distribution of morphemes relative to each other. And, almost as a countermovement to this, transformational-generativists

extended the term *syntax* to include word structure itself, thereby blurring the vital distinction between inter-word relations and intra-word relations. Both phases of descriptive grammar in their different ways drew attention away from the unique and vital status of the word as both a formal and a semantic unit of language structure, whether in written or in unwritten languages.

It is very hard for most of us to keep up with all the developments that are taking place within linguistics today. Perhaps some few of the greatest scholars in our field have in their own times come within some distance of this achievement: Sapir, Bloomfield, and the preeminent linguist of our generation, Roman Jakobson, whose recent death has so saddened us all. If such a panoptic control of the whole range of linguistic studies is less and less attainable, this is due not just to our own inadequacies and failings but to the ever growing scope of the topics deemed properly to fall within linguistics. And most of these topics have been recognized and represented somewhere at this Congress.

Ars longa, vita brevis: we must each develop the theoretical line that most appeals to us and in the field in which we think we can contribute most. But we must never fail to read, to listen to, and to learn from the work of others, including, and perhaps especially, those with whose viewpoint and approach we may feel less sympathy. This is the value of large-scale congresses like this one. We have had the opportunity to listen to reports on the present state and the future prospects of whole areas of linguistic research, to hear brief accounts of individual research activities, and to take part in working groups investigating particular areas and topics. But there are limits to all this. Even if some of those present here could, like Chomsky's idealized speaker-listener, make themselves immune to inattention, hunger and thirst, and weariness, not to mention the attractions of Tokyo city by day and night, even such paragons of scholarly virtue could not (as yet) be in more than one place at one time; and all of us will have to wait for the published *Proceedings* to get an entire picture of what has been done here this summer.

This, however, is to undervalue one very important part of our quinquennial gatherings. It is good just to meet each other informally at lunch or at tea, in the lounges of the Nippon Toshu Centre and in our hotels. Once one has seen and talked with a colleague from another part of the world one hears his voice through one's reading of his written work, and one's reading thereby becomes more vivid and instructive. It is, too, surprising how many colleagues and friends we meet at congresses like this whom we might not have seen otherwise. Formal papers and symposia are excellent and indispensable. Equally valuable are our informal meetings, greetings, and conversations.

These quinquennial Congresses are the major concern of CIPL, the *Comité International Permanent des Linguistes*, and along with the *Linguistic Bibliography* they are the principal benefit conferred by CIPL on the community of scholars working in linguistics. The Congresses provide the sites for the principal meetings of the Executive Committee and the General Assembly. CIPL repre-

sents linguistic scholarship in international bodies and before international authorities. In Tokyo we have taken an important step to strengthen the position of CIPL by extending the scope of its potential membership to include international linguistic societies as well as individual countries; and it is pleasant at this point to record the application by Greece to become a member country, an application accepted with alacrity by the Executive Committee at its meeting last week.

And now our Congress is nearly over. For many of us from outside Japan it has been our first occasion to visit these islands, and, much as we have enjoyed Tokyo and its surroundings, most of us are looking forward keenly to seeing a little of Japan outside Tokyo. All too soon we must leave, but we all hope to come back one day, and therefore the final words from us visitors should, perhaps, not be *sayōnara*, but rather *mata o-ai shimashō*.

Tokyo

September 4th, 1982.

Address at Closing Ceremony by Kazuko Inoue

Secretary-General of the Congress

The first item in this final report from the secretariat is the unexpectedly large number of participants in this congress. Fourteen hundred and forty-eight people paid their fees and turned in final registration cards, and thirteen hundred and thirty-eight people actually participated in this congress. This means that one hundred and ten people did not come. Among them fifty-five —exactly half— were those from abroad. Concerning those who did come, we have been impressed with great efforts of some people to make arrangements for the trip to Tokyo under very difficult conditions. We are very sorry for the absentees, and we will send all the materials used at the congress for those people as soon as possible.

At the outset of our planning, we anticipated about eight hundred participants. Therefore, we were very much concerned about the size of this conference hall when the number of applicants went beyond twelve hundred. Even though we tried our best to overcome this shortcoming in many ways, you were probably inconvenienced. We would like to offer our sincere apologies for these inconveniences, including the poor acoustics in some rooms, insufficient equipment such as overhead projectors, slides projectors, and so on.

We thank you very much for your cooperation. The atmosphere of the whole congress was very congenial and stimulating. Without your help in many capacities, the congress could not have fulfilled its academic responsibilities. Special thanks go to Professors Robins and Uhlenbeck, who represent the CIPL as its President and Secretary-General respectively, and those who chaired plenary and section meetings or organized the working groups.

I would like to add on this occasion that many Japanese people both in academic and business circles have given us financial and moral support for this congress.

Finally the secretariat would like to remind you that the deadline of your manuscripts for the *Proceedings* is 12:00 o'clock today. The subscriptions for the *Proceedings* are still accepted at the front desk. The congress office will move back to Gakushuin University this afternoon.

Thank you again for your most hearty cooperation.

PLENARY SESSIONS

Address at Closing Ceremony of **Kanbo**

Secretary-General of the **CIPL**

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Language, Function and Typology*)

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1. Historical perspective

Of the many arbitrary strictures that became the stock-in-trade of structuralism in linguistics from Saussure via Bloomfield and on to Chomsky and his latter day minions,¹⁾ strictures that have culminated in the convoluted flowering of so-called autonomous syntax, none have been quite as damaging to the integrity and good health of the discipline studying human language and its syntax as the exclusion of *communicative function* and *cross-language* typology. This is so because by eliminating from the study of syntax considerations of function, of surface structure typology and of the systematic correlations between the two, the structuralist orthodoxy thus ruled out, by an inexplicable *fiat*, a serious investigation of the most salient feature of human language as an instrument of coding and communicating information—the systematicity, non-arbitrariness or *iconicity* of the linguistic coding relation.

Early functionalist ripples in the polished generative surface, such as Katz and Postal (1964) or Gruber (1965, 1967), were rapidly absorbed into one of two forbidding formalisms—generative and interpretive semantics. Occasional forays into so-called 'deep typology', such as Ross (1970), Sanders and Thai (1972), Bach (1970) or McCawley (1970), or more recently Hale (1981), degenerated rather sadly into empirically bizarre postulations.²⁾ Throughout the long double era of structuralist famine in American linguistics, the typological heresy was kept alive by Greenberg (1966) and the functional heresy by Bolinger (1952, 1954).³⁾ The merging of these two separate lines of heterodoxy which began

*) This paper was read to the Plenary Session on Syntax and Semantics at XIIIth Congress of Linguists as a means for supplying the theoretical and methodological background for the pre-circulated "Typology and functional domains", whose published version may be found in Givón (1981).

1) *Viz* the exclusion of performance/cognition/neurology, ontogeny/phylogeny/diachrony, and society/culture/ontology, to mention notable others.

2) For example, McCawley (1970) characterized English, one of the most rigid SVO languages, as 'deeply VSO'; Bach (1970) tagged Amharic, an equally rigid SOV language, as 'underlyingly VSO'; and Hale (1981) has conferred upon Navajo, just as rigid an SOV language, the brand new typological status of *W-star* (totally free word-order).

3) The study of syntactic typology of course harkens back a hundred years or more to the Neo-Grammarians. Functionalism in Syntax, via Tesnière and various antecedents, is probably just as old. By citing here only Greenberg and Bolinger I wish to merely point out to the direct lines of transmission acknowledged by my own generation in linguistics.

to take place in the early 1970's, has at long last made it possible to refocus attention upon syntactic structure and its two main functional correlates—positional semantics and discourse pragmatics—in a more profound way.

2. Structure and function: The biological analogy

It is perhaps not altogether an accident that Chomsky has tended over the years to lift his analogies from the realm of physics, a discipline that has neither meaning nor functional correlates, nor a population with certain range of purposeful behavior, nor a range of behavioral and thus phenomenological variability. The study of so-called autonomous syntax may be likened more aptly within the realm of biology to an attempt to describe and understand skeletal support structures independently of their function(s) and of the variety of skeletal types attested across the animal kingdom. Such an endeavor, to the biologist, is so patently absurd and vacuous that the biological literature seldom if ever bothers to mention—let alone justify it.⁴⁾ Rather, biologists readily note the *function* of this system: The maintenance of a particular spatial orientation of the various bodily organs to insure the discharge of their individual as well as correlated functions, as well as—in more limited sub-realms—the anchoring of the musculature responsible for ambulation. Biologists would then observe the *typology* of skeletal support systems, beginning from the two major types, *exo*-skeleton for mollusks and insects and *endo*-skeleton for vertebrates and several coelenterates, and proceeding toward finer divisions to minor and minor-yet types. Finally, the biologist worth his/her salt would proceed to describe how the various skeletal types perform their common function(s), eventually adducing general iconic principles of biological design and perhaps ultimately also pondering upon functional, adaptive or evolutionary reasons for the seemingly stringent limits imposed upon the typological variety within each functional domain or constraining the form of entire organisms. In the study of language and, more narrowly, of syntax, there is reason to assume that we are engaged in a close analog to the work of the biologist as construed above. By marrying the functional and typological approaches, one may thus open the door to the investigation of how diverse structures may perform—or “solve”—similar functions, and why.

3. Functional realms coded by language

A falacy which has long been perpetrated by behaviorists in psychology, logical-positivist in philosophy and structuralists in linguistics is that somehow,

4) In his *Functional Human Anatomy* Crouch (1978) writes: “... *Anatomy* is the science that deals with the *structure* of the body. . . . *Systematic Anatomy* is the other approach to the study of gross human anatomy. With this method, the systems, such as skeletal, nervous, digestive, and other systems are studied in their entirety. . . . We wish to present anatomy as a *basis for physiology*. Physiology is defined as the science of *function*. Anatomy and Physiology have more meaning when studied together. . . .” [pp. 9–10]. The entire book is infused with the functional-ontogenetic-phylogenetic approach. I see no reason why the study of the communicative function of the species should not benefit from a similar integrative approach.

rather mysteriously, communicative function and communicative intent are less accessible to independent investigation by the scientist. In linguistics, the most rabid extremists⁵⁾ ruled out lexical semantics as, in principle, not part and parcel of the discipline. Later versions of the generative orthodoxy grudgingly allowed the investigation of both lexical and propositional semantics, via the 'independent', 'rigorous' tool of deductive logic. The meta-functional realm of discourse-pragmatics, with the exclusion of "speech acts" and other well-chosen chunks that were reduced and trivialized by various formal schools,⁶⁾ has remained largely *terra incognita* to the structuralist. This falacy emanates from a single source—the refusal to study an expanded data base of actual communication, the persistence in studying words and sentences in isolation from their communicative discourse context. When utterances are studied in such a context, there is nothing mysterious, mushy or intuitive about arriving at functional regularities, recurrences and cooccurrences which feed the progressively widening analysis. Certainly, there is nothing more mysterious about the methodology by which function becomes accessible than the one which makes structure accessible. The falacy is simply the by-product of arbitrary and indefensible limitations imposed upon the data base by structuralist.⁷⁾

If one for the moment disregards the socio-cultural or psycho-emotive functions of language, it is possible to recognize three major *functional realms* which receive systematic and distinct coding in human language:

(a) *Lexical semantics*: This functional realm pertains primarily to the storage of *generic knowledge* as manifest largely in the lexicon, whereby relatively stable phenomena, concepts or points of reference constitute an intricate network that is part and parcel of our cognitive map of the phenomenological universe. This functional realm is coded primarily by linearly-sequenced *sounds*, and although it is not altogether devoid of iconicity, nonetheless the degree of arbitrariness found in the relationship between sound code units and lexical meaning is perhaps the greatest among functional realms.⁸⁾

5) Such as, for example, Bloomfield (1933), also echoed in Chomsky (1957).

6) See for example Grice (1968/1975), Gordon and Lakoff (1971), Gazdar (1979) or Karttunen and Peters (1979), among many others.

7) In the same vein, the learned commentator on my "Typology and functional domains", a certain A. Marantz, has asserted in public, wearing an altogether straight face and the supreme confidence that only an equally supreme ignorance could engender, that describing or explaining function in the absence of an *independently* crafted "syntactic theory" (i.e., in his linguistic genre, *syntactic formalism*) is an impossible and indeed misguided venture. It never occurred to him that if two entities are *not* defined independently of each other, either correlating one to the other or explaining one through the other is in principle nonsensical, i.e. a *tautology*.

8) By observing that the degree of arbitrariness in the lexicon is the highest among coded functional realms, one does not necessarily mean that there is no iconicity relations worth studying in the coding of the human lexicon. In fact, there are grounds for believing that a general "iconicity imperative"—i.e. the fundamental assumption that in human communication there is a rough correlation between form and function that strives but never totally accedes to the idealized one-to-one—repeatedly manifests itself in speakers' search for lexical iconicity,

(b) *Propositional semantics*: This functional realm involves the *specific information* concerning (i) the characterization of a proposition as state, event or action, and (ii) the characterization of the arguments/participants/roles in terms of their semantic properties vis-a-vis the verb/predication. As a conflation of the two, propositional semantics also deals with the transitivity properties of the sentence (cf. Hopper and Thompson, 1980). Informally, the two semantic realms taken together impart the basic information about states, actions and events, i.e. "who did what to whom, when, where, by what means and under what circumstances". However in terms of coding, propositional semantics is jointly coded with discourse pragmatics (see (c) below) via *syntactic structure*. And syntactic structure is made of the complex interaction of three primary *coding devices*: Word-order, grammatical/inflectional morphology, and intonation.

(c) *Discourse pragmatics*: This functional realm involves the sequencing or placing of 'atomic' propositions within *communicative context* in discourse. The context for any utterance is of course a huge, open ended complex. But if one for the moments disregards the *generic* human-universal and culturally-shared context, then the bulk of communicative context *specific* to the discourse at hand may be divided into three main components:

- (i) *Speaker's goals*: Speech-act valuation and more specific pragmatic goals;
- (ii) *Interactive context*: Speaker's detailed knowledge of the hearer's goals, pre-dispositions or specific knowledge; and
- (iii) *Discourse context*: What information was processed in the preceding discourse, what one could take for granted as less-likely to be challenged,⁹⁾ foreground-background relations of the successive propositions in discourse.

As suggested above, this functional realm is *jointly coded*, together with propositional semantics, by syntactic structure. And from this fact arises what may be termed the *functional bind* of syntactic coding.

4. The functional bind of complex syntactic structures

The reader should have gathered by now that the division between propositional semantics and discourse pragmatics echoes broadly but nonetheless faithfully the generative-transformational division in syntax between kernel/simple and transformed/complex sentences. This division is of course implicit in traditional grammarian descriptive practices since time immemorial. Further, it is implicit in the early (pre 1970's) work on syntactic typology, which concerned itself almost exclusively¹⁰⁾ with the word-order, case-marking and voicing-cum-transitivity syntactic properties which code propositional semantics. To

in morphology, onomatopoeia, sound symbolism, rhyme or alliteration. For further discussion see Givón (forthcoming), Halman (forthcoming).

9) In formal terms, this is considered "presupposition". For justification of the challengeability approach, see Givón (1982).

10) An earlier typological preoccupation was, actually, the word-structure morphological typology which occupied the Neo-Grammarians (i.e. synthetic vs. agglutinative vs. isolating, etc.).

some extent, of course, this is an idealization of the facts, since not even the simplest, 'kernel', 'neutral' sentence may exist in isolation from *some* discourse context, *however* neutral itself. But early typological studies as well as the *entire* generative school glossed over this fact, perhaps initially with a certain measure of *methodological*—though hardly theoretical—justification.

The functional bind of syntax arises from its having to code jointly, with the same syntactic structure of a sentence, *both* the propositional semantic and discourse-pragmatic information associated with a proposition. As one knows well, this involves a *readjustment* (the transformationalist's "structural change") in the morphology, word-order or intonation that would have coded the proposition at its most neutral context, i.e. the structural clues coding the semantic case-roles and underlying transitivity of the sentence. These structural clues are thus tampered with, and this tampering most commonly requires *compensatory recoverability strategies*¹¹⁾ by which the information normally coded by the destroyed clues can nevertheless be recovered. Much of these strategies rely implicitly on redundant information available in the discourse context. But one may also view complex ('transformed') sentences as a *communicative compromise*, within which some structural clues pertain exclusively to the coding of propositional-semantic information, some exclusively to the coding of discourse-pragmatic information, and some are part of the readjustments via which the communicative compromise is transacted.

The way in which various languages create—and then solve—the functional bind of syntactic coding may be viewed as one of the main parameters of the syntactic typology of complex sentential structures. Thus, for example, in the English-type "promotional passive", where the topic/subject of the passive loses its original case-role coding and assumes the nominative marking typical of the active subject/agent, one may view the compensatory recoverability strategy as consisting of the following major elements:¹²⁾

- (a) Passivization is severely constrained, so that only a small range on non-agent arguments may be promoted to subject/topic of the passive, chiefly the direct-object or accusative; and then
- (b) If the verb is marked as an *active* verb, the subject/nominative of a transitive proposition is interpreted as the *agent*; but
- (c) If the verb is marked as an *adjectival/stative* verb (i.e. via the auxiliary 'be' and the perfect aspect), the subject/nominative is interpreted as the direct-object/accusative.

The very same communicative bind is solved in the "non-promotional" Ute passive by retaining the original semantic-role case morphology on the newly promoted topic of the passive, then coding its topic status either by word-order or by more subtle, context-dependent implicit means. Finally, the Philippine solution may be characterized as follows:

- (i) Mark the *topic* by a nominative morphology; but

11) The initial treatment of 'recoverability strategies' may be found in Givón (1979, Ch. 4).

12) See details in Givón (1979, Ch. 4 and 1981).

(ii) Code *on the verb* (by prefix) the semantic case-role of the topic NP. All three "types" of passivization may thus be viewed as different *typological solution* to the same functional bind arising from the fact that syntax must code jointly two functional realms.

5. How to define functional domains

Traditionally, one may find two paths for the discovery and definition of functional domains coded by syntax, each carrying its rather predictable strengths and weaknesses. First, one may discover domains via *structural* means, by relying on syntactic/structural similarities—intra-language or cross-linguistically—to clue us into what sentence types code the same—or similar—function. This method is implicit in the generative tradition, which, however, ignored an important corollary that makes the use of this method possible *at all*, namely the implicit assumption about *iconicity* in syntax:

"It is only because the coding relation between structure and function in syntax is *non-arbitrary*, i.e. iconic, that one could proceed to infer common function from common structure".

The pitfalls of the structural means for identifying functions are equally obvious, although generations of structuralists of whatever stripe have ignored them to their detriment. They arise primarily from the lack of broadly-based cross-linguistic sample, combined with the lack of independent function-based means for identifying functional domains. A prime example of these pit-falls is again the passive, which was defined by the generative school as: "such a structure which closely resembles the English passive in terms of

- (a) The promotion of a new topic to the subject case-role characteristic of the agent of the active;
- (b) The demotion of the active subject to an oblique case-role; and
- (c) The marking of the passive verb by an adjectival/stative/intransitive morphology."

This definition, *by fiat*, ruled out of the passive functional domain the passives in all languages—the majority of the world's sample—which delete the agent obligatorily. It also ruled out the passives of all languages, such as Ute, where the new topic is not promoted by explicit *morphological* means. Finally, it ruled out related structures such as various impersonals or left-dislocations where the verb is not morphologically de-transitivized. It, finally, also ignored the fact that even in a language such as English the impersonal ('agent suppression') functional domain is a strong component of the passive function, as attested by the predominantly *agentless* appearance of English passives in actual discourse.¹³⁾

The other extreme method of defining functional domains within the realm of discourse pragmatics is by studying *function* as rigorously, taxonomically and exhaustively as **one** studies structure, by studying in detail actual communicative interaction, i.e. *discourse*. The strength of this method is obvious. Its chief weakness is obvious as well: Functions are often related via *small incremental*

13) See text counts and discussion in Givón (1979, Ch. 2).

steps, *a la* Wittgenstein's *family resemblances*. So that by employing this method exclusively and in an extreme fashion, one may arrive at a totally mushy view of functions and their relative ranking.

The secret of a *sane* practice of linguistics—as distinct from absurd or extreme practices—is in combining the strengths of both methods, thus neutralizing their weaknesses. In this way, the discreteness of structure helps constrain the potential mushiness of function, while the substantiality of function helps constrain the potential vacuousness of structure. In elucidating a viable alternative to the structuralist dogmas of the past two generations in linguistics, the marriage of Greenberg's typological heresy and Bolinger's functionalism is indeed a fortunate union.

6. The complex nature of functional domains in syntax

Since syntax codes a mix of semantic and pragmatic functions, it is only natural if its various functional domains turn out to be rather complex. This complexity is manifest in two distinct senses:

- (a) Functional domains are often *scalar*, *graded continua* (rather than discrete).
- (b) Functional domains are often *multi-dimensional* (rather than uni-dimensional).

The study of the passive again affords an example to both senses of complexity. Its three main functional domains are:¹⁴⁾

- (i) Topic assignment and topic continuity
- (ii) Agent suppression ('impersonalization')
- (iii) Stativization of the verb

Of the three, (i) is primarily discourse-pragmatic, (ii) is a mix of semantics and pragmatics, and (iii) is primarily a propositional-semantic domain. Further, each may be shown to be scalar/graded, a fact that is underscored by the graded nature of their correlated *coding dimensions*. Thus, Keenan (1975) has noted that the syntactic promotion of non-agents to the subject-of-passive role is a matter of degree. The demotion and degree of *Chômeurism* of the agent of the active in passivization could be easily shown to be similarly graded. And similarly, the transitivity marking of the passive verb is a graded coding dimension (Givón, 1981).

Within one functional domain, further complexity may be found. Thus, for example, the domain of topic continuity has been shown to be a conflation of several thematic, pragmatic and semantic considerations (Givón, ed., 1983). In principle, the depth, complexity and internal structure of functional domains is a matter for empirical determination.

7. Iconicity principles in syntactic coding

The degree of non-arbitrariness or iconicity in syntactic coding of semantic and pragmatic function is also a matter for rigorous and often complex empirical

¹⁴⁾ One ought to leave the possibility open that other, perhaps more minor and predictable, functions may also be associated with the passive. For further discussion see Givón (1981).

examination. Again borrowing the example of the passive (Givón, 1981), one could demonstrate that three major syntactic elements in passive structures closely mirror the three functional domains of the passive. Each of the three performs its coding function by taking into consideration the *background expectations* engendered in the coding of the active.

(a) *Topic coding*: The coding of the agent-subject of the active establishes a background norm for the nominative/topic. As Keenan (1975) has pointed out, one may then assess the coding of the topic-of-passive as to the degree to which it approximates that norm.

(b) *Agent suppression coding*: Iconically, the agent is expressed in the most exposed, foregrounded position when coded as the subject of the active. On this background, one may view demotion to oblique and the loss of characteristic coding properties of the nominative as partial suppression ('impersonalization'), and the obligatory deletion of the agent as total suppression.

(c) *Transitivity coding of the verb*: On the background of the two iconic extremes, adjectives or nouns on one hand and active verbs on the other,¹⁵⁾ one could assess the coding of the passive verb as to its proximity to either extreme.

Iconicity principles of this type are specific to each functional domain. After investigating in detail a great number of functional domains, one can proceed to formulate—and eventually explain—more general coding principles, perhaps meta-iconicity principles. For example, in the coding of topic continuity (Givón, ed., 1983), one finds the following quantitative gradation among syntactic devices:

(1) *most continuous topic* ('most predictable')

zero anaphora

clitic/unstressed pronouns

stressed/independent pronouns

✓ full definite NP's

least continuous topic ('least predictable')

This gradation corresponds exactly to the *phonological size* of these devices, whereby zero anaphora is smallest and full NP's are largest. And this clear iconicity relation may in turn suggest the following explanatory principle:¹⁶⁾

(2) "Expend less code—i.e. effort—on tasks that are easier and more obvious"

The same study (Givón, ed., 1983) also suggests a related gradation of less-syntactic, more rudimentary coding devices, incorporating word-order:¹⁷⁾

(3) *most obvious topic*

COMMENT (zero topic)

COMMENT-TOPIC

15) See discussion in Givón (1979, Ch. 8).

16) See further discussion in Givón (forthcoming) and Haiman (forthcoming).

17) In Givón (1984) the data-base investigated was that of rudimentary pidginized English, where devices such as topic repetition or comment repetition are prevalent, and where topic, comment and their relative order are relatively accessible, but 'subject', 'object', 'verb' or 'word-order' are not.

↓ TOPIC-COMMENT
 ↓ TOPIC (zero comment)

Here the governing principle has nothing to do with size, and may be summed up as:

(4) "Attend first to the most urgent task"

Whatever coding principles or iconicity principles one may identify, at whatever levels of generality, may ultimately be explained in terms of the various explanatory parameters of language, such as semantic or communicative function, ontogenetic, phylogenetic or diachronic evolution, cognitive and neurological structure or social-cultural adaptation.

8. Cross-linguistic typology in syntax

Once functional domains in syntax are identified in a non-arbitrary fashion, one may proceed with a non-arbitrary typological comparison of the ways in which different languages perform the same or closely-related functions, and thus arrive eventually at a typology of any functional domain.¹⁸⁾ When this is done, with the normal bind of deciding between major vs. minor types and thus between 'difference in kind' and 'difference in degree', one most commonly finds that the *typological variety* within a functional domain is severely constrained. That is to say, the inventory of "coding types" available cross-linguistically is limited, and the same major *leitmotifs* tend to recur cross-linguistically. The structuralist may acknowledge this as a fact governed by *genetic structure*—and thereby congratulate himself/herself on having achieved 'explanation'.¹⁹⁾ The task for a broader type of linguistics is to proceed beyond such a gimmick and seek systematic explanations to the seeming paucity of coding types, in two broad areas:

- (a) Language universal function-related explanations, such as the iconicity relations discussed in section 7. above; and
- (b) Language-specific explanation, whereby functionally-related domains in the grammar constrain each other in terms of *available coding means*, *functional proximity* and *coding density*.

Once the second step is practiced seriously, one may arrive at an overall view of the grammar as not only a disparate collection of coded functions, but as an *organism* replete with various interdependencies, correlations and well-motivated interactions. In this respect, a biological analogy may perhaps be again illuminating. One would expect, in terms of anatomy and physiology, that the circulatory system, pulmonary system, digestive system and eliminatory system in the body may exert more stringent mutual constraints upon each other than on

¹⁸⁾ Just as important is, of course, the intra-language comparison of the ways in which the *same* language performs related functions within the same realm, domain or sub-domain. Section 7. above in fact deals with this, within the context of isolating iconicity relations and meta-iconic principles.

¹⁹⁾ Cf. for example Chomsky (1968) or a more reasonable but ultimately just as self-defeating a rendition in Bickerton (1981).

either the skeletal or reproductive systems. Similarly, one would expect more intimate interaction between the skeletal and muscular systems than between either and the digestive or reproductive systems. The organization of biological function is thus just as hierarchic as the organization of linguistic function, with sub-domains closely coordinated within domains and domains joining into higher level functional realms. While this type of investigation in linguistics is yet to outgrow its mere infancy, some encouraging beginnings are already with us. Thus, for example, the case-marking, word-order and voicing typology of a language, which together code primarily propositional-semantic function (plus a portion of clausal-topic assignment), have been known from the dawn of linguistics to constrain all other syntactically-coded functional domains in the grammar. In a similar vein, relativization in many languages is also constrained by case-coding devices available via dative-shifting and passivization (Givón, 1979, Ch. 4). While the bulk of research in this area is yet to be done, the worth of the general approach is no longer in doubt. And it is only by marrying the functional and typological investigation of syntax that one may begin to approach this task.

9. Developmental constraints on synchronic structure

One of the most glaring Achilles heels of structuralism, from Saussure via Bloomfield and on to Chomsky, has been its profound anti-developmental approach to the structure of language. This is of course not an accident, since development—be it *phylogenetic*, *ontogenetic* or *diachronic*—is motivated primarily by functional-adaptive requirements of a task-oriented organism. Thus, an approach that disregards function is equally apt to disregard the major venues by which *non-arbitrary* relations between function and structure are achieved. In the study of syntactic evolution over the life-time of a single *individual* (ontogeny), one observes that structures that code some functional domain tend to spread over time to closely-related domains. The historical change—and transmission—of syntax over the lifetime of a *population* (diachrony) is similarly motivated by functional considerations. This is noted, for the passive, in Givón (1981), but is really rather uncontroversial by now. There is, further, little reason to assume that the evolution of language and syntax over the lifetime of the *species* (phylogeny) is motivated in any other way.²⁰ One may thus view the developmental, evolutionary aspect of language as the mediating tool via which structure and function become correlated. Ultimately, then, the study of syntax from a functional-typological perspective cannot become fully systematic without integrating it closely with the study of linguistic evolution.

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12-2

Principles of Discourse Deletion

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§ 1. Introduction

In English, Japanese and all other languages that I know, certain constituents in a sentence can be deleted when the condition of discourse recoverability is met. For example, observe the following exchanges:

(1) Speaker A: Can you see Mt. Fuji from where you live?

Speaker B: Yes, I can see it \emptyset on clear days.

(2) Speaker A: Did you find any letters in my mailbox?

Speaker B: Yes, I found some \emptyset .

In (1B) and (2B), *from where I live* and *in your mailbox* are missing. Constituents that can be missing under the condition of discourse recoverability are called "optional" constituents. On the other hand, the object *it* of (1B), for example, cannot be missing in spite of the fact that it is perfectly recoverable from context. Constituents that cannot be missing even when the recoverability condition is satisfied are called "obligatory" constituents. The object of the verb *see* is an obligatory constituent.¹⁾

The above discourse deletion process, however, seems to apply selectively. There are contexts in which deletion of optional constituents results in unacceptability: # is used here to mark sentences that are unacceptable in the specified context, but which might be acceptable given appropriate contexts.

(3) Speaker A: Did you get your degree *last year*?

Speaker B: a. Yes, I got it *last year*.

b. #Yes, I got it \emptyset .

(4) Speaker A: Did you find this letter *on the front lawn*?

Speaker B: a. Yes I found it *there*.

b. #Yes, I found it \emptyset .

Given the way that (3A) and (4A) are ordinarily interpreted, the (b) sentences are not appropriate answers. It might be possible to come up with contexts which would make the (A-Bb) exchanges appropriate, but such contexts would be marked ones and are not immediately obvious. What makes deletion of

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1) *See* can have an object missing, as in *We see with our eyes*. However, this usage is limited to when a generic act of seeing is referred to, and is banned when a more specific object is involved.

optional constituents possible in (1, 2), and inappropriate in (3, 4)? This paper addresses itself to this question, and proposes a constraint on discourse deletion that seems to have a broad scope of application both within individual languages and cross linguistically.

§ 2. Pecking Order of Deletion

The phenomenon that we observed in the preceding section is clearly a nonsyntactic one, and seems to be related to the relative importance of the information content of the syntactically optional constituent that undergoes deletion. Observe the following discourse fragments:

- (5) Speaker A: Did you buy a watch *in Switzerland*?
 Speaker B: Yes, I bought one Ø.
- (6) Speaker A: Did you buy *this watch in Switzerland*?
 Speaker B: #Yes, I bought it Ø.

In the ordinary interpretation of (5A), the focus of the question is either *a watch* or *buy a watch*. In this interpretation, *in Switzerland* is a thematic adverb and is not under the scope of questioning, as is shown by the fact that it can be readily placed at the beginning of the sentence as a scene-setting adverb:²⁾

- (7) *In Switzerland*, did you buy a watch?

In this interpretation, *a watch* or *buy a watch* conveys information which is more important than the information provided by *in Switzerland*. In (5B), the focus of the question is repeated, while the thematic adverb *in Switzerland*, which conveys less important information than *a watch* or *buy a watch*, is missing. The acceptability of the sentence as an answer to (5A) suggests that an optional time adverb can be deleted if it conveys information that is less important than the constituents (in this case, *a watch* or *buy a watch*) that are left behind.

Let us now consider the information content of the relevant constituents in (6A). Here, in the most ordinary interpretation, *in Switzerland* is the focus of the question, and *this watch* a nonfocus of the question. If (5A) can be paraphrased with (8), (6A) can be paraphrased with (9):

- (8) a. What did you buy in Switzerland?
 b. What did you do in Switzerland?

- (9) Where did you buy this watch?

We can say that in (6), *in Switzerland*, as the focus of the question, conveys more important information than *this watch* or *buy this watch* does. The unacceptability of (6B) as an answer to (6A) suggests that it is not possible to delete a constituent which conveys important information while retaining constituents which convey less important information.

The above observations lead to the following formulation:

- (10) *Pecking Order of Deletion Principle*: Delete less important information first, and more important information last.

²⁾ See Kuno (1975) for detailed discussion on the distinction between scene-setting thematic adverbs of time and place, and time- and place- specifying adverbs.

The following sentences further illustrate this principle in operation:

- (11) Speaker A: Were you already born in 1960?
 Speaker B: Yes, I was already born \emptyset .
- (12) Speaker A: Were you born in 1960?
 Speaker B: #Yes, I was born \emptyset .
- (13) Speaker A: Were you born in Tokyo?
 Speaker B: #Yes, I was born \emptyset .
- (14) Speaker A: How many babies were born in this hospital last year?
 Speaker B: I guess about two hundred were born \emptyset \emptyset .

(11A) is a question that has *already born* as its focus, and *in 1960* as a thematic adverb. This can be confirmed by the fact that *in 1960* can be placed at sentence-initial position without significant difference in meaning. Since *in 1960* is not the most important information, it can be deleted as shown by the acceptability of (11B) as an answer to the question. In contrast, (12A) is a question that has *in 1960* as its focus: the sentence can be paraphrased as a *when* question: 'When were you born?'. *Born*, by virtue of the fact that the birth of the addressee in some year in the past is presupposed, conveys much less important information than *in 1960* does. Similarly, in (13A), the focus of the question is *in Tokyo*. The unacceptability of (12B) and (13B) is accounted for as a violation of the Pecking Order of Deletion Principle, since in these sentences, constituents that represent important information, namely, *in 1960* and *in Tokyo*, have been deleted while the less important *born* has been left behind. It is not possible to explain the unacceptability of these answers by claiming that (*be*) *born* obligatorily requires either a time or place adverb because (14B), which has neither, is an acceptable answer to (14A). What distinguishes (14) from (12) and (13) is the fact that the time and place adverbs in (14A) are not the foci of the question: (14B) is acceptable because the most important information *about two hundred* is left behind, while the less important information *in this hospital* and *last year* has been deleted. This pattern of deletion does not violate the Pecking Order of Deletion Principle, and therefore, unacceptability does not result.

The Pecking Order of Deletion Principle as it has been formulated in (10) embodies three implicit claims. They are:

- (15) A. The crucial factor that determines the order of deletion is that of "more important/less important" and not "newer/older". In earlier formulations of the principle, as found in Kuno (1978a, 1978b, 1979a, 1979b), I erroneously assumed that the crucial factor was that of "newer/older" or "more unpredictable/more predictable". The formulation given in the present paper corrects this mistake.
- B. The order of deletion cannot be accounted for by a dichotomy between important and unimportant. It needs to be based on relative degrees of importance.
- C. The Pecking Order of Deletion Principle does not apply when two or more constituents of the same degree of importance are involved.

In the following sections, I will justify each of these claims.

§ 3. "Importance"

Thus far, I have examined only question-answer pairs, and assumed that the part of an answer that corresponds to the focus of the question is the most important part of the sentence. The following examples show that the applicability of the Pecking Order of Deletion Principle is not limited to question-answer pairs:

- (16) Lots of people have abandoned their cars. John commutes to his office on foot, and
- Bill commutes to his office on foot, too.
 - Bill commutes \emptyset on foot, too.
 - #Bill commutes to his office \emptyset , too.
 - #Bill commutes \emptyset \emptyset , too.

The acceptability of (16b) and the unacceptability of (16c, d) is automatically explained by the Pecking Order of Deletion Principle if we assume that *on foot* conveys the most important information in (16a)—specifically, that it conveys information more important than *to his office* and *commutes*. (16b) is acceptable because the deleted information *to his office* is not more important than either *commutes* or *on foot*. In contrast, (16c) and (16d) are unacceptable because the deleted *on foot* is more important than the retained constituents. We can account for these facts by assuming that the second sentence in (16) is an answer to the understood wh-word question:

- (17) How do Bill and John commute to their offices?

Then, we can say that *on foot* is the most important information in this sentence because it corresponds to the focus of the implicit question.³⁾

We need to ask here how the concept of "importance" here differs from the concept of "newness/unpredictability of information", which has been recognized as a useful concept in discourse analysis. Let us first give a formal definition to the concept of "new/unpredictable":

- (18) *Newness of Information*: An element in a given sentence in a given context conveys new information if it is not recoverable from the *preceding* context when garbled with noise.⁴⁾

According to the above definition, *last May* in Speaker B's response in the dialogue (19) conveys *new/unpredictable* information:

- (19) Speaker A: When did Mary go to Paris?

Speaker B: *She went there last May.*

old/predictable new/unpredictable

3) There is nothing wrong in deleting *on foot* as long as elements that represent less important information are also deleted. Therefore, the following is an acceptable alternative to (16):

(1) Lots of people have abandoned their cars. John commutes to his office on foot, and Bill \emptyset , too.

4) See Prince (1981) for different definitions of what is referred to by terms such as old/new, known/new, given/new and presupposition/focus.

In (19B), *last May* is also the most important information in the sentence because it gives an answer to the focus of the question. Therefore, the above exchange does not distinguish between the concepts of "importance" and "newness" of information. As a matter of fact, (19) represents the normal state of affairs: in general, old information is unimportant information, and new information is important information.

There are instances, however, when these two dichotomies do not coincide. For example, observe the following back-and-forth conversation between Speaker A and Speaker B:

- (20) Speaker A(1): You must have spent a fortune on hotels during your trip. Couldn't you stay with your friends, or your friends' friends?
- Speaker B(1): In some cities, I did, but in many cities, I did have to stay in hotels.
- Speaker A(2): You started out in Paris, right? Did you stay in a hotel there?
- Speaker B(2): No, I didn't stay in a hotel \emptyset —I stayed with an old friend of mine who is studying music there.
- Speaker A(3): Did you stay in a hotel in London? I hear hotels are getting awfully expensive there.
- Speaker B(3): a. Yes, I stayed in a hotel \emptyset because I didn't have any friends there.
b. Yes, I'm sorry to say, in a hotel.
c. #Yes, in London.

In A(3), *in London* is unrecoverable in the sense that if this part of the sentence is garbled with noise, there would be no way for Speaker B to reconstruct it. On the other hand, *in a hotel* in this sentence is recoverable from the preceding context, as witnessed by the fact that Speaker A could have used *How about London?* as a synonymous question. In spite of this fact, the focus of the question A(3) is not *in London*, but is *in a hotel*. In other words, A(3) is a question about where Speaker B stayed in London, and not about what city Speaker B stayed in a hotel in:

- (21) Speaker A(3): *Did you stay in a hotel in London?*
- | | |
|----------------|----------------|
| older | newer |
| more important | less important |

The acceptability of B(3a, b) and the unacceptability of B(3c) shows that the pecking order of deletion is sensitive to the distinction between "more important/less important" and not to the distinction between "newer/older". Thus we see that *in London* in the question is newer, but less important than *in a hotel*, and therefore its deletion does not violate the Pecking Order of Deletion Principle so long as the principle is based on the concept of "importance", and not on that of "newness".

If my explanation is correct that *in London* can be missing in (20B (3a, b)) because it is not the focus of the question in (20A(3)), it should be undeletable

in a context that would force the focus interpretation of the same expression. Such contexts abound. For example, observe the following discourse:

(22) Speaker A(1): Of all the cities that I visited last summer, only in one place did I stay in a hotel. Guess where it was.

Speaker B(1): Let's see. Did you stay in a hotel in London?

a. Yes, I stayed in a hotel there.

b. #Yes, I stayed in a hotel \emptyset .

c. Yes, (in) London.

d. #Yes, (in) a hotel.

Because of the context given in A(1), B(1) is interpreted as meaning "Was it in London that you stayed in a hotel?", with *in London* as the focus of the question. The unacceptability of A(2b, d) is due to the fact that *in London*, which has this focus as antecedent, has been deleted while *in a hotel*, whose antecedent is not a focus, has been left behind.

§ 4. Relative Degrees of Importance

As has already been noted, the Pecking Order of Deletion Principle is stated in such a way as to be sensitive to the relative degrees of importance of the elements in a sentence, and not just to the two-way distinction between "important" and "unimportant". In order to see whether the "relative-degree" formulation is necessary or not, let us examine the following discourse:

(23) Speaker A: Is it possible to go there on foot?

Speaker B: a. Yes, it is possible to go there on foot.

b. #Yes, it is possible to go there \emptyset .

c. Yes, it is possible \emptyset .

d. #Yes, on foot (but the train is more convenient).

e. Yes, possible (but not very wise).

In (23A), there are three constituents whose information content we need to consider: *possible*, *go there*, and *on foot*. The fact that (Bb) is not an acceptable answer to (A) shows that the deleted constituent *on foot* is more important than either *possible* or *go there*. On the other hand, the fact that (Bc) and (Be) are acceptable shows that *on foot* cannot be more important than *possible*. Therefore, it must be that *on foot* is more important than *go there*. Furthermore, the fact that (Bd) is not an acceptable answer shows that at least one of the two deleted constituents *possible* and *go there* must be more important than the retained constituent *on foot*. Since *go there* has already been established as being less important than *on foot*, it must be that *possible* is more important than *on foot*. Thus, we have established that *possible* is more important than *on foot*, which is more important than *go there*.

The above fact shows that we cannot explain discourse deletion facts on the basis of simple dichotomy between "important" and "unimportant". It is necessary to deal with them on the basis of relative degrees of importance.

The above observation does not mean that a sentence can have only one combination of "importance" values assigned to its constituents. It is well known

that emphatic stress shifts the focus of a question from where it ordinarily rests to the element that is emphasized: for example, observe the following sentences:

- (24) a. Did you buy this watch in Switzerland?
b. Did you buy THIS watch in Switzerland?

The way that (24a) is ordinarily pronounced, *in Switzerland*, as the last element in the sentence, receives a normal nonemphatic stress, and serves as the focus of the question. In contrast, in (24b), where distinct emphatic stress is assigned to *this*, the focus of the question has shifted to this stressed constituent. Thus, it becomes possible to retain this important information, and delete the less important *in Switzerland*.

- (25) Yes, I bought that one Ø.

From the above point of view, it is interesting to observe that there are speakers who consider (23Bd) as a (nearly) acceptable answer to (23A). Let us examine the exchange:

- (26) Speaker A: Is it possible to go there on foot?
Speaker B: Yes, on foot.

According to the Pecking Order of Deletion Principle, Speaker B's reply should be possible only when he has considered *on foot* to be the most important information (or at least on a par with *possible*). In fact, it seems that (26B) is possible only if (26A) is given a special intonation destressing *possible* and pronounced with a strong "incredulous question" type intonation on *on foot*.⁵⁾

§ 5. Interaction with Syntactic Constraints

Thus far, I have avoided discussing the most common discourse deletion pattern in question-answer pairs, namely, that of Verb Phrase Deletion.

Observe the following discourse:

- (27) Speaker A: Did you buy this watch in Switzerland?
Speaker B: Yes, I did.

In (27A), as already mentioned, *in Switzerland* is the focus. In (27B), this focus as well as nonfocus constituents *buy* and *this watch* have been deleted. Therefore, there is no violation of the Pecking Order of Deletion Principle involved in the deletion of these latter two constituents. What remains to be looked into is whether the retention of *I* and *did* has violated the principle. I assume that auxiliary verb *did* conveys here the affirmative nature of the answer, and in this sense, it conveys important information. Since (27A) can be answered either with (27B), or with "Yes, in Switzerland," let us assume that *did* (i.e., the affirmative nature of the answer) and *in Switzerland* convey equally important information in the answer. On the other hand, the subject of *did* in (27B) clearly conveys much less important information than the deleted *in Switzerland* if (27A) is uttered without any emphatic stress on *you*. If the Pecking Order of Deletion Principle is correct, (27B) should be unacceptable because *in Switzerland*, which conveys important information, has been deleted while *I*, which conveys less im-

5) I am indebted to John Whitman (personal communication) for this observation.

portant information, has been left behind. Why is it that (27B) is perfectly acceptable in spite of this violation of the Pecking Order Principle? I hypothesize that the Pecking Order of Deletion Principle is sensitive to the distinction between violations which are "intentional", so to speak, and those which are "unintentional". In the case under discussion, the retention of the subject *I*, which carries relatively unimportant information in discourse, is necessitated by a constraint in English which says that a tensed verb must have a surface subject. The moment that the decision was made to leave *did* behind as a marker that conveys the affirmative nature of the answer, the retention of its subject was automatically determined by this surface subject constraint. Thus the violation of the Pecking Order of Deletion Principle attributable to this retention is "unintentional", and therefore, it does not result in unacceptability. Compare this situation with those crucial cases that we have discussed in the preceding sections. For example,

(28) Speaker A: Were you born in Tokyo?

Speaker B: #Yes, I was born \emptyset .

The decision to delete *in Tokyo* while retaining the much less important information was not forced by any syntactic constraint of English: it was an "intentional" decision. Hence, the unacceptability of the sentence.

I have explained above what superficially appears to be a selective application of the Pecking Order of Deletion Principle as the result of interference from a syntactic constraint. This happens to be a subcase of a more general principle that can be stated as follows.

(29) *Active and Passive Discourse-Rule Violations*: Sentences that involve active avoidable (or intentional) violation of discourse principles are unacceptable. On the other hand, sentences that involve passive unavoidable (or unintentional) violation of discourse principles go unpenalized and are acceptable.

The above principle can be independently motivated by various phenomena involving interactions of discourse principles and syntactic rules. Justifications of this principle can be found in Kuno (1978a, 1978b, 1979a, 1979b).

In the above explanation of the acceptability of (27B), I resorted to the assumption that a tensed verb in English requires a surface subject. One might question the plausibility of this assumption by giving examples of the following kind:

(30) Speaker A: What did you do yesterday?

Speaker B: Went to see the movies with Mary.

(31) Speaker A: Did you buy this watch or did you steal it?

Speaker B: Bought it, of course.

Observe, however, the following exchanges:

(32) Speaker A: Did you go to the movies yesterday?

Speaker B: *Yes, went (to the movies).

(33) Speaker A: What pieces did you play?

Speaker B: *Played Beethoven's Sonatas 1 and 3.

The above examples show that in informal colloquial speech (including diary-style writing), the subject of a tensed verb is deletable only when the tensed verb is the unrecoverable focus. Returning to the exchange in (27), it is clear that it does not satisfy this condition for subject deletion, and therefore, we can safely conclude that the subject *I* in the answer has been retained unintentionally, due to the Surface Subject Constraint.

§ 6. Focus-Only Answers

Thus far, we have assumed that discourse deletion is an optional process, and that given a nonobligatory element in a sentence, it can be either deleted or left behind as long as the Pecking Order of Deletion Principle is not violated. For example, observe the following exchange:

- (34) Speaker A: Did you earn any money in the States last year?
 Speaker B: a. Yes, I earned some money in the States last year.
 b. Yes, I earned some money in the States.
 c. Yes, I earned some money last year.
 d. Yes, I earned some money.

The above phenomenon makes for an interesting contrast with the following:

- (35) Speaker A: What was John studying?
 Speaker B: a. *Studying math.
 b. Math.

The unacceptability of (Ba) seems to indicate that discourse deletion of *studying* is obligatory if the answer is non-sentential.

It seems that the above conflict can be resolved only if we assume that there is a strategy for giving a minimal focus-only answer, and that all *nonsentential* answers are generated by this strategy.⁶⁾ I should simply mention here that this strategy in English still requires subcategorized elements of verbs to be present even if they are not foci, as shown in the following:

- (36) Speaker A: What did you decide to do about the money?
 Speaker B: a. Put it in the bank.
 b. *Put Ø in the bank.

§ 7. Twisted Answers

Thus far, I have assumed that in question-answer exchanges, it is always the case that the focus of the question is also the focus of the answer. This is certainly a normal state of affairs, but there are many instances in which the answerer shifts the focus because he wants to say more than what he is being asked to say, or because he wants to change the direction of the conversation. For example, observe the following variation on (20) in the back-and-forth exchange between Speakers A and B:

- (37) A(1): You must have spent a fortune on hotels during your trip. Couldn't you stay with your friends, or your friends' friends?

6) This strategy, called Stripping, was first proposed by Hankamer (1971) to account for short answers to Wh-questions.

- B(1): In some cities, I did, but in many cities I did have to stay in hotels.
 A(2): You started out in Paris, right? Did you stay in a hotel there?
 B(2): Yes, I stayed in a hotel \emptyset because my friend there was out of town.
 A(3): Did you stay in a hotel in London? I hear hotels are getting awfully expensive there.
 B(3): a. Yes, I stayed in a hotel in London, too.
 b. Yes, in London, too.

What needs to be explained is why in B(3), the answer (b) is acceptable. A(3) has (*stay*) in a hotel as the focus of the question, and therefore, in Speaker B's response, one would expect that (*stayed*) in a hotel would be more important than in London. Therefore, the Pecking Order of Deletion Principle predicts that (b) would be unacceptable, but in actuality, it is a perfectly acceptable answer.

Closer examination of A(3) and B(3) shows, however, that there is a definite, albeit subtle, shift of focus between the question and the answer. As has already been noted, A(3) is a question about where Speaker B stayed in London, and not about what city he stayed in a hotel in. Now, there seem to be two ways to interpret B(3a). Below, I will show what Speaker B has said in this conversation:

- (38) ... Yes, I stayed in a hotel (in Paris) because my friend there was out of town ... Yes, I stayed in a hotel in London, too.

The last sentence of (38) is interpretable either as (i) a statement about where Speaker B stayed in London, or as (ii) a statement about what cities he stayed in a hotel in. In the second interpretation, the focus has shifted from *in a hotel* in the question to *in London, too* in the answer. In this interpretation, *in London* is the focus of the statement, and hence, there is no problem in retaining this constituent, and deleting the rest of the sentence, as shown in B(3b).

The same kind of focus switch is observable in the following alternative continuation of (37B(2)):

- (39) A(3): Did you stay in a hotel in London? I hear hotels are awfully expensive there.
 B(3): a. No, I didn't stay in a hotel in London.
 b. No, not in a hotel.
 c. No, not in London.

B(3b) seems to be a straightforward answer to the question, which has *in a hotel* as the focus. On the other hand, there seems to be a shift in focus between A(3) and B(3c). It seems that this answer means the following:

- (40) Speaker B: I stayed in a hotel in Paris, but not in London.

The above statement is readily interpretable as a statement about what cities Speaker B stayed in a hotel in. In this interpretation, *in London* is the focus of the statement, and hence, it is possible to leave it undeleted while deleting the rest of the sentence.

Similarly, observe the following exchange:

- (41) Speaker A(1): Is it possible to go to the museum on foot?

- Speaker B(1): Yes, it's possible.
 Speaker A(2): Is it possible to go to the aquarium on foot?
 Speaker B(2): Yes, it's possible.
 Speaker A(3): Is it possible to go to the stadium on foot?
 Speaker B(3): a. No, it's not possible.
 b. No, not to the stadium.

B(3a) is a straightforward answer in which Speaker B considers *possible* as the most important information. In contrast, B(3b) is not a straightforward answer: Speaker B has shifted the focus from *possible* to *to the stadium*. The discourse which started out as one about how difficult or easy it is to go to Place X on foot has ended up with a statement about where one can go on foot.

Focus switch of the kind that we have observed above is used very frequently in conversation for changing the direction of conversation. For example, observe the following:

- (42) Speaker A: Have you read this book?
 Speaker B: No, not that book, but I have read a spy story by the same author.

Speaker A meant the question as one about whether Speaker B has read or has not read the book under discussion, namely, he meant it as a question having *have read* as its focus. Speaker B has shifted the focus to the book under discussion, by reinterpreting the question as meaning 'What kind of books have you read?', and drawn Speaker A into his own sphere of control—talking about the book of his own choice that he has read.

§ 8. Conclusion

In this paper, I have shown that deletion of optional constituents in a sentence follows a certain principle: namely, it proceeds from less important information to more important information, and that it never applies in the reverse order. I have also shown how this principle interacts with syntactic constraints in English, especially, that for surface subjects for tensed verbs. I have also shown how the Pecking Order of Deletion Principle can be either intentionally or unintentionally violated by speakers to change the direction of conversations or for some other purposes.

My research shows that the Pecking Order of Deletion Principle applies to Russian, French, German, Turkish, Thai, Japanese, Korean, Arabic and Hebrew as well. Therefore, it is fairly safe to assume that the principle is at least a near language universal, and most likely a true language universal.

The Pecking Order of Deletion Principle itself might sound too natural and *matter-of-course* to merit attention, but the principle, coupled with the Active and Passive Discourse-Rule Violations Principle, which dictates that only "intentional" violations of discourse principles result in unacceptability, give us a means, until now unavailable, for initiating a systematic analysis of extremely complex and poorly understood discourse deletion phenomena. Preliminary results of such an attempt for Japanese and Russian are found in Kuno (1982).

It is hoped that the present paper becomes a first step along a new avenue of research on the interaction of discourse and syntactic constraints.

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Toward an Understanding of the Typology and Function of Case-Marking*

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While the typology of case-marking has been taken as an important feature in classifying the languages of the world, variations within a type and the function of case-marking in general have not been given due scrutiny until very recently. From a typological point of view, languages have been classified as (nominative-)accusative or as ergative(-absolutive).¹⁾ However, recent studies, particularly of the so-called ergative languages, indicate that many languages have mixed (ergative and accusative) case-marking systems, indicating the difficulty of using case-marking as an unequivocal typological feature.

A prevailing attitude with regard to the function of case-marking has been to assume that it indicates grammatical relations of noun phrases in a clause. In particular, identification of the subject of an intransitive sentence and the subject of a transitive sentence made by the nominative case has figured importantly in our understanding of the structure of accusative languages. However, the same procedure when applied to ergative languages has led to an uncomfortable understanding of the structure of ergative languages, in which what (from the point of view of an accusative language) appear to be objects of transitive sentences are case-marked similarly as subjects of intransitive sentences. Recent investigations have shed much light on the relationship between case-markers and grammatical relations as well as on other functions of case-marking, though our understanding is far from satisfactory. This paper examines issues involved in the typology of case-marking and those pertaining to the function of case-markers.

1. Problems in case-marking typology

Following the recent practice in the field, I use the symbol S in reference to the obligatory argument of an intransitive clause; i.e. what corresponds to *Bill* in the English sentence *Bill came*. The symbols A and O refer to what respec-

* An earlier version of this paper was read before a plenary session of the 13th International Congress of Linguists on August 30, 1982 in Tokyo. The topics covered in this paper are discussed from a different angle in another paper published in *Lingua* 57 (1982) under the title "Japanese Grammar and Universal Grammar." I am grateful to Anna Wierzbicka, Scott DeLancey, Mimi Klaiman, and Tasaku Tsunoda, who commented on an earlier version of this paper. I have profited greatly from both their comments and their thought-provoking papers on related topics.

1) Cf. Trubetskoj, N. S., 'Le Rapport entre le déterminé, le déterminant et le défini,' in *Mélanges de linguistique offerts à Charles Bally*, Geneva: 1939.

tively correspond to the subject and the object of a transitive sentence in an accusative language; i.e. what corresponds to *Bill* is A and what corresponds to *the bear* is O in a sentence that translates *Bill killed the bear*. The accusative/ergative dichotomy is based on the ways in which S, A, and O are case-marked.²⁾ Languages that case-mark S and A identically and O differently from these are called nominative-accusative or simply accusative, for S and A are distinguished from O by marking the latter, while leaving the former very often unmarked. Ergative languages, on the other hand, case-mark S and O identically in the absolutive (often unmarked) and A with a special ergative marker. The following examples illustrate typical situations:

(1) Accusative pattern: Quechua

a. Juan wañu-n.

die-3sg

'Juan died.'

b. Pedro Juan-ta wañu-či-n.

ACC die-CAUS-3sg

'Pedro killed Juan.'

(2) Ergative pattern: Warrgamay

a. nulmburu gaga-ma.

woman-ABS go-Fut.

'The woman will go.'

b. maal-du nulmburu nunda-lma

man-ERG woman-ABS see-Fut.

'The man will see the woman.'

(Dixon 1980)

While the two types of case-marking illustrated above appear to be quite distinct and may seem to provide a clear-cut feature for the purpose of typological classification, rarely is any language consistent throughout its case-marking paradigm. Most often the so-called ergative languages have what Silverstein (1976) calls "split case-marking"—a systematic display of both ergative and accusative patterns. This is most clearly seen in the ergative languages of Australia, where in one case the split is conditioned by the inherent semantic nature of noun phrases; most typically nouns and 3rd person pronouns exhibit the ergative pattern and 1st and 2nd person pronouns the accusative pattern. Contrast the following Warrgamay forms with 1st and 2nd person pronouns with those in (2), where nouns are involved.

(3) a. nana gaga-ma.

we (NOM) go-Fut.

'We go.'

2) Case-marking may be realized in a number of different morphological types, most typically in case inflections, affixes, particles or enclitics. Furthermore, ergativity can be manifested in terms of word order and agreement, both of which can co-occur with case-marking. In this paper however, our discussions are confined to the case-marking phenomena of noun phrases.

- b. nyuurra nana-nya nunda-lma.
 you (NOM) we-ACC see-Fut.
 'You will see us.'

(Dixon 1980)

In addition to the split case-marking languages, there are languages whose case-marking is ambiguous in that S's and A's are divided into two classes and are marked differently. What is called the active-type language involves different marking of S's depending on their nature. In this type, most well attested in the Amerindian languages of the Siouan family and among Tibetan languages, S's of one type require an ergative marker, and the other S's an absolutive marker. The Lhasa Tibetan data below illustrate this type:

(4) Active-type language: Lhasa Tibetan

- a. ña-s stag bsad-pa-yin.
 I-ERG tiger kill-PERF/VOLITIONAL
 'I killed a tiger.'
- b. ña-s ñus-pa-yin.
 I-ERG cry-PERF/VOLITIONAL
 'I cried.'
- c. ña ši-byuñ
 I die-PERF/INVOLITIONAL
 'I died.'

(DeLancey 1982b)

In the so-called active-type languages illustrated above, it is S's that manifest two different case-markers—A's receiving consistent ergative marking.

In the case of North Caucasian ergative languages as described by Catford (1975), not only intransitive sentences but also transitive sentences have differential A marking with concomitant meaning differences. Among the oft-quoted examples are the following Batsby and Kabardian forms:

(5) Batsby (Volitionality of A)

- a. tʰo naizdraʃ qitra.
 we (NOM) to-the-ground fell
 'We fell to the ground (unintentionally).'
- b. atʰo naizdraʃ qitra.
 we (ERG)
 'We fell to the ground (intentionally).'

Kabardian (Affectedness of O)

- a. ħe-m qʷ'ipšĥe-r je-dzaq'e.
 dog-ERG bone-NOM bites
 'The dog is biting the bone—bites through to the marrow.'
- b. ħe-r qʷ'ipšĥe-m je-w-dzaq'e.
 dog-NOM bone-ERG
 'The dog is biting the bone—gnaws around superficially.'

(Catford 1975)

While the complications in case-marking discussed above may seem to be special properties of ergative languages, accusative languages in fact also show variations in the case-marking pattern. Among the deviations from the canonical case pattern of a transitive clause, the following are most frequently observed.³⁾

- | | | | | |
|-----|-----|-----|-----|-----------------------|
| (6) | | A | O | |
| | (a) | NOM | ACC | V (canonical pattern) |
| | (b) | NOM | DAT | V |
| | (c) | NOM | NOM | V |
| | (d) | DAT | NOM | V |

The NOM-DAT pattern is seen in many languages including Japanese with verbs for 'to meet,' 'to follow,' etc., Turkish with verbs for 'to begin,' 'to touch,' etc., Russian with verbs for 'to help,' 'to trust,' etc. and German with verbs for 'to help,' 'to thank,' etc. In addition to the NOM-DAT pattern, many languages allow O to be marked by other case forms such as a partitive or genitive form under specific circumstances.

What interests us most in connection with our discussion of the function of case-markers in nominative-accusative systems are the NOM-NOM pattern and the DAT-NOM pattern. The former, however, is not widely attested, the clearest cases observed only in Japanese and Korean. For example:

- (7) Japanese
 Taroo ga Hanako ga sukida.
 NOM NOM like
 'Taro likes Hanako.'

- (8) Korean
 Chalsu ka yŏnhi ka cotha.
 NOM NOM like
 'Cholsu likes Yunghi.'

In both languages the predicates expressing the meaning of 'to like,' 'to dislike,' 'to be good at,' and the like exhibit the NOM-NOM pattern.

Compared to the above pattern, the DAT-NOM pattern is observed quite widely. Here O is in the nominative just like S and A of a prototypical transitive clause, while A is in the dative. In all the relevant languages the constructions that exhibit this pattern involve two-place predicates expressing the notions of liking, desire, possession, necessity, and ability. To give just one example from different languages:

- (9) a. Latin: Puero liber est.
 boy-DAT book-NOM is
 'The boy has a book.'

3) Throughout this paper, the first case form refers to the marking of A and the second case form the marking of O regardless of actual word order in a language.

- b. Spanish: Me gusta la cerveza.
I-DAT like the beer-NOM
'I like beer.'
- c. Russian: Mne nužna kniga.
I-DAT necessary book-NOM
'I need a book.'
- d. Kannada: Manage jarman baratte.
I-DAT German-NOM can
'I can (understand) German.'
- e. German: Mir gefallen diese Damen.
I-DAT like these ladies-NOM
'I like these ladies.'
- f. Turkish: Ban-a para lazım.
I-DAT money-NOM need
'I need money.'
- g. Korean: Kim-ssi-eke ton-i mantha.
Mr.-DAT money-NOM much
'Mr. Kim has a lot of money.'
- h. Japanese: Taroo ni eigo ga wakaru.
DAT English NOM understand
'Taro understands English.'

As observed, the parallel DAT-NOM pattern spreads over a wide variety of languages that are normally considered to be accusative languages. The marking of O with the nominative marker can be construed as an expression of ergativity in these accusative languages (Moravcsik 1978, Shibatani 1979). But the realization that in many ergative languages the parallel constructions shun the ergative pattern casts some doubt on this view. Tsunoda (1981) culls a large number of examples from divergent ergative languages which show that the constructions paralleling the DAT-NOM pattern of accusative languages exhibit the DAT-ABS, LOC-ABS or other patterns. For example:

- (10) Ladakhi: kho-e thug-gu-ñis yot.
he-DAT child-two (ABS) to be
'He has two children.'
- Tibetan: ña-la khañpa yod.
I-LOC house (ABS) be
'I have a house.'
- Avar: di-ye j-as j-ol'ula.
I-DAT f-girl (ABS) f-love (GEN) (PRES)
'I love the girl.'
- Georgian: šven sami švili gvqvs.
we-DAT three children (NOM) be
'We have three children.'

Djaru: ngaju nga-rna ngaringka-yaru
 ISg-ABS C-ISg (NOM) woman-HAVING-ABS
 'I am with a woman,' 'I have a woman,'
 'I am married,' etc.

Moreover, if one is to define the ergative construction in terms of transitive agent-marking, like DeLancey (1981: 628), or in terms of the presence of a prototypical transitive verb, like Tsunoda (1982), the DAT-NOM construction in question does not qualify as an ergative construction. However, we shall see below that these narrow views of ergativity miss the essence of the ergative construction, and that the broader view that focuses on nominative/absolutive marking on O and that thereby encompasses the DAT-NOM construction is more illuminating. This discussion is most fruitfully carried out in connection with the function of case-marking, and we therefore hasten to conclude the discussion of the typology of case-marking.

As apparent from the above discussion, case-marking patterns are rarely consistent even within one language, be it the so-called accusative type of language or the so-called ergative type of language. The recent research on the typology of case-marking thus has shown that the ergative/accusative dichotomy is not applicable to the entire structure of the majority of languages, thereby reducing the value of ergativity as a useful classificatory feature, but it has resulted in the discovery of important cross-linguistic patterns in case-marking. Not only are the pattern exhibited by the split case-marking systems governed by the principle embodied in Silverstein's (op. cit.) hierarchy, but also the kind of deviation in case-marking observed in (6)–(9) and in (10) is controlled by a common factor. Specially, Hopper and Thompson (1980) and Tsunoda (1981) show that the canonical ergative and accusative patterns and the deviations from them are largely determined by the degree of transitivity.

Hopper and Thompson characterize the notion of transitivity in terms of a collection of parameters which include familiar notions such as agency, volitionality, and affectedness of O; those expressions with a high degree of agency on the part of A, volitional A, and totally affected O have a very high degree of transitivity. A similar attempt has been made by Tsunoda, who defines a transitivity hierarchy in terms of "effectiveness"—as to whether the expressed activity is realized/completed and whether it affects/impinges on O. Sentences with verbs like 'kill' and 'break' are among the sentences that have the highest degree of transitivity (especially when they are in the perfective aspect). And it is in these sentences that either the ergative or the accusative pattern is manifested most clearly across languages. As the transitivity of sentences declines with the choice of stative predicates, languages tend to exhibit deviations from both the ergative and the accusative pattern and the contrast is lost. This is what is seen in (9) and (10). In other words, there is an area of overlap in case-marking between ergative and accusative languages. A conclusion to be drawn from this is that, aside from the existence of a clear-cut split case-marking lan-

guages, the notion of ergative case-marking or that of accusative case-marking does not characterize the entire case-marking system of a language. On the basis of the case-marking patterns of prototypical transitive clauses, we may still characterize a language to be either ergative or accusative, as we do in this paper, but the nature of ergativity must be pursued apart from this general use of the classificatory terms.

2. Function of case-marking

We will now turn to the discussion of the function of case-marking. Our discussion is concerned with those case-markers that do not have clear correlations with the semantic roles of noun phrases. In other words, we will be dealing primarily with the case-markers identified as the nominative, the accusative, the ergative and the absolutive.

2.1. Discriminatory function

While the case-markers such as the ergative and the accusative have special functions under specific circumstances, as discussed below, the basic motivation for case-marking undoubtedly relates to the discriminatory function; that of discriminating A from O in a transitive clause. This basic function of case-marking has been extensively discussed by Silverstein (1976), Comrie (1978) and Dixon (1979), and therefore there is not much to be added here other than briefly summarizing their discussion.

Though there are many languages that have distinct markers for both A and O, from the point of view of a discriminatory function it suffices to mark only one of the arguments. Indeed this seems to be a normal situation in both accusative and ergative languages (cf. the examples in (1) and (2)). The ergative/accusative distinction is made on the basis of which of the two arguments of a transitive clause receives marking: If O is marked, leaving A and S unmarked, then the accusative pattern obtains, while if A is marked and O and S are left unmarked, then the ergative pattern results. Furthermore, as noted by Comrie (*op. cit.*), there does not seem to be any language that marks S specially, leaving A and O unmarked. This is understandable in a theory that attributes a discriminatory function to case-marking, for such a system fails to discriminate between the two nominals, A and O that co-occur in a transitive clause—a situation where discrimination is called for.

2.2. Semantic correlates

There are situations where the occurrence of an ergative marker is correlated with a meaning. The active-type languages, illustrated in (4), display such a correlation. Thus in (4b) the subject is a volitional agent in crying while in (4c), the subject is an involuntary patient undergoing a change of state. Active-type languages in general show this kind of correlation between an ergative marker and agentivity, and the same correlation is seen in the Batsby forms in (5). In fact, the two sets of examples in (5) illustrate two typical cases where

the ergative marking is correlated with meaning; namely (i) when A is agentive (the Batsby case), and (ii) when the affectedness of O is intense (the Kabardian case, and see 2.4. below).

As we saw in (10), an ergative marker is generally avoided when A is not agentive (see Tsunoda (1981) for a relevant discussion) indicating that even in those languages that do not systematically employ an ergative marker semantically as in active-type languages, the absence of volitionality in A is correlated with non-ergative markers, most typically the dative form. A parallel situation is observed in accusative languages, where, as in (9), a non-nominative marker is used when a non-volitional A is involved. The examples (9), where a non-volitional A is in the dative, are typical of the situation, but Bengali marks non-volitional A's with the genitive marker (Klaiman 1980). Observe the contrast in the following Kannada and Bengali pairs, where (a)'s, the direct forms, express the involvement of volitional A's, and (b)'s that of non-volitional A's.

(11) Kannada

- a. avanu jvara (-vannu) barisikonḍa.
he (NOM) fever (-ACC) cause-come-Pst.
'He got a fever.'
- b. avanige jvara bantu.
he (DAT) fever came
'He got a fever.'

(Sridhar 1976)

Bengali

- a. se naak ḍaake.
he nose calls
'He snores.'
- b. taar naak ḍaake.
his nose calls
'He snores.,

(Klaiman 1980)

(12) Kannada

- a. avanu ī suddi (-yannu) tiḷidukonḍanu.
he (NOM) this news (-ACC) learnt
'He learnt this news.'
- b. avanige ī suddi tiḷiyitu.
he (DAT) this news became known
'He came to know this news'

(Sridhar 1976)

Bengali

- a. aami tomaake caai.
I you-O need
'I want you.'

- b. aamaar tomaake caai.
 my you-O need
 'I need you.'

(Klaiman 1980)

From the observation like the above, one might be led to conclude that the nominative case marks an agentive A. However, this is not quite correct, for in languages like Japanese and Korean the dative A in the DAT-NOM construction exemplified by (9g, h) can alternate freely with the nominative A without noticeable difference in meaning. That is, unlike the Kannada and the Bengali pairs above, (9g) and (13a), and (9h) and (13b) are virtually synonymous; i.e., no volitionality is implied by the presence of the nominative A's in the following examples:

- (13) a. Kim-ssi-ka ton-i mantha.
 Mr.-NOM money-NOM much
 'Mr. Kim has a lot of money.
 b. Taroo ga eigo ga waku.
 NOM English NOM understand
 'Taro understands English.'

Just as the ergative in many ergative languages has a generalized A marking function, the nominative in many accusative languages has a generalized A marking function. In such cases the ergative and the nominative do not strictly correlate with the presence of volitionality in A. On the other hand, the opposite correlation does seem to obtain; namely, a non-nominative or non-ergative A is correlated with the absence of volition in A.

Catford (1975) distinguishes between functional ergative languages and formal ergative languages. In the former the ergative construction alternates with the nominative construction with a concomitant meaning change (see the Kabardian forms in (5)), while in the latter the ergative marker is obligatory for A and hence only has a generalized A-marking function. Functional ergative languages are relevant to our discussion, but since the discussion involves the overall ergative-absolutive pattern rather than individual markers, we will defer it until a later section.

2.3. Case-marking and grammatical relations

We now turn to the problem of ascertaining a possible function relating to the correspondence in case-marking between S of an intransitive clause and one of the arguments of a transitive clause. The question is whether the nominative case that identifies S and A in the accusative language and the absolutive case, which identifies S and O in the ergative language, have any function in unifying the respective two types of arguments. One extreme view, derivable from the explanation based on the discriminatory function, is that they do not have any positive function; S and A or A and O are simply identified by default. That is, the discrimination of argument types is required only in the transitive

clause, and even here only one argument (either A or O) needs to be marked. In the case of S of an intransitive clause, no marking is needed since there is only one argument. Thus, if A of a transitive clause is marked, as in the ergative language leaving O unmarked, then O and S end up being identified; both are unmarked. By the same token, if O is marked, leaving A unmarked, A and S result in a residual identification. (cf. Dixon 1979.)

In view of the fact that the nominative in an accusative language and the absolutive in an ergative language are most often indispensable elements in the respective type of language (cf. Shibatani 1977, Dixon op. cit.), it is unlikely that they are devoid of a positive function. A prevailing traditional view, largely based on accusative languages, is of course that the nominative indicates the grammatical function of subject. In other words, in accusative languages, the nominative indicates that A and S are subjects. By extension, in ergative languages some consider that S and O are subjects for they are identified by the absolutive—the counterpart of the nominative. What is crucial in this view is explication of the notion of subject. Perhaps the most explicit syntactic attempt at this is that seen in the practice of Relational Grammar. Here the subject is explicated in terms of its syntactic functions; e.g. they agree with verbs, function as antecedent of a reflexive, undergo Equi-NP deletion. Indeed it is not the case that any NP of a clause is associated with these syntactic properties—rather only specific NP's are syntactically prominent. In short, subjects are those NP's that have syntactic priority over other NP's.

Investigations of the syntax of ergative languages have revealed that in perhaps the majority of ergative languages, the morphological identification of S and O by the absolutive is not correlated with the syntactic grouping of NP arguments. Specifically, for the syntactic purposes S and A, which have distinct case-markers, function as subject (see Anderson 1976, Chung 1978, Dixon 1979).⁴⁾

In the nominative languages a similar situation is observed with regard to the DAT-NOM construction exemplified in (9). In this construction O is in the nominative but except its marking and verb agreement, the nominative O has no other syntactic subject properties. It is the dative A that is accorded with subject properties including the position in word order. For example, in both Japanese and Korean, reflexives are controlled by the subject, which in the normal construction is in the nominative, but in the DAT-NOM construction dative NP functions as the antecedent of the reflexives *zibun* and *caki*.

(14) a. Japanese

Yamada	ni	wa	Taroo	ga	<i>zibun</i>	no	otooto	yor
			DAT	TOP	NOM	self	of	brother
			yoku	wakaru.				
			well	understand				

4) The notable exception is, as well-known in the field, Dyirbal in which S and O are grouped together for a syntactic purpose (cf. Dixon 1979).

(Lit.) 'Yamada_i understands Taro_j better than self_i's/*self_j's brother.'

b. Korean

Ki saram eke nin *caki* ii saenkak i phiryo ha-ta.
that person DAT TOP self of idea NOM need
'He needs his own idea.'

Similarly, the so-called subject honorification is controlled by the dative NP in the construction under discussion rather than by the nominative NP.

(15) a. Japanese

Yamada sensei ni wa okosan ga o-ari-ni naru.
professor DAT TOP child NOM have
(honorific)

'Professor Yamada has a child.' (Prof. Y exalted.)

b. Korean

Ki samsaenim eke nin ton i manhi iss-i-si-ta.
that teacher DAT TOP money NOM much have

'That teacher has a lot of money.' (The teacher exalted.)

Sridhar (1976) shows that in the DAT-NOM construction in Kannada, the dative NP controls the reflexive, and it also undergoes Equi-NP deletion—the properties associated with the subject.

(16) Kannada

a. dinēshanige kyārolin *tannannu* prītisuttāle endu gottu.
Dinesh-DAT Caroline self-ACC loves compl. knows
'Dinesh knows that Caroline loves him.'

b. [Ø_i henḍatiya jnāpaka bandu] rāma_i vihvalanādanu.
Ø_i wife's remembrance having-come Rama_i went berserk
'Remembering his wife, Rama went berserk.'

Finally, in the Russian DAT-NOM construction, the dative NP rather than the nominative NP controls the possessive reflexive form.

(17) Russian

V svojej gruppe Ivanu nraivitsja Maša bol'se vsej.
in self's group Ivan-DAT like Masha-NOM best
(Lit.) 'In self_i's/*self_j's group Ivan_i likes Masha_j the best.'

All these cases point to the conclusion that the absolutive and the nominative do not always mark a syntactically definable subject. Schematically represented, the situations look like the following:

(18) a. Ergative languages

ABS	V	intransitive sentence
ERG	ABS V	transitive sentence
↑		
subject		

b. Accusative languages

NOM		V	intransitive sentence
NOM	ACC	V	transitive sentence
DAT	NOM	V	DAT-NOM transitive
↑			
subject			

In the framework of Relational Grammar, the dilemma caused by the DAT-NOM construction is resolved by means of a notion called 'Inversion' (see Perlmutter 1979). Inversion makes the original subject the 'derived' indirect object; i.e. 1>3 demotion, which entails the promotion of the direct object to the subject. Thus, in the DAT-NOM construction, the dative NP functions like subject with regard to certain phenomena, while the nominative NP assumes the status of 'surface' subject, receiving the nominative marking and triggering verb agreement where relevant.⁵⁾

The Inversion solution is unsatisfactory in a number of ways. First, there is no evidence other than the dative marking which indicates that the dative NP is not a subject. Notice that the dative NP still occupies the normal subject position in word order. It is odd that the surface non-subject occupies the subject position in a surface phenomenon like word order. Similarly, there is no evidence other than the nominative marking (and verb agreement) that points to the subjecthood of the nominative NP.

Secondly, in the case of Bengali discussed above, Inversion must be stated as a demotion of subject to an oblique case. Thus, despite the parallelism in semantics between the Bengali forms and Kannada forms in (11) and (12), two different treatments must be posited. Furthermore in the case of Bengali the original object appears to remain object (see the object form in (12b)). Thus, the Inversion treatment, which identifies the case forms and grammatical relations, or something analogous leads to the violation of the final 1 law: every basic clause must have a 1-arc [subject] in the final stratum (Perlmutter 1980:211). Of course, once one frees himself from the idea of strict identity between case markers and grammatical functions, no problem arises, since, as Klaiman (1980) notes, the genitive NP functions like a subject.

Thirdly, in order to capture the similarity in the ways in which case-marking and syntactic functions disagree in the DAT-NOM construction of accusative languages and in the ergative construction in general (see (18) above), Inversion must be posited for the majority of ergative languages.

A suggestion that the DAT-NOM construction is an intransitive sentence is equally unsatisfactory.⁶⁾ For one thing, the predicates involved obligatorily take two arguments unlike intransitive verbs. For another, while the dative NP

5) In the recent framework of Relational Grammar, no notion of derivation is utilized; however, for an expository purpose I have used familiar expressions.

6) Such a suggestion was made at the Congress by Alec Marantz, who participated in the meeting as a discussant of this paper.

is associated with the general subject properties except nominative marking, the alleged subject in the nominative lacks the subject properties despite its marking.

These problems are resolved once the relationship between case-marking and the grammatical functions is assumed to be relative; i.e., there are subjects that are not marked by the nominative or absolutive and there are non-subjects that are marked by the nominative or absolutive. Of course such an assumption leaves us with the task of identifying the function of the nominative/absolutive, the problem to which we now turn.

2.4. Nominative/absolutive and viewpoint

The works by Hopper and Thompson (1980) and Tsunoda (1981) show that in those ergative languages that have split case-marking, it is always the case that when O represents an entity that is totally affected, it is in the absolutive; i.e., the ergative pattern obtains. Similarly in the tense-aspect split ergative case, the past and the perfective forms have O in the absolutive. Observations regarding these facts seem to have been made independently by a number of linguists working in Caucasian ergative languages.

Catford (1975), with quite a few interesting pairs of ergative forms and corresponding nominative forms (see (5) above), draws the conclusion that in functional ergative languages, in which the ergative construction alternates with the nominative construction, "the ergative construction underlies the relation of verb to object, the nominative construction lays more stress on the relationship between subject and verb—the activity of the subject." (40) In slightly different wording: The ergative construction directs more attention to VERB-OBJECT relation: the nominative construction highlights the verbal relation itself, and the ACTIVITY OF THE SUBJECT." (42)⁷

With regard to the association of the ergative construction with the past tense and the perfective, Catford quotes Regamey: "This association is not fortuitous: it results from the semantic character of the nominative and ergative diatheses. When we envisage a transitive action [in the past or the perfective, M.S.] with respect to the patient [i.e., our O, M.S.], we take account of the EFFECT of this action, or that which is accomplished. This action has already been detached from the agent, it has been transferred on to the patient." (46)

In other words, the ergative construction or the marking of O with the absolutive case form is typically associated with a situation where O is brought into focus. It is this kind of phenomenon that underlies the classical view that the essence of the ergative construction is the 'patient-orientedness' (cf. Wierzbicka 1981:68). And this view is consistent with DeLancey's hypothesis (1981, 1982a) that the nominative/absolutive (and verb agreement) indicates the viewpoint of a sentence from which the speaker describes a particular event or state.

⁷ Catford (1975) has been published in 1976, but our page reference is to the prepublication copy, which has been widely circulated and which has a fuller discussion than the published version.

In order to lend some support to DeLancey's hypothesis, we need to explain the nominative marking of O in the DAT-NOM construction (cf. (9)). Among the relevant languages Japanese shows the most general pattern in that all two-place stative predicates require the nominative marking on O, but hitherto there has been no explanation for this. Having seen that the absolutive marking of O is associated with the perfective, we might speculate on some connection between the stative predicates and the perfective. And it so turns out that a link between the two is not hard to find. That is, when we consider what the perfective is, we realize that it "refers to the *state* which results from the completion of the action or process" (Lyons 1968: 314: emphasis his),⁸ and that this stativity is shared by the stative predicates. We further notice that the Japanese stative predicates do not yield the resultative forms, which indicates their inherent affinity to the expression of a resultative state. To summarize then, the stative predicates and the perfective forms share a great deal of semantic similarities, including the attention focus on the connection between O and the predicate.

Trask (1979) also sees the connection among the perfective, stative, and ergativity in his discussion of what he calls Type B ergativity, which, among other characteristics, has the tense-aspect split in the fashion described at the beginning of this section. Trask's discussion further touches on the correlation between the ergative construction and the possessive construction in this type of languages, suggesting that "Type B ergativity should be correlated with the absence of a verb *have*, or to put it positively, with the presence of dative-subject or locative-subject constructions." (398) Indeed, this is precisely our point, and the DAT-NOM pattern of accusative languages exemplified in (9) should be understood in the same light as the ergative construction in which O is a point of view.

Our earlier discussion on the syntax of the DAT-NOM construction (section 2.3.) also underscores the similarity between this construction and Trask's Type B ergative languages in that ergativity is not "deep" in the sense of Dyrbal. (Trask, 389)

Returning to DeLancey's hypothesis, it also says that verb agreement too is correlated with viewpoint, and this is consistent with the nominative marking in the DAT-NOM construction. In those languages that have verb agreement, verbs agree with nominative NP's in (9). Thus the traditional view that verb agreement is a property of a subject must be modified just as the view that the subject is what is in the nominative case.

One problem with DeLancey's hypothesis has to do with the double nominative sentences like Japanese (7) and (13b) and Korean (8) and (13a), which would force us to conclude that they have two points of view. Certainly this is an uncomfortable conclusion. However, one consolation is that, as noted earlier, there are not many languages that have two nominatives in a basic sentence type, and

⁸ Lyons is referring to the Greek perfective, but Vogt, as quoted by DeLancey (1982a: 175), makes a similar observation concerning the Georgian perfect.

this rarity reflects the marked nature of sentences with double viewpoints.⁹⁾

Furthermore, there appear to be some instances where sentences with ambiguous viewpoint arise. DeLancey (1982a) discusses a conflict in NP marking and verb marking in Georgian, and his summary reproduced below shows that while in the imperfect and the perfect, NP marking and verb marking align, the aorist involves conflicting marking—NP marking indicating that O is a viewpoint, and verb marking indicating that A is a viewpoint.

(19)

	<i>Imperfect</i>	<i>Aorist</i>	<i>Perfect</i>
NP marking	NOM-ACC	ERG-ABC	ERG-ABS
Verb marking	NOM-ACC	NOM-ACC	ERG-ABS

Just as the above situation in the Georgian aorist can be considered as a transitional point in the shift of viewpoint along the tense-aspect dimension, the Japanese and Korean NOM-NOM pattern can be understood to reflect a transition in the shift of the viewpoint along the transitivity dimension; i.e., the gradual shift of viewpoint is reflected in the changes of the pattern: NOM-ACC → NOM-NOM → DAT-NOM.

2.5. Viewpoint and other pragmatic notions

The hypothesis that the nominative/absolutive correlates with viewpoint still leaves a large number of issues unresolved. For one thing, the relationship between viewpoint and syntactic subject must be explicated. In the nominative languages, a viewpoint is normally selected as a syntactic subject, but as seen in this paper, in the DAT-NOM construction and the majority of ergative languages, they do not coincide. This difference must be somehow explained.

In the works of Zubin (1979) and Van Valin and Foley (1980), the nominative/absolutive is said to mark the "focus of interest," which undoubtedly is one of the elements that determine the placement of viewpoint. Zubin supports his hypothesis by demonstrating in reference to the German nominative that the nominative NP's fulfill an important discourse function; namely, discourse cohesion is achieved by the use of the nominative case. Van Valin and Foley define what they call "pragmatic peak" as "the most salient NP in a clause in terms of discourse prominence and speaker's focus of interest," and draw a parallel between the discourse function of the German nominative and that of the absolutive in Dyirbal.

Although the nominative and the absolutive are undoubtedly correlated with the discourse topic, direct connection as suggested by Zubin, Van Valin and

9) Catford (1975) talks about double nominative constructions in a number of Caucasian languages but they seem to be a derived pattern involving compound verb-phrases. What is interesting about them in connection with our discussion is that verbs agree with both A and O, the situation which Catford indicates in the format $S^N \quad O^N \quad V \quad V$.

This pattern is to be contrasted with the ergative alternate pattern of $SE \quad ON \quad V \quad V$. (S^N , O^N , SE refer to the NOM A, the NOM O, and the ERG A in our system.)

Foley needs to be avoided. For in those languages like Japanese that have a distinct topic construction, not only the nominative but also NP's in other cases are topicalizable, indicating that in this type of languages the nominative has a discourse function rather indirectly. Also with the DAT-NOM construction, it is the DAT NP that gets topicalized more easily than the NOM NP. Thus compared to (20b), (20c) requires a more elaborate context for its occurrence.

- (20) a. Taroo ni eigo ga waku.
DAT English NOM understand

'Taro understands English.'

- b. Taroo ni wa eigo ga waku.

TOP

'As for Taro, he understands English.'

- c. Eigo wa Taroo ni waku.

TOP

'As for English, Taro understands it.'

The above point relates to the notion of topicworthiness, which is also discussed in connection with the O-orientedness in ergativity. (Cf. Wierzbicka 1981, Plank 1979). But, as the situation in (20) shows, the notion of viewpoint must be distinguished from that of topicworthiness. The naturalness of (20b) over (20c) certainly comes from the topicworthiness of Taro as compared to English.

Just as in the case of defining the term subject, many overlapping concepts such as viewpoint, focus of interest, empathy, topicworthiness, saliency hierarchy have been discussed in connection with the topics covered in this paper, but they need to be clearly delineated before we come to a full understanding of the function of case-marking. The difficulty is caused by the fact that their linguistic manifestations tend to converge on the same element in a sentence. However, by examining areas in which there are displacements in the distribution of various factors, as we did in this paper, we will be able to gain much insight into them. Recent studies in the ergative construction and related phenomena attest fruitfulness of such an endeavor.

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Rôle des représentations métalinguistiques en syntaxe

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Le présent exposé se fixe un triple but: (1) définir le champ de la syntaxe à l'intérieur de la recherche linguistique. Pour cela, il est nécessaire d'explicitier les objectifs que s'assigne le linguiste et de poser les fondements théoriques de la recherche; (2) montrer de façon succincte comment sont construits les outils techniques (entités, catégories, opérations) avec lesquels on travaille, et décrire quelques-uns de ces outils; (3) travailler sur un domaine précis et sur un problème restreint, de façon à mettre à l'épreuve et la construction théorique et, à travers les procédures techniques, la méthodologie employée.

Je définirai la linguistique comme la science qui a pour objet le langage appréhendé à travers la diversité des langues naturelles. Cette définition est programmatique: elle assigne un objectif et, de ce fait, influe sur la théorisation et sur la méthodologie. Mais elle n'exclut pas d'autres objectifs, variables selon les finalités d'un programme, les domaines d'application, et les contraintes techniques. Le langage, activité signifiante de représentation, ne nous est accessible qu'à travers des textes, c'est-à-dire des agencements de marqueurs: ces agencements sont la trace d'opérations. Or, l'observation et des classements même rudimentaires montrent qu'il existe, par-delà la diversité des réalisations et des catégories, des propriétés analogiques stables; en bref, les langues ne sont pas irréductiblement spécifiques. Mais trop souvent, la référence au langage a conduit à l'idée que l'on pourrait utiliser un métalangage de portée universelle, qu'il s'agisse de formalismes logiques ou d'un langage génotype. Or, l'objectif n'est pas de construire une grammaire universelle, mais de re-construire, par une démarche théorique et formelle de type fondationnel, les notions primitives, les opérations élémentaires, les règles et schémas, qui engendrent les catégories grammaticales et les agencements propres à chaque langue, bref, de rechercher les *invariants* qui fondent et règlent l'activité de langage, telle qu'elle apparaît à travers les configurations des différentes langues.

Il nous faut pour cela dépasser les propriétés classificatoires et l'étiquetage, nous dégager du discours intuitif grâce à la construction d'un système de représentation métalinguistique (qui inclura la langue usuelle), construire une théorie des observables, et, à partir de classes de phénomènes (en particulier par la constitution de familles d'énoncés en relation paraphrastique) *formuler* des problèmes et construire des procédures de raisonnement. Une telle entreprise est une entreprise complexe, où l'on passe des observations à une problématique pour retourner aux phénomènes, d'où l'importance d'un discours contrôlé.

Adopter cet objectif, c'est ne pas distinguer, en principe, les délimitations entre prosodie, syntaxe, sémantique, et pragmatique. La démarcation ce qui

est représentable et régulier, et ce qui est hétérogène par rapport aux règles métalinguistiques que l'on a construites. Si l'on a affaire à des agencements de marqueurs, on a une *forme* (au sens abstrait). Cette forme étant le produit d'opérations, il nous faut simuler la relation opérations-marqueurs grâce à une construction métalinguistique. Il ne s'agit donc pas de réduire la syntaxe à un noyau arbitrairement restreint, mais de *traiter* tout ce qui est dans un domaine méthodologiquement homogène, ou dans des domaines localement homogènes que l'on peut articuler entre eux. Je soutiens que l'on peut fournir une théorie unifiée intégrant des phénomènes répartis, à l'heure actuelle, dans des secteurs séparés. Il est vrai qu'un tel objectif exige que l'on multiplie les précautions, mais il exige aussi que l'on ne restreigne pas un domaine de recherche sans donner, de façon explicite, les justifications théoriques ou techniques de pareille décision. Je plaide donc la cause d'une syntaxe définie comme une hypersyntaxe.

Mais il est vrai que la prudence s'impose: études minutieuses et étendues dans une langue donnée, puis, par des procédures formalisées, recherche du généralisable; refus de confondre phrase et énoncé, obligation de travailler à intonation constante et de prendre en compte les gloses des locuteurs quand on leur demande de construire un contexte explicite; rejet de toute confusion entre les problèmes logico-philosophiques de la référence (valeurs de vérité, référence externe, statut ontologique des individus) et la construction (non-symétrique) par des interlocuteurs de valeurs référentielles assignées à des énoncés, à travers la production et la reconnaissance de formes; d'où la nécessité de ne pas se cantonner à un univers rigide, strictement extensionnel, dont on a éliminé l'activité des sujets énonciateurs et la déformabilité caractéristique des phénomènes linguistiques.

Ainsi, autonomie de la syntaxe, si l'on entend par là qu'il existe des formes, engendrées par des systèmes structurés d'opérations dont on peut fournir une représentation et un traitement, si l'on veut dire qu'il est possible de travailler sur l'activité de production et de reconnaissance des énonciateurs, ainsi que sur le calcul des valeurs référentielles des énoncés sans s'engager dans la sémantique de la référence. Pas d'autonomie, si cela entraîne une réduction, certes inévitable dans tout projet scientifique, mais qui, dans le cas considéré, dénaturerait l'objet même de la recherche. De toute façon, l'essentiel n'est pas là: le type de recherche décrit ici n'est guère concerné par de tels débats où les termes en discussion, les objectifs et les procédures de confrontation sont rarement définis avec la clarté souhaitable.

Description de quelques opérations

1) l'opération de repérage. Nous avons vu que produire, ou reconnaître, un énoncé, c'est construire, ou re-construire, des agencements de marqueurs, qui sont la trace d'opérations auxquelles nous n'avons pas accès. Si nous appelons niveau I le niveau des opérations auxquelles nous n'avons pas accès, les agencements de marqueurs sont de niveau II et sont les représentants des opérations de niveau

I. Il nous faut donc construire, grâce à un système de représentation métalinguistique, des opérations de niveau III (on aura ainsi des représentants de représentants). Il s'ensuit que nous ne pouvons pas nous satisfaire de travailler sur des relations toutes constituées, mais que nous devons représenter les étapes de la constitution même des relations et catégories grammaticales à travers un enchaînement d'opérations.

L'observation minutieuse de langues variées et la théorisation de phénomènes en apparence éloignés, m'a amené à poser une relation fondamentale, appelée relation de repérage, construite par l'opération élémentaire primitive dite *opération de repérage*. Le concept de repérage est lié au concept de localisation relative et à celui de détermination. Dire que x est repéré par rapport à y signifie que x est localisé (au sens abstrait du terme), situé par rapport à y , que ce dernier, qui sert de repère (point de référence) soit lui-même repéré par rapport à un autre repère, ou à un repère origine, ou qu'il soit, lui-même, origine. Rien n'interdit qu'un terme soit repéré par rapport à lui-même, qu'un terme qui était repère dans une 1^{ère} relation devienne ensuite terme repéré, ou que deux termes soient dans une relation réciproque de repère à repéré. Lorsque, à l'intérieur d'un système de référence, un terme x est repéré par rapport à un terme y , l'opération fournit à x une valeur référentielle (détermination d'une propriété) qu'il ne possédait pas auparavant. J'ajoute que *terme* doit être compris dans son sens étendu, c.-à-d., tout objet construit dans le système, du moment qu'il entre dans une relation, qu'il s'agisse de notions, de relations (on a alors une relation entre relations), de coordonnées énonciatives, etc. Des opérations telles que la construction de relatives, la topicalisation, les relations inter-propositionnelles, les opérations d'anaphore, les relations de deixis, de "point de vue", comportent des opérations de repérage. En fait, il s'agit d'une opération fondamentale que l'on retrouve dans les phénomènes les plus variés. L'introduction de ce concept permet une construction théorique unifiée, un travail transcatégoriel (par ex., reliant détermination; modalité et aspect). L'idée fondamentale est qu'un objet n'acquiert une valeur déterminée que grâce à un système de repérage.

La relation de repérage est toujours binaire, et si l'on a, par ex., 3 termes, on obtient une seconde relation (binaire) sur la 1^{ère} relation. Pour construire une relation binaire, on utilise un opérateur unaire "est repéré par rapport à" ou encore "a pour repère". L'opérateur de repérage sera noté \in (qui se lit "épsilon" ou, en langue usuelle, "est repéré par rapport à"). Ainsi $\langle x \in () \rangle$ signifie que x est le terme repéré dans une relation que l'on est en train de constituer. A partir de $\langle x \in () \rangle$, on va construire un repère, disons y , d'où la relation $\langle x \in y \rangle$ "x est repéré par rapport à y". On voit donc que, quand nous parlons de repérage, nous renvoyons à la fois à la constitution d'une relation et à la relation constituée. Il n'y a pas de danger à cette confusion courante (cp. le mot *construction* qui réfère à un processus ou à un produit, c.-à-d. un bâtiment), à condition que l'on en soit conscient et que l'on sache distinguer, chaque fois que ce sera nécessaire, la relation de l'opération qui la construit.

Si l'on retourne à l'observation des phénomènes linguistiques, on s'aperçoit que les relations auxquelles on a affaire comportent toujours deux propriétés primitives qui vont permettre de spécifier la notion de repérage: tout tourne autour des propriétés de réflexivité/non-réflexivité et de symétrie/non-symétrie. Lorsqu'on a un repérage réflexif et symétrique, on a *identification* (pour être plus précis, l'opération constitutive (identifié-identifieur) est non-symétrique, mais elle produit une relation qui est symétrique. Lorsque le repérage a la propriété de non-symétrie, on a *différenciation*. Sans entrer dans le détail théorique, on peut montrer, à partir d'observations linguistiques, que la différenciation est, même si cela peut paraître paradoxal, compatible avec la réflexivité autant qu'avec la non-réflexivité. Ainsi, nous verrons plus loin que la passivation entraîne la réflexivité, sans éliminer nécessairement la non-symétrie dans la relation prédicative dite transitive. Au contraire, dans la localisation spatiale (*Il y a un livre sur la table*), il n'y a pas réflexivité.

On voit qu'à partir de l'opération primitive de repérage, nous avons construit deux valeurs fondamentales qui souvent s'entremêlent dans les réalisations linguistiques. Il existe une 3^e valeur fondamentale, ainsi qu'une 4^e valeur qui est composée à partir des trois premières, mais je n'en parlerai pas. Me fondant, une fois encore, sur les observations, j'ai été conduit à associer à l'opérateur de repérage \in un opérateur dual \ni (epsilon miroir), tel que $\langle x \in y \rangle \rightarrow \langle y \ni x \rangle$ (par ex. " x est possédé par y " \rightarrow , " y possède x "; " x est sur y " \rightarrow " y contient x "; etc.). Il s'agit là de phénomènes linguistiques bien connus, ayant fait l'objet d'analyses détaillées: l'intéressant est d'en construire une représentation munie de propriétés formelles afin d'effectuer un calcul.

Pour compléter ces notes de travail, je signale une propriété importante de l'opération de repérage en liaison avec la catégorie de la détermination. Soit un terme x dont je dis qu'il est repéré: $\langle x \in () \rangle$. Introduisons maintenant le repère y . J'ai constitué une relation $\langle x \in y \rangle$, où, sauf explicitation de ma part (à moi, énonciateur), y est construit comme repère de x , sans rien dire de plus que "il existe un repère et ce repère est y ". Ceci ne signifie ni qu'il y ait d'autres valeurs possibles pour "repère de x ", ni qu'il n'y en ait pas d'autres, mais cela signifie que, dans un cadre de référence donné, pour une détermination donnée, l'opération a construit un chemin unique entre le repéré et le repère. Nous dirons que la valeur y est unique sans plus, ou, si l'on veut "faiblement unique" ("strictement unique se dirait de " y et seulement y "). Je ne donnerai pas les fondements théoriques de cette propriété, car cela nous entraînerait trop loin. Quant au dual \ni , il a, lui, des propriétés qui varient selon l'histoire de la relation. On peut, en simplifiant, distinguer deux cas: (1) d'un côté, je peux tirer de $\langle x \in y \rangle$, par dualité, $\langle y \ni x \rangle$. J'ai alors deux formulations équivalentes (mais attention à ne pas confondre équivalence et identité). Si "Paul a le livre", alors "le livre est chez Paul"; si "Paul a un livre", alors "il y a un livre chez Paul", etc. (2) En dehors de ce cas, et sauf explication toujours possible, l'opérateur \ni ne va pas construire un chemin nécessaire entre y et seulement x , mais entre le repère y et le domaine d'objets repérés par rapport à y , auquel il est clair que

x appartient. Ainsi, dans $\langle y \ni x \rangle$, x n'est pas nécessairement unique. Comparons ici \ni et \ni : le premier se re-construit dans une relation comme "A qui est ce chapeau?" (je recherche le possesseur du chapeau). De même, si je dis "la maison est à Paul", j'exclus d'autres propriétaires. Ou encore, dans "le livre est sur la table", *table* est le localisateur de *livre*. Mais de "Paul a un chapeau", je n'en conclurai pas que c'est sa seule possession (le bon sens n'a rien à voir, ici); de "Il y a un livre sur la table", je ne peux pas inférer que "sur la table il n'y a rien d'autre qu'un livre" (Je n'insiste pas sur ces phénomènes bien connus). Ainsi, $\langle y \ni x \rangle$ peut se lire, selon les cas, soit " y sert de repère à, en tout cas x " ou bien " y sert de repère à, entre autres, x ". Ces interprétations montrent que \ni est, dans ces cas, non-déterministe.

2) Opérations constitutives d'un énoncé.

(a) Relation ordonnée, ou relation primitive: nous appellerons *notion* un système complexe de représentation structurant des propriétés physico-culturelles d'ordre cognitif (notions dites lexicales; notions grammaticales (aspectualité, etc.), et, de façon générale, toute relation entre notions. Une notion est antérieure à la catégorisation en nom, verbe, etc. Elle est définie en intension et n'est pas quantifiable. A partir d'une notion, on construit un domaine notionnel, muni de propriétés formelles (construction de la classe; construction du complémentaire linguistique; etc.).

Tout terme constituant d'une relation prédicative appartient à un domaine notionnel; ainsi, toute relation prédicative présuppose une relation entre domaines, c.-à-d. en dernier ressort, entre les faisceaux de propriétés constitutifs des notions. On appellera *relation primitive* une telle relation (partie à tout; intérieur/extérieur; etc., pour ne citer que quelques propriétés). La relation primitive est *ordonnée*, et nous parlerons de *source* et de *but* (sans connotations casuelles). Comme il ne s'agit pas ici de sémantique générale, mais de base cognitive filtrée par les cultures et les conditions d'énonciation, il n'y aurait aucun sens à dresser une liste des "sources"! Mais il existe des relations stables, parmi lesquelles celle d'agentivité (où l'agent est *source* et l'agi *but*). A dire vrai, la relation d'agentivité est complexe et s'organise sur plusieurs domaines composés entre eux (1. notion d'/animé/: humain; animé; adulte; enfant; animal domestique; inanimé; forces de la nature;. 2. déterminé/: individuable; massif; insécable; etc. 3./téléonomique/: processus finalisé; initiateur; conscient ou non; accidentel; erroné; contraint; force; instrument; etc. 4./appréciatif/: bénéfique (pour soi, pour autrui); détrimental (pour soi, pour autrui); indifférent).

Outre la relation ordonnée source et but, nous nous donnons un prédicat à deux places. (Sur les relations dites intransitives, voir plus loin).

(b) Relation prédicative (relation orientée): soit, d'un côté, une relation primitive spécifiée par un prédicat, et, d'autre part, un *schéma*, dit schéma de lexis. Ce dernier est noté $\langle \xi_0, \xi_1, \pi \rangle$, où ξ_0 et ξ_1 sont des variables d'arguments et π une variable d'opérateur de prédication. A partir de la relation primitive et du schéma, on construira le prédicat et les arguments, en distinguant un 1^{er} argu-

ment (d'ordre 0) et un 2nd argument (d'ordre 1). Ainsi, une lexis résulte de l'instanciation d'un schéma par des termes, eux-mêmes construits à partir de notions. On produit par cette opération un agencement complexe, qui n'est pas le produit d'une simple opération d'assignation, par laquelle on substituerait aux variables du schéma des termes catégorisés (en prédicat et arguments; en verbe et noms). En conséquence, la construction d'une lexis entraîne (on le verra plus loin) la constitution d'un paquet de relations entre les constituants de la relation prédicative.

Une lexis n'est pas un énoncé: elle n'est ni assertée, ni non-assertée, car elle n'est pas (encore) située (repérée) dans un espace énonciatif muni d'un référentiel (système de coordonnées énonciatives). Si nous désignons par λ une lexis et par *Sit* (pour Situation d'énonciation) le système de repérage énonciatif, on voit qu'un énoncé est le produit de l'opération $\langle \lambda \in \text{Sit} \rangle$. Une lexis est donc à la fois ce qu'on appelle souvent un contenu propositionnel (en ce sens, elle est proche du *lekton* des Stoiciens) et une forme génératrice d'autres formes dérivées (famille de relations prédicatives, d'où constitution éventuelle d'une famille paraphrastique d'énoncés). Toute relation qui a cette propriété est une lexis, qu'elle devienne un syntagme ou une phrase. Ainsi, *le livre de Pierre*; *Pierre, son livre*; *Pierre, lui, son livre*, etc., de même que *Pierre a un livre*, *Pierre, lui, a un livre*, etc. appartiennent à la même famille. *De* (ou *à dans Pierre, son livre à lui*), le possessif, ou la forme finie *a* sont les traces de l'opération de dérivation. Ajoutons qu'une lexis peut être composée avec une autre lexis, et que l'on peut construire une relation de repérage entre lexis.

Lorsqu'on a affaire à plus de 2 arguments, l'introduction d'un 3^e argument se fera nécessairement à partir de la lexis élémentaire, et l'on construira ainsi plusieurs relations dans lesquelles sera intriqué le 3^e argument. La notion d'*intrication* m'était apparue indispensable pour traiter certains problèmes linguistiques où, manifestement, les arborescences étaient inadéquates (parce qu'on constituait deux relations en parallèle et qu'un terme se trouvait donc appartenir à deux relations. Ceci se rencontre par ex. dans l'étude des causatives, ou dans la thématisation, type: *Moi, mon frère, sa maison, le toit, c'est lui qui l'a réparé*). De façon indépendante, J. P. Desclés avait découvert l'intérêt d'une telle opération dans le traitement formel; c'est grâce à lui que l'on a maintenant une définition mathématique de l'opération abstraite d'intrication (in *Opérateurs, opérations: méthodes intrinsèques en informatique fondamentale et application à la linguistique et aux bases de données*, Paris 1980). Restons dans le discours intuitif: soit 2 termes x et y et entre eux la relation. $\langle y \ni x \rangle$ Quand j'ajoute un 3^e terme z , je vais de ce fait construire une relation entre x et z (par ex. $\langle x \ni z \rangle$), entre y et z (par ex. $\langle y \ni z \rangle$), ainsi qu'entre $\langle y \ni x \rangle$ et $\langle x \ni z \rangle$. Telle que je viens de la décrire, cette schématisation nous fournit la représentation d'un prédicat de type *donner*: y a x , y établit une relation avec z de sorte que x ait z pour localisateur. Bref, nous avons construit: 1 une relation inter-sujets (pour prendre un cas privilégié) entre donneur et destinataire, 2 une relation entre donneur et donné, 3 entre destinataire (attributaire) et donné (reçu), et 4 une

relation de la transition entre 2 et 3. On voit que 1 va fournir une éventuelle valeur modale inter-sujets, et 4 combine modalité et temporalité. En comparant la forme (abstraite) de *donner* et le schéma de la relation causative (cf. plus loin), la similitude apparaît et l'on comprend que *donner* soit employé comme prédicat de causative (*Jean donne le champ à labourer à Paul*); on comprend aussi les valeurs subjectives de l'alternance *à/par* dans les causatives en français, la rétro-gradation de l'Objet en Objet Indirect; on peut mieux formuler la question de savoir s'il peut exister deux Objets, etc. Mais par manque de place, je n'aborderai pas ces problèmes.

Si l'on recherche les schémas privilégiés d'intrication de 3^e argument, on aboutit à 4 relations entre un 3^e terme et la lexis élémentaire: (1) la relation, dont nous venons de parler (peu importe sa désignation); (2) "l'applicatif" (*y* fait *x* pour *z*); (3) "l'instrumental" (*y* a *w* et il fait *x* avec *w*); (4) le locatif, qui fournit le repère spatial d'un événement, bref, qui est l'une des relations fondamentales (par généralisation, tout terme qui pourra jouer le rôle de localisateur abstrait, par delà la localisation spatiale, cette dernière étant privilégiée).

On peut, en outre, avoir moins de 2 arguments assignables (verbes dits intransitifs, procès à un seul actant): on conservera les 2 places, et l'on aura une relation réflexive (2 places distinctes, identifiables l'une à l'autre) ou en boucle (2 places qui coïncident).

Après ces généralités sur la construction d'une lexis, passons au problème de l'orientation, c'est-à-dire de la sélection du 1^{er} argument (ou complément de rang zéro). Pour orienter, il faut un point initial: celui-ci est fourni par la construction du 1^{er} argument. Dans une relation de diathèse active, le 1^{er} argument sera construit en sélectionnant le terme source dans la relation primitive, pour instancier la place ξ_0 du schéma de lexis. Désignons la source par la lettre *a*, *b* notera le but, et $\langle () r () \rangle$ le prédicat (la position des parenthèses est sans importance; on peut aussi bien les mettre toutes à gauche ou à droite, pour la commodité de l'écriture, selon la langue étudiée. L'essentiel est de respecter les règles de traitement). Une fois *a* sélectionné, on construit une relation de repérage: *a* est repéré par rapport au second membre, donc la relation non saturée $\langle () rb \rangle$, d'où (1) $\langle a \in \langle () rb \rangle \rangle$. Or, $\langle () rb \rangle$ provient lui-même d'une opération de repérage, par laquelle *b*, but dans la relation primitive, est repéré par rapport au prédicat $\langle () r () \rangle$, ce qui me donne (2) $\langle b \in \langle () r () \rangle \rangle$ (je ne donne pas la formule duale). De (1) et de (2), on tire (3) $\langle a \in \langle b \in \langle () r () \rangle \rangle \rangle$. Mais, d'un autre côté, la relation primitive entre *a* et *b* est une relation non-réflexive et non-symétrique que l'on peut représenter ainsi: $\langle b \in a \rangle \leftrightarrow \langle a \ni b \rangle$. Ce qui permet de construire un agencement intriqué, que la représentation linéaire ne peut rendre qu'avec beaucoup de maladresse:

$$(4) \quad \langle \langle \overset{0}{a} \in \langle \overset{0}{b} \in \langle () r () \rangle \rangle \rangle \rangle$$

On peut faire 3 constatations: la première, c'est que cet enchaînement d'opérations produit un paquet de relations: *a-rb*, *a-b*, *ab-r*, *b-ar*, *b-r*, *a-r*. Ce jeu de

relations joue un rôle important dans la topicalisation. La seconde remarque est que le terme *b* est doublement repéré, et il fait partie, d'autre part, du membre de la relation prédicative qui sert de repère à *a*: *b* est le terme indispensable autour duquel s'organise l'agencement prédicatif, du moins dans celles des langues où l'on marque explicitement la non-symétrie de la relation primitive entre *a* et *b*, c'est-à-dire dans les langues à ergatif. Ainsi se trouve confirmée par la construction la position de ceux qui voient dans l'absolutif le terme dont on ne peut se passer (alors que l'ergatif peut être effacé). Nous verrons plus loin que la formule (4) nous permet de comprendre la dérivation des relations prédicatives en basque. C'est, par ailleurs, la formule (1), où l'on sélectionne le 1^{er} argument sans marquer la non-symétrie de la relation primitive, qui nous fournit la relation prédicative à orientation active d'une langue comme le français.

(c) Construction du terme de départ: dans la relation orientée que nous avons construite, nous allons distinguer un terme, à partir duquel va s'organiser la relation prédicative. Cette construction du repère prédicatif (à ne pas confondre avec le 1^{er} argument, même si, très souvent, ils coïncident ou ne sont pas distingués) peut, en simplifiant, se ramener aux trois cas suivants: (1) le terme de départ (terme distingué) est le 1^{er} argument. Ceci se notera: $\langle a \ni \langle a \in \langle ()rb \rangle \rangle \rangle$ (je choisis, pour fixer les idées, la formule qui représente la relation à orientation active en français). Un tel schéma neutralise la distinction *sans plus/en tout cas*, mentionnée plus haut. Il pose un terme à partir duquel se structure la relation orientée (nous reviendrons plus loin sur les opérations d'absorption). Si le terme de départ est complexe, rien n'empêche qu'il soit recomposé par dérivation (ainsi, *le père de Jean est à Paris* a pour équivalent *Jean son père (il) est à Paris*). (2) le terme de départ est le second membre de la relation, d'où la notation: $\langle \langle \langle ()rb \rangle \ni a \rangle \in \langle ()rb \rangle \rangle$; on sait qu'en français, on a soit un marqueur prosodique de cette opération, soit un marqueur morphématique (substitution anaphorique soit par identification stricte, donc une valeur assignée pour une place à instancier, soit par identification globale, c'est-à-dire construction du domaine des valeurs assignables). On montre aisément que dans le 1^{er} cas, on aboutit à l'assignation nécessaire de la valeur *a*, dans le 2nd cas, *a* est une valeur parmi d'autres possibles. Dans le 1^{er} cas, on a le verbe *être* et l'anaphorique *ce* (c'est *a* qui...), dans le second cas, on a le verbe *avoir* et le localisateur *y* (il y a *a* qui...), c.-à-d. un repérage non déterministe. (3) on ne distingue aucun terme. Cette équipondération entraîne le repérage en bloc de la relation par rapport au repère situationnel *Sit*. Ceci sera noté $Sit \ni \langle a \in ()rb \rangle$. Il s'agit d'énoncés de prédication existentielle avec valeur de surprise, mise en garde, transformation brusque, etc. (The baby's crying!; Il y a Paul qui mange le gâteau. Il existe en français une autre possibilité dérivée du repérage situationnel: utilisation comme repère situationnel d'un terme hors de la relation prédicative: $\langle c \ni \langle a \rangle \in ()rb \rangle$ *J'ai ma soeur qui a quitté Paris*, à côté de *Il y a ma soeur qui a quitté Paris*. En fait, on a intrication de deux relations $\langle J'ai ma soeur \rangle$ et $\langle ma soeur a quitté Paris \rangle$).

(d) Construction du repère constitutif: construire le repère constitutif c'est

construire le domaine organisateur de l'énoncé. Dans certains cas, 1^{er} argument, terme de départ et repère constitutif vont coïncider, mais ce n'est pas nécessairement le cas. Le repère constitutif n'est pas obligatoirement composé d'un seul terme (cf. japonais, coréen, arabe, français). On peut en outre montrer que le repère constitutif a toutes les propriétés formelles d'un domaine notionnel, d'où les éventuelles valeurs bien connues de contraste (on associe un domaine complémentaire), de *en tout cas*, *entre autres* (on construit la frontière, qui fait partie du domaine des valeurs assignables), etc. Ici encore, l'intéressant est que l'on retrouve par une démarche théorique un ensemble de problèmes bien connus; l'avantage, c'est que une méthodologie constructiviste donne la possibilité de conduire des raisonnements où la langue usuelle est contrôlée par les représentations métalinguistiques. On sait enfin que le repère constitutif (le topique ou le thème, selon les linguistes) doit être identifié, donc stable (c'est ce qu'on appelle parfois le donné ou l'information ancienne): on aura donc soit un nom propre (au sens *large* du terme), soit un générique (toute occurrence dans la classe est identifiable à toute autre occurrence) ou une partition sur la classe, soit une reprise.

(e) Repérage par rapport au système de coordonnées énonciatives: une lexis est repérée par rapport à un système complexe comprenant un repère situationnel-origine Sit_0 , un repère de l'événement de locution Sit_1 , un repère de l'événement auquel on réfère Sit_2 . Chaque repère comprend 2 paramètres (S pour sujet énonciateur, locuteur; T pour les repères (spatio-)temporels de l'origine énonciative, de l'acte de locution, de l'événement auquel on réfère). Ce système est minimal et peut être enrichi de façon réglée par la construction d'autres repères. La formule de repérage situationnel est donc $\lambda \in \langle Sit_2(S_2, T_2) \in Sit_1(S_1, T_1) \in Sit_0(S_0, T_0) \rangle$.

Il existe d'autre part un ensemble structuré d'opérations qui permet de construire la catégorie de la détermination et, en particulier, de traiter de la quantification et de la qualification des termes qui entrent dans une relation de repérage énonciatif. Ceci permet de ramener les catégories, à travers leurs marqueurs, à des opérations fondamentales: on peut ainsi travailler à l'analyse de phénomènes spécifiques avec un cadre commun, et de ce fait se donner quelques chances de succès supplémentaires dans la recherche des invariants langagiers. Cela permet aussi de travailler de façon modulaire et de construire une problématique. Enfin, nombre de phénomènes dits pragmatiques sont intégrés qui souvent apportent une amorce de solution à des problèmes de syntaxe.

Etude d'un cas: dérivation de relations prédicatives formant une famille d'énoncés en français.

1) Trois remarques préliminaires: (1) je me suis délibérément restreint à une langue par manque de place et pour mener à terme un ensemble systématique de procédures et ne pas parsemer l'exposé d'allusions (exactes, mais non justifiées par le raisonnement). (2) je me suis limité, à l'intérieur d'une langue, à un seul problème que je ne traite pourtant pas avec l'exhaustivité souhaitable. Que l'on garde en mémoire qu'il ne s'agit que d'un fragment illustratif. (3)

Je ne discute pas explicitement les positions des différents courants, car, là encore, il n'y a aucun sens à une confrontation allusive. Mais ce silence ne marque ni le refus de la discussion, ni l'isolement à l'abri d'un parapluie technique. Très souvent, la démarche que j'adopte montre, de façon indépendante et d'autant plus probante qu'elle est soumise à des contraintes formelles, la justesse des positions de tel ou tel. On peut montrer que S. Kuno a raison quand il soutient que la construction d'une relative implique une thématisation; je suis d'accord avec Dik sur bien des analyses; je pourrais montrer que la Grammaire relationnelle a posé de fort bonnes questions et proposé un cadre stimulant; avec un peu de place, je reprendrais tel ou tel point et je montrerais la convergence des recherches.

2) Considérations techniques sur le système de représentation:

(a) Dans ce qui suit, j'adopte une représentation linéaire, malgré sa maladresse. Tout en respectant les contraintes du formalisme, je dispose les formules de façon à me rapprocher de la disposition du français. Ceci devrait faciliter la lecture, à coup sûr pénible, de cette suite de formules.

(b) Les chevrons sont utilisés pour délimiter une expression (on l'a vu dans ce qui précède) et j'introduis, de façon non systématique, des indices afin de faciliter la lecture, rappeler l'histoire d'un agencement intriqué.

(c) Les parenthèses vides sont employées dans 3 cas: (α) dans une expression non saturée, pour représenter une place à instancier (par ex. $\langle ()rb \rangle$, ou encore $\langle a \in () \rangle$, pour prendre 2 exemples déjà rencontrés). (β) pour marquer une place vidée par déplacement du terme instanciant la place (par ex. $\langle b \in \langle ()r() \rangle \rangle \rightarrow \langle () \in \langle \langle ()r() \rangle \rangle \supseteq b \rangle$). (γ) pour marquer des domaines de valeurs liés (ainsi, si je construis la classe abstraite $()_k$ des valeurs qui valident la relation $\langle ()rb \rangle$ par instanciation de la place $()$, j'obtiens l'expression $\langle ()_k \in ()rb \rangle$, où le lien marque que, pour toute valeur i, j , appartenant à la classe, la relation sera validée.

(d) Règle d'extrapolation: soit une relation $\langle x \in y \rangle$ "x est repéré par rapport à y". Non seulement je peux en tirer par dualité $\langle y \supseteq x \rangle$ "y est repère pour x, y a x comme objet repéré", mais je peux, par une opération d'extrapolation, construire les dérivées $\langle x \in () \rangle \in y$, $x \in \langle () \in y \rangle$, $\langle () \in y \rangle \supseteq x$ (ceci n'est pas exhaustif).

(e) Règle de contraction: lorsque la composition de deux expressions entraîne l'instanciation de parenthèses vides, ou la contiguïté de 2 termes identiques, on pourra contracter le résultat. Ainsi, à partir de $\langle x \in () \rangle$ et de $\langle y \supseteq () \rangle$, j'obtiens $\langle \langle x \in () \rangle \in \langle y \supseteq () \rangle \rangle \rightarrow \langle y \supseteq x \rangle$. Mais lorsqu'une parenthèse marque une place dans une expression canonique, elle ne peut être effacée (ainsi, dans la représentation du prédicat $()_0 r ()_1$, je ne peux pas effacer par contraction l'une des parenthèses. De même, dans l'exemple cité plus haut, $\langle ()_k \in ()rb \rangle$ je ne peux effectuer aucune absorption, puisque chaque symbole marque une étape de l'opération constitutive de la classe. Ce n'est qu'en fin de parcours (fin de la

chaîne d'opérations) que je pourrai éventuellement appliquer la règle de contraction.

(f) Enfin, et ceci est un point capital, on ne doit pas considérer les symboles comme des étiquettes, une sorte de traduction terme-à-terme d'une langue dans une métalangue et inversement. Il s'agit, je le rappelle, par des procédures réglées et indépendantes, de constituer des relations où les positions, les termes constitués et les agencements ne valent que par leur histoire et les relations de repérage qui se construisent au fur et à mesure des opérations. Ainsi les marqueurs en surface proviendront d'un calcul: *je* ne sera pas le représentant de S_1 (repère de locution), mais le marqueur de l'identification de S_1 à S_0 . De même, quand j'introduis le relatif *qui* (par ex. dans $\langle c \ni a \in \langle ()rb \rangle \rangle$ *Jean a son frère qui a acheté une voiture*, pour $c=Jean$, $a=son\ frère$ $\langle ()rb \rangle = ()a\ acheté\ une\ voiture$, ce n'est que l'aboutissement d'un calcul (terme a , repéré par rapport à une propriété $\langle ()rb \rangle$, identification de a comme valeur assignée à la place à instancier). Dans un autre cas, l'opération pourra aboutir à l'anaphorique *il*. En bonne règle, il faudrait donner les règles de transfert et justifier les décisions, mais cela rendrait disproportionné le commentaire. De même, je me servirai toujours des mêmes lettres, même si le texte français donné comme illustration suppose des formules beaucoup plus compliquées. Enfin, j'ai éliminé toute représentation des opérations constitutives de l'aspect ou de la modalité, de même que je mets tout simplement *Sit*, là où il faudrait expliciter.

3) Formulaire commenté: notre point de départ sera la formule $\langle a \in \langle ()rb \rangle \rangle$, construite plus haut, et qui donne, pour le français, la relation prédicative orientée de diathèse active, pour un prédicat à 2 places instanciées.

1. 1^{ère} étape: on ne travaille que sur la relation telle qu'elle est constituée. On prend d'abord a comme terme de départ, d'où la formule déjà donnée $\langle a \ni a \in \langle ()rb \rangle \rangle$. Par contraction, on obtient $\langle a \ni ()rb \rangle$ ($a \ni$ absorbe en tant que repère prédictif $a \in$, terme repéré). Ceci se glose, terme à terme, pour $a=Jean$, $r=manger$, $b=gâteau$, "Jean, il mange un gâteau", où *il* provient de la relation anaphorique entre a et la valeur a assignée à la place vide dans la relation non saturée. Je ne reprends pas le cas où l'on prend comme terme de départ $\langle ()rb \rangle$, car une présentation correcte serait trop longue. Je rappelle seulement que l'on a dans un cas la glose "C'est Jean qui mange un gâteau" et dans l'autre "Il y a Jean qui mange un gâteau". La formule suivante est $Sit \ni a \ni \langle ()rb \rangle$: elle correspond à "Il y a Jean qui mange un gâteau!", avec un contour prosodique caractéristique. Enfin, il importe de signaler que, par contraction, on peut obtenir $\langle arb \rangle$ "Jean mange un gâteau". Il faudrait ensuite étudier le positionnement de a , qui peut passer à droite "il mange un gâteau, Jean" et, par introduction du repère situationnel, construire $\langle Sit \ni a \ni ()rb \rangle$ "il y a Jean, il mange un gâteau". Le système de représentation permet d'engendrer les formules et de leur assigner des interprétations par des règles de transfert qui ne soient pas de simples ajustements, mais une relation opérations-formules-marqueurs théoriquement satisfaisante.

11. 2^e étape: on constitue une relation complexe en introduisant un repère

autre que a , $\langle ()rb \rangle$, ou *Sit*. Nous avons, on le voit, 2 possibilités: ou bien nous prenons un terme qui n'appartient pas à la relation (désignons-le par c), ou bien nous prenons le terme b qui, lui, appartient à la relation. En outre, 2 cas peuvent se présenter: ou bien ce nouveau terme de départ est un localisateur comme l'est le repère situationnel *Sit*, ou bien son introduction entraîne la constitution d'une relation entre le nouveau terme (c ou b) et le 1^{er} argument de la relation prédicative primaire, à savoir a . D'où les formules suivantes:

A) Localisation; avec c , on obtient (1) $\langle c \supseteq \langle a \rangle \in \langle ()rb \rangle \rangle$ ce qui se glose, pour c =Jean, a =Paul, r =soigner, b =Marie, "Jean a Paul qui soigne Marie"; avec b , on obtient (2) $\langle b \in \langle a \rangle \in \langle ()rb \rangle \rangle$ "Marie a Paul qui la soigne" (on remarque la reprise anaphorique régulière *Marie-la*, sans réflexivité, correspondant à b - b , où la 2nde occurrence de b est dans une relation dominée par a).

B) Relation inter-sujets: si l'on a affaire à une relation entre termes Animés (le plus souvent, Humains), il peut se constituer entre les 2 termes une relation d'agentivité. C'est donc une relation non-symétrique, que nous allons noter Agent \supseteq Agi, et l'on parle dans ce cas de Causation. On voit que l'on a donc deux relations, puisque une 2nde relation primitive vient s'ajouter à la relation $\langle a \supseteq b \rangle$.

Introduisons c : on obtient la relation $\langle c \supseteq a \rangle$, outre $\langle a \supseteq b \rangle$. Opérons d'abord avec la relation $\langle a \supseteq b \rangle$ posée par rapport à c , c'est-à-dire en désinquant a de la relation $\langle c \supseteq a \rangle$. Cela donne $c \supseteq \langle \langle a \supseteq b \rangle \in \langle ()r() \rangle \rangle$. On fait alors passer a à droite (règle de dualité) et l'on assigne b à la place de 2nd argument. On obtient ainsi (3) $\langle c \supseteq \langle ()rb \rangle \rangle \in a$ qui se glose "Jean fait soigner Marie par Paul".

(Naturellement, dans la formule, *faire* a été réduit à sa propriété de non-symétrie. Pour être complet, il faudrait traiter de *laisser* et *voir*, de *have* en anglais, etc.). On constate que, d'un côté, la relation $\langle ()rb \rangle$ conserve son orientation, mais que, d'un autre côté, une place reste vide et que r fonctionne comme un prédicat où les 2 places coïncident (schéma en boucle). Ce sont là, nous le verrons, des caractéristiques du passif. On remarque en outre que a a été expulsé de la relation prédicative $\langle ()rb \rangle$. Pour des raisons complexes, on peut montrer que ces facteurs cumulés (désinquantification de a , rejet hors de la relation élémentaire avec, du coup, maintien de b comme seul élément distingué de la relation) entraînent et l'apparition de *par* et une valeur modale de "non-intersubjectivité" entre c et a . Si, au contraire, nous privilégions la relation $\langle c \supseteq a \rangle$, on partira de $\langle c \supseteq \langle a \rangle \supseteq \langle ()rb \rangle \rangle$. On en tire la représentation sui-

vante: (4) $\langle c \supseteq \langle ()rb \rangle \in a \rangle$. Membre de la relation $\langle c \supseteq a \rangle$, a reste intriqué et est à la fois source dans une relation et but dans l'autre; il n'est donc pas expulsé hors de la relation élémentaire, qui conserve son orientation non-symétrique. Si l'on compare à la forme schématique du verbe *donner* (cf. plus haut), on voit la parenté. a a le statut de 3^e argument et l'on a emploi régulier de la préposition *à*, avec une teneur affective due à la relation inter-sources (entre

et a). Donnons un exemple: *Jean fait charrier de gros sacs aux enfants* (cp. *Jean donne de gros sacs à charrier aux enfants*). La valeur détrimentale est claire (elle pourrait, dans d'autres énoncés, être une valeur bénéfique). On notera au passage que l'on vérifie par le jeu des représentations l'exactitude de l'analyse faite par Larry Hyman et Karl Zimmer (*Embedded Topic in French*, in *Subject and Topic*, ed. Charles N. Li, en particulier, pp. 199-200).

Introduisons maintenant b : comme tout à l'heure avec c , nous avons 2 relations: d'un côté, $\langle a \ni b \rangle$, de l'autre, $\langle b \ni a \rangle$. Commençons par la relation $\langle a \ni b \rangle$ comme précédemment. On aboutit, par une succession d'opérations déjà décrites, à la formule (5) $\langle b \ni \langle ()rb \rangle \rangle \in a$ "Marie se fait soigner par Paul". Le commentaire est le même que pour la formule (3), concernant le statut de a et la double propriété de réflexivité et de non-symétrie. Une étude détaillée des phénomènes montrerait la fécondité de la représentation. Ajoutons que la relation primitive $\langle a \ni b \rangle$ bloque la possibilité de privilégier la relation $\langle b \ni a \rangle$ (cela aboutirait à des relations contradictoires). Enfin, pour compléter l'analyse, nous remarquerons que l'on peut ne pas assigner de valeur déterminée à la place qu'instancie a dans la formule (5), d'où la représentation (6) $\langle b \ni \langle ()rb \rangle \rangle \in ()$ "M. s'est fait arrêter, duper, etc.". On sait que l'on a là un équivalent banal du passif, et que l'on retrouve cette relation causatif-réflexif dans une langue comme le hongrois, (qui, par ailleurs, n'a pas de passif) du moins à l'état de vestiges dans la langue actuelle.

III. 3^e étape: constitution de la relation réfléchie. La formule (6) implique une relation inter-sujets. Si nous éliminons cette relation, nous sommes ramenés au schéma de localisation, c.-à-d., puisque b est en tête, à la formule (2); mais l'agent ayant disparu, nous obtenons (7) $b \ni \langle () \in \langle ()rb \rangle \rangle$, qui montre que b est le lieu d'un procès qui l'affecte sans qu'il y ait de source externe. Il reste donc la propriété de réflexivité, sans agentivité externe. Par contraction, on obtient (8) $\langle brb \rangle$ "le cable s'est rompu, le sac s'est déchiré" (l'emploi du passé composé est volontaire pour éviter toute ambiguïté d'interprétation). Mais il existe une autre valeur des relations réfléchies qui, elle, a une valeur agentive indéniable (*ce moteur se nettoie à l'essence; ce tissu se lave bien*). Quelle est l'origine de cette valeur? La construction de la relation va nous permettre de comprendre le phénomène. Nous avons affaire ici à un procès effectué par un agent externe d'où la parenté avec (2). Il s'y ajoute une valeur modale (déontique; potentiel). Celle-ci est liée au fait que l'on n'a pas affaire à un agent défini ou définissable ni même individuable, mais à un membre strictement quelconque de la classe (abstraite) construite sur $\langle ()rb \rangle$.

On voit alors que la formule (6) devient (8') $\langle b \ni \langle ()rb \rangle \in \langle () \kappa \in \langle ()rb \rangle \rangle \rangle$. Si l'on partait de (2), on aboutirait à un schéma voisin. Je ne peux montrer ici comment cet agencement entraîne la déformation modale, mais ce qui est clair, sans commentaire superflu, c'est que le schéma est doublement réflexif $b-b$ et $\langle ()rb \rangle - \langle ()rb \rangle$ et que, par construction, il implique l'agentivité externe, à la différence de (7) qui l'excluait. Comme la contraction de (8') est

$\langle brb \rangle$, on comprend l'ambivalence de *la porte se ferme mal*, par exemple.

Jusqu'à maintenant, nous avons travaillé avec *a* ou *b* comme terme de départ. Mais nous pouvons sélectionner le repère situationnel *Sit* (S, T). Dans certaines langues, c'est la fonction localisatrice qui est marquée (néerlandais, danois), dans d'autres (français, suédois, allemand), c'est le paramètre *S* qui apparaît. Pour obtenir le réfléchi impersonnel, partons de la formule (8') ci-dessus, éliminons *b* de la place de départ et, conjointement, instancions par *Sit* la place ainsi vidée grâce à la construction d'un anaphorique. D'où: (9)

$Sit \ni \langle () \ni \langle ()rb \rangle \in \langle () \kappa \in \langle ()rb \rangle \rangle \rangle$. Faisons passer à gauche la représentation de la classe d'agents, et développons l'expression $\langle ()rb \rangle$. Cela donne:

Il se construit des maisons

(10) $Sit \ni () \ni \langle () \kappa \in ()rb \rangle \ni \langle \langle ()r() \rangle \ni b \rangle$.

Cette formule permet de prévoir les propriétés et les contraintes (en particulier déterminations quantitatives); une simple comparaison montrerait l'analogie avec le schéma de prédication existentielle (*il y a/il se trouve*); on a nécessairement une valeur agentive.

IV. 4^e étape: la passivation. En I, la relation est agencée autour du prédicat *r*; en II, nous avons utilisé l'opérateur \ni ; en III, la relation, par suite de son histoire, est, réflexivement, agencée autour de *r*; en IV, nous allons utiliser l'opérateur \in . Dans la formule de localisation, *b* était le localisateur d'un procès qui l'affectait. Que l'on expulse l'agent du procès à l'extérieur de la relation élémentaire, et, on l'a vu, on obtient la réflexivité ou l'assimilation à un prédicat intransitif où les 2 arguments se confondent. Le localisateur n'a plus de fonction de repère par rapport à un second terme; il devient terme localisé par rapport à une relation, d'où la formule (11) $\langle b \in \langle ()r() \rangle \rangle$. L'opération de localisation est, en français, marquée (ici) par le verbe *être* (être repéré par rapport à) et le participe passé, dont la propriété ambivalente est d'être compatible avec la réflexivité et la non-symétrie, ainsi qu'avec les valeurs aspectuelles afférentes. En français, l'agentivité peut, selon les prédicats et selon les valeurs référentielles de modalité et d'aspect, disparaître ou ré-apparaître, prouvant que réflexivité et non-symétrie coexistent, l'une ou l'autre l'emportant selon des règles claires. Le schéma de localisation permet de comprendre pourquoi on peut avoir un passif impersonnel, même avec un intransitif ou un verbe prépositionnel (il a déjà été monté au-dessus de 8000 m sans oxygène; il est alors procédé à une seconde vérification). En effet, passiver, c'est construire une relation doublement converse (changement du terme de départ; changement de l'orientation). L'introduction de *Sit* permet de fournir un second argument et d'effectuer la 1^{ère} opération; quand à la seconde opération, elle revient à construire un schéma de localisation abstraite.

A la suite des travaux de G. Rebuschi sur le basque (en particulier, thèse Paris 1982), on pourrait reprendre la formule (4) de la page 5: sous la forme $a \ni \langle b \in \langle ()r() \rangle \rangle$ elle donne la représentation de la relation active ergative (*a* \ni est marqué par l'ergatif, *b* \in par l'absolutif; on a dans ce cas un auxiliaire

qui reprend la relation non-symétrique $a \supseteq b$); sous la forme $a \in \langle b \in () r() \rangle$, elle nous donne la représentation de l'antipassif, avec 2 absolutifs et, comme le montre le parenthésage, relation serrée entre b et r ; si l'on prend $b \in \langle () r() \rangle$, cela nous donne le passif court (dans le cas de l'antipassif, comme dans le cas du passif, on a une localisation: $a \in$ d'un côté, $b \in$ de l'autre. On a donc, comme en français, un localisateur tel que *être* ou *se trouver*).

On voit, je l'espère, malgré le caractère allusif et bousculé de cette présentation, que la démarche décrite vise à affiner les observations, à rendre plus rigoureux les raisonnements, à construire un outil de représentation et de traitement qui permette de re-construire les invariants langagiers. Ma conviction est que les langues sont plus abstraites et cohérentes dans leur syntaxe (au sens large) que nous ne le pensons, mais que cette syntaxe est régie par un petit nombre d'opérations fondamentales.

Some Basic Principles of Functional Grammar

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0. Introduction; scope of the paper

Functionally oriented approaches to the analysis of natural languages seem to be rapidly gaining in popularity: Functional Sentence Perspective (Prague School) and its more recent descendants Functional Generative Grammar (Sgall et alii) and Functional Syntax (Kuno); Systemic (Functional) Grammar (Halliday); Lexical (Functional) Grammar (Bresnan), and other approaches. While recognizing the family resemblances with these different approaches (in general philosophy, sometimes in points of detail), I concentrate in this paper on Functional Grammar (FG) in the stricter sense of the approach which was developed in Dik (1978). Apart from this initial publication, the following are the most easily available sources of information on the basic properties and the further developments of this theory: Dik (1980a, 1980b), Hoekstra et al. (1981), and Bolkestein et al. (1981). A full bibliography on FG is available from Casper de Groot, Faculty of Letters, Tilburg University, The Netherlands.

1. The senses of 'functional'

FG wishes to be a theory of the organization of natural languages which is 'functional' in at least three different, though interrelated senses: (i) it takes a functional view on the nature of language; (ii) it attaches primary importance to functional relations at different levels in the organization of grammar; (iii) it wishes to be practically applicable to the analysis of diverse aspects of language and language use.

2. The functional view of natural language

In the functional view a language is regarded as an instrument which human beings use in order to achieve certain goals and purposes. These goals and purposes are taken to lie in the first place in the establishment of complex patterns of social interaction. Speakers use linguistic expressions to communicate messages to interpreters so as to change these interpreters in certain ways. The intended changes may be purely mental or emotional (as when the knowledge, the convictions, or the feelings of interpreters are modified), or they may be directed at effecting further changes in the projected practical activity patterns of the interpreters.

As is the case with other types of human instruments, it is in principle possible to study a natural language while abstracting away from the communicative purposes for which it is used. But there is **not** much point in such an

exercise, since the very essence of the instrument is lost in the abstraction process. Therefore, a functionalist approach to language is not interested in *langue* when this language cannot be used to explain *parole*; it is not interested in *competence* when this competence tells us little or nothing about *performance*. Does this imply that any distinction between competence and performance is rejected? No. Of course a basic distinction must be made between what speakers and listeners actually do in specific instances of communication (performance), and the knowledge and abilities by virtue of which they are able to do what they do (competence). There is no point, however, in dissecting competence from performance, and study it in isolation (if that would be at all possible); for competence is competence-to-perform-in-communicative-situations, in other words: communicative competence (Hymes), and the abilities which together constitute this communicative competence are abilities to act and perform in given ways, with respect to given interpreters in given settings, and in relation to given communicative goals and purposes.

Some corollaries:

a. *Functional explanations.* From the instrumental view on the nature of language it follows that FG is not content with the idea that the organization of a language is just an arbitrary given which linguistic theory is supposed to reconstruct. From the functional point of view one wishes, wherever this is possible, to understand why languages are organized as they are, in the light of the uses to which they are put: it is the functional, i.e. purpose-related properties of a language which tell us most about its essential nature. Does this mean that every structural property must somewhere find a functional explanation? No: as with all human instruments, the functional requirements put on a language leave quite a bit of leeway for alternative specifications of non-functional properties; historical developments unavoidably create rudimentary properties (properties which, once functionally relevant, have lost their functionality); and conflicts between different functional requirements may even create dys-functionality in given areas of linguistic organization.

The functional approach to language is not committed to the view that any property of a language must be functionally explainable; it is committed to the attempt at arriving at a functional understanding of the structure of a language to the extent that this is at all possible.

b. *Pragmatic adequacy.* From the functional point of view it follows that FG will strive for an optimal degree of pragmatic adequacy in its account of the grammatical organization of a language. The pragmatic adequacy of a grammar is higher to the extent that it fits in more easily with a wider, pragmatic theory of verbal interaction. FG thus wishes to describe linguistic expressions in such a way that it becomes understandable that these expressions, when used by given speakers in given settings, can communicate certain messages to given interpreters. It does not restrict the notion of grammar to isolated sentences, but would want to clarify how sentences can be integrated into coher-

ent texts; and how linguistic expressions relate to non-linguistic settings. In the long run, a functional grammar should be capable of operating naturally as an integrated component of models of verbal communication.

c. *Psychological adequacy*. From this, again, it follows that FG would like to relate as closely as possible to psychological models of linguistic competence and linguistic behaviour. Psychological models naturally split up in models of how speakers go about in constructing and formulating linguistic expressions (production models), and models of how interpreters go about in processing and interpreting given linguistic expressions (comprehension models). Generative grammars have often been said to be completely neutral with respect to this division. But a functional grammar, when it wishes to achieve pragmatic and psychological adequacy, cannot so simply dissociate itself from production and comprehension: why should a grammar be neutral as between producing and comprehending linguistic expressions, when producing and comprehending such expressions is just what the grammar is there for? A closer fit with psychological reality would seem to be reached when we conceptualize the grammar itself as consisting of (a) a production model, (b) a comprehension model, (c) a store of elements and principles which are used in both (a) and (b). In that way, it would also be easier to build a grammar of this type into models which are meant to simulate actual linguistic behaviour of speakers and interpreters, and easier to evaluate a grammar through psychological testing methods. Seen in this light, FG as so far developed more closely approximates a production model than a comprehension model. It is written in the productive 'mode', in terms of recipes for constructing linguistic expressions from their basic building blocks, the predicate-frames. This productive mode should be supplemented with an analytic mode, consisting of principles and strategies for arriving at an analysis and interpretation of given linguistic expressions.

3. The status of functional relations

Early versions of Transformational-Generative Grammar can be seen as attempts at reconstructing the organization of natural languages in terms of purely structural notions such as: constituency, categorization, linear order, and transformation (=mapping of sets of trees onto sets of trees). Functional and relational notions were given a derivative, subsidiary, or additional status. In FG this is, in many respects, just the other way round; functional notions are taken to play an essential and fundamental role at different levels of grammatical organization; the role of constituent analysis is strongly reduced; (sub)categorization plays a subsidiary role; linear order is defined in a very late stage of the production process; and transformations (in the sense of structure-changing operations) are avoided wherever possible.

Many of the rules and principles of FG, then, are formulated in terms of functional notions. Three types or levels of function are distinguished:

- (1) *Semantic functions* (Agent, Goal, Recipient, etc.) which define the roles

that participants play in states of affairs, as designated by predications.

(2) *Syntactic functions* (Subject and Object) which define different perspectives through which states of affairs are presented in linguistic expressions.

(3) *Pragmatic functions* (Theme and Tail, Topic and Focus) which define the informational status of constituents of linguistic expressions as used in given settings.

The semantic functions are coded in the predicate-frames which underlie the construction of predications; syntactic and pragmatic functions are added to the constituents of predications by later assignments. Some details of these different procedures will be outlined in section 5 below.

The structure of FG is such that every term (=expression which can be used to refer to some entity in some world) occurring in a predication has at least a semantic function, and may also receive a syntactic and/or a pragmatic function. In this way it is possible to explicitly characterize a given constituent as, e.g., the Goal, the Subject, and the Focus of a linguistic expression. Each of these functions contributes to the final overall information content of the expression; on the other hand, each of these functions has certain effects on the way in which the expression is built up and formally expressed. Thus, a direct relation is established between the functional structure and the formal expression of predications. Rules and principles of FG can be formulated in terms of functions of each of the three levels (or combinations of these), because the functional specifications are explicitly available and retrievable in the underlying structure of predications.

4. Practical applicability

FG wishes to achieve a maximum degree of practical applicability in the analysis of diverse aspects of language and language use. It is attempted to reach this goal by (i) maximizing the degree of *typological adequacy* of the theory, while at the same time (ii) minimizing the degree of *abstractness* of linguistic analyses. By typological adequacy is meant the degree to which the theory can be used to cope with the grammatical facts of languages of diverse types. By degree of abstractness is meant the distance (as measured in terms of rules, operations, or procedures) between the structures postulated for a given language on the basis of the theory, and the actual linguistic expressions of that language which are reconstructed in terms of these structures. These two notions interrelate in the following way: when the theory is too concrete in the description of a given language, it cannot be typologically adequate with respect to other languages; when the theory is too abstract, it overshoots its mark of defining the most significant generalizations over languages, and thus loses in empirical import. In other words, the theory wishes to stick as closely as possible to the actual facts as they present themselves in different languages, while still being formulated in terms of organizational principles which are applicable to any natural language without forcing, i.e., without artificially adapting any language to the theory rather than adapting the theory to the

language.

Let me give two examples which involve the principle of typological adequacy:

(a) *constituent order*. It is well known that languages differ from each other in patterns of constituent order, both in kind (VSO, SVO, SOV languages, etc.) and in degree (languages with stricter patterns vs. languages with freer patterns). Furthermore, in most languages we find different constituent orderings under different conditions. It is thus unlikely that a theory which postulates one single underlying constituent order for a given language can do justice to the variety of patterns found in that language. And it is excluded that a theory which would postulate one underlying order for all languages could achieve typological adequacy. The conclusion seems obvious: constituent order is not a 'deep' property of natural languages, and we need a level of representation at which we can generalize over different languages while disregarding the ordering pattern in which the expressions of these languages are finally realized. This is just what is provided by FG.

(b) *copula constructions*. Many languages have constructions of the form *John is ill*, *John is a painter*, etc., which seem to contain the copula verb *be* as an essential characteristic. Other languages, however, have constructions of the form *John ill*, *John a painter*, etc. with essentially the same properties. Suppose we have a theory which assumes the copula to be an essential, 'deep' property of such constructions as these. Such a theory could only be upheld by postulating the 'deep' presence of a copula, which is then deleted in languages in which it does not occur. This leads both to artificial 'forcing' and to an increase in abstractness. Much more adequate, then, is the view that the fundamentals of this construction type must be describable without mentioning a copula, which can then be introduced in those languages which do actually have it. Instead of a 'deep' property of constructions of this type, the copula is now seen as an expression device which some languages do, and others don't use in expressing predications which underlyingly can be described in the same way.

In order to restrict the degree of abstractness of linguistic analyses, the following constraints have been tentatively adopted in FG:

- *transformations* in the sense of structure-changing operations are avoided;
- *filter devices* are disallowed;
- *abstract lexical decomposition* is not applied.

The impact of these constraints can be summarized as follows: once a structure has been formed, it may be further expanded, but it may not be changed; a linguistic expression generated by the grammar may not be later rejected as ill-formed; and from the deepest level onwards, underlying representations are constructed in terms of lexical elements of the language described, rather than in terms of some abstract semantic meta-language.

It is evident that the quest for the most concrete theoretical level which is still typologically adequate requires a continuous interplay between application of the theory on as many different languages as possible, and adaptation

and modification of the theory in the light of the results of such applications.

5. The organization of a Functional Grammar

The overall structure of FG is outlined in Figure 1. Please use this as a map for the brief guided tour which now follows.

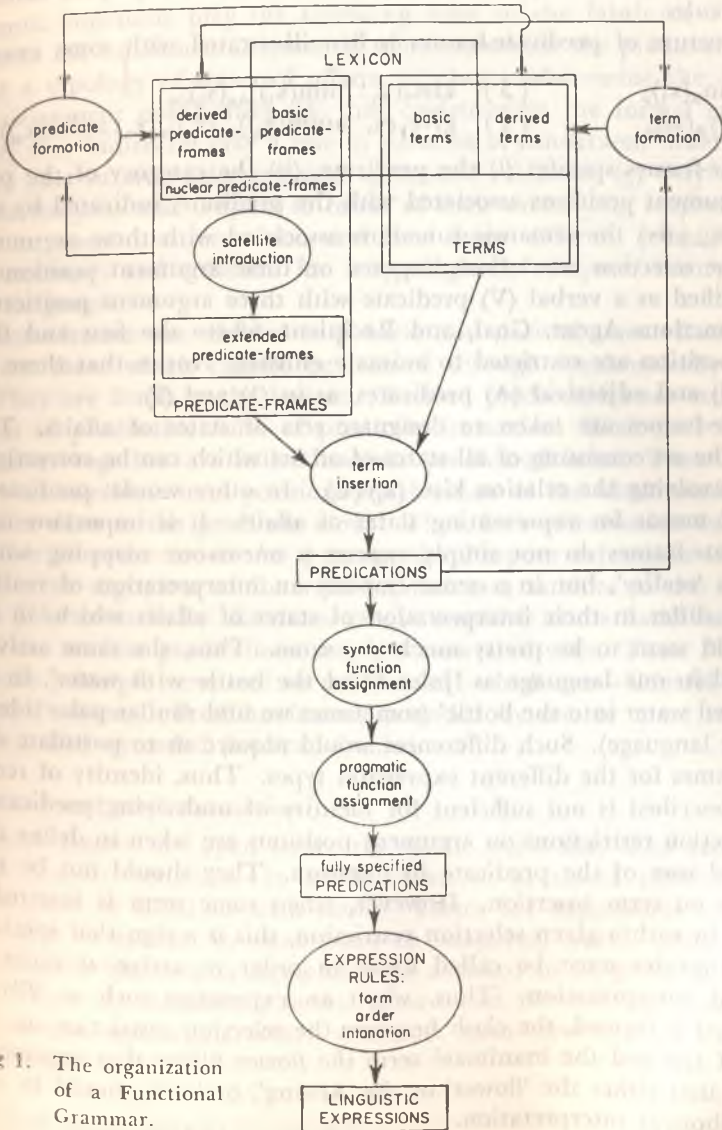


Figure 1. The organization of a Functional Grammar.

5.1. Predicates and predicate-frames

All content elements of a language are reconstructed in FG as *predicates*: expressions which designate properties or relations. Predicates, however, are not treated as isolated elements, but as *structures*. These structures are called *predicate-frames*, and every predicate is from the very start part of such a frame. Predicate-frames are the basic building blocks from which *predications* are constructed, which are finally mapped onto linguistic expressions through *expression rules*.

The structure of predicate-frames is first illustrated with some examples:

- | | |
|---|--|
| (1) man _N (x ₁) _φ | (3) kiss _V (x ₁ : hum(x ₁)) _{Ag} (x ₂) _{Go} |
| (2) silly _A (x ₁) _φ | (4) give _V (x ₁ : anim(x ₁)) _{Ag} (x ₂) _{Go} (x ₃ : anim(x ₃)) _{Rec} |

Predicate-frames specify: (i) the predicate, (ii) the category of the predicate, (iii) the argument positions associated with the predicate, indicated by variables x₁, x₂, ..., x_n, (iv) the semantic functions associated with these argument positions, (v) the selection restrictions imposed on these argument positions. Thus, *give* is specified as a verbal (V) predicate with three argument positions in the semantic functions Agent, Goal, and Recipient, where the first and the third argument position are restricted to animate entities. Notice that there are also nominal (N) and adjectival (A) predicates, as in (1) and (2).

Predicate-frames are taken to designate sets of states of affairs. Thus, (3) designates the set consisting of all states of affairs which can be correctly characterized as involving the relation *kiss* (x₁) (x₂). In other words: predicate-frames are codified means for representing states of affairs. It is important to realize that predicate-frames do not simply express a one-to-one mapping with states of affairs in 'reality', but in a sense embody an interpretation of reality: languages may differ in their interpretation of states of affairs which, in the real world, would seem to be pretty much the same. Thus, the same activity may be described in one language as 'John filled the bottle with water', in another as 'John filled water into the bottle' (sometimes we find similar pairs side by side in the same language). Such differences would require us to postulate different predicate-frames for the different expression types. Thus, identity of real world situations described is not sufficient for identity of underlying predicate-frame.

The selection restrictions on argument positions are taken to define the non-metaphorical uses of the predicate in question. They should not be taken as prohibitions on term insertion. However, when some term is inserted which does not fit in with a given selection restriction, this is a sign that special interpretation strategies must be called upon in order to arrive at some sort of metaphorical interpretation. Thus, when an expression such as *The flower kissed the girl* is formed, the clash between the selection restriction on the first argument of *kiss* and the inanimate term *the flower* filling that argument position signals that either the 'flower' or the 'kissing', or both, should be assigned some metaphorical interpretation.

The semantic functions on the argument positions relate to a fourfold division of states of affairs according to the parameters \pm Control and \pm Dynamism. A state of affairs is controlled if one of the entities involved determines the incidence of that state of affairs; it is dynamic when it involves some transition from one state to another. Thus, we distinguish Actions (+Co, +Dy), Processes (-Co, +Dy), Positions (+Co, -Dy), and States (-Co, -Dy). This typology of states of affairs is taken to be universally relevant, but not complete: further subdivisions are required in terms of more refined states of affairs properties.

Semantic functions play the following roles in the fabric of FG: (a) they characterize the fundamental semantic relations within the predication, (b) they co-define a typology of states of affairs, (c) they co-determine the Subject and Object assignment possibilities, (d) they co-determine the formal expression of terms, (e) they indirectly play a role in patterns of constituent ordering, (f) they may take part in cross-reference and agreement relations, (g) they serve to differentiate Subjects and Objects in terms of their underlying semantic function.

It is an open question which and how many different semantic functions will be necessary and sufficient for a typologically adequate theory of FG for natural languages.

Predicate-frames are divided into basic and derived ones. Basic predicate-frames are those which cannot be formed through synchronically productive rules. They are listed in the lexicon. Derived predicate-frames can be formed by means of productive rules of predicate formation. Basic and derived predicate-frames together are called nuclear predicate-frames. These can be extended with *satellites*: term positions which relate to the whole nuclear predication rather than to the predicate alone. Thus, in a construction such as: *John went home in order to see his parents*, the Purpose expression is not reconstructed as an argument of the verb *go*, but as a satellite to the nuclear predication *John went home*. Satellites can be sensitive to the basic semantic properties of the nuclear predications they are associated with. Thus, Purpose satellites are in principle restricted to predications which are +Control (i.e., to Actions and Positions). Predicate-frames extended by one or more satellite positions are called extended predicate-frames.

5.2. Terms and term formation

Predicate-frames are open structures in the sense that their term positions are indicated by variables. In order to arrive at closed predications, these variables must be filled in with *terms*. Terms are expressions which can be used to refer to entities in some (read or imagined) world. Some expressions which can only function as terms may be assumed to be given as such in the lexicon. These are called basic terms. Most terms, however, are formed through term formation operations, which construct term structures of the following general format:

$$(6) \quad (\omega x_1 : \varphi_1(x_1) : \varphi_2(x_1) : \dots : \varphi_n(x_1))$$

Here x_i is the term variable symbolizing the intended referent of the term; ω indicates one or more *term operators* (operators for Definiteness, Number, etc.); the colon can be read as 'such that'; and each $\varphi_j(x_i)$ indicates some 'open predication in x_i ', i.e. some predicate-frame of which all the term positions, except that of x_i , have been filled with terms. The best way to illustrate this is to give a concrete example. Thus, the term structure for the term *the girl who kissed the silly man* would be as follows:

$$(7) \quad (dlx_i : girl_N(x_i)_\phi : kiss_V(x_i)_{Ag} (dlx_j : man_N(x_j)_\phi : silly_A(x_j)_\phi)_{Go})$$

to be read as: 'definite single entity x_i such that 'girl' of x_i such that 'kiss' of definite single entity x_j such that 'man' of x_j such that 'silly' of x_j '.

The term can be seen as providing a task for the interpreter: he must try to identify some entity x_i which is progressively defined in terms of properties which are 'stacked' onto each other. Each open predication (also called *restrictor*) further restricts the set of potential referents from which the intended referent is to be drawn.

Notice the following points about this treatment of term structure:

- the different restrictors are given the same status in underlying term structure, but they may be formally realized in different ways: as the nominal head of a term, as an adjectival or other type of modifier, as a participial construction, or as a relative clause. The differentiation of these types is left to the expression rules.
- restrictors may themselves contain terms. Thus, term formation is a recursive operation.
- the order in which the elements are given in term structure is intended to reflect the semantic relations, not the linear order in which they will be mapped into linguistic expressions. Thus, (a) term structure is taken to be formally identical for different languages, and (b) expression rules will have to specify the linear ordering patterns in which terms can be realized in a given language.

Term structures can be inserted into the argument and satellite slots of predicate-frames. If such insertion is applied to all the open slots of a given predicate-frame, the result is a (closed) predication.

One further operation, not indicated in Figure 1, is the specification of *predicate operators*. These are operators for such categories as Tense, Aspect, Negation, etc. These categories are taken to operate on the predicate. But since a predicate is necessarily a structure (necessarily part of a predicate-frame), predicate operators automatically take the whole predication in their scope.

Let us now illustrate the structure of predications by means of the following example:

- (8) The girl who kissed the silly man didn't give the book to the tall boy.
 (9) Neg Past give_v
 $(dlx_i : girl_N(x_i)_\phi : Past \ kiss_V(x_i)_{Ag} (dlx_j : man_N(x_j)_\phi : silly_A(x_j)_\phi)_{Go})_{Ag}$

$$(dx_k : \text{book}_N(x_k)_\phi)_{Go}$$

$$(dx_1 : \text{boy}_N(x_1)_\phi : \text{tall}_A(x_1)_\phi)_{Rec}$$

Note the following points:

- each predication contains just as many occurrences of content elements (predicates) as are found in the linguistic expression described. Thus, no deletion of content elements (a structure-changing operation) is required.
- FG makes a neat distinction between the content elements and the form elements in the structure of linguistic expressions: the former are represented by predicates; the latter come out as the expression of several types of functions and operators which together co-define the formal structure of predications.

5.3. Syntactic function assignment

Many languages have means of presenting the states of affairs designated by predications from different perspectives. Compare:

- (10) The girl kissed the boy
 (11) The boy was kissed by the girl

These two linguistic expressions are taken to be based on the same underlying predication, and thus to designate the same set of states of affairs. The difference is taken to be that the state of affairs is presented from the point of view of 'the girl' in (10), and from the point of view of 'the boy' in (11). Such differences as these are accounted for by defining different possible assignments of the functions Subject (primary vantage point) and Object (secondary vantage point) to the terms of underlying predications. The underlying representations for (10) and (11) would thus differ in the following way:

- (10') Past kiss_r $(dx_i : \text{girl}_N(x_i)_\phi)_{AgSubj} (dx_j : \text{boy}_N(x_j)_\phi)_{GoObj}$
 (11') Past kiss_v $(dx_i : \text{girl}_N(x_i)_\phi)_{Ag} (dx_j : \text{boy}_N(x_j)_\phi)_{GoSubj}$

The different assignments of Subj and Obj function will trigger the expression rules so as to result in (10) and (11), respectively.

In a similar way, Object assignment is used to differentiate between such constructions as:

- (12) The girl_{AgSubj} gave the book_{GoObj} to the boy_{Rec}
 (13) The girl_{AgSubj} gave the boy_{RecObj} the book_{Go}

Languages may differ from each other in the following ways:

- (a) with respect to the question whether they make any use of the Subj and Obj assignment possibilities at all. Many languages do not have oppositions of type (12)–(13), and thus have no need for alternative Obj assignment. And quite a few languages have no opposition comparable to (10)–(11), and thus need no alternative Subj assignment. Thus, Subj and Obj are not taken to be universally relevant functions. The Subj and Obj assignment procedure is taken to be universal, however, in the sense that, if a language decides to make use of its

services, it will do so according to certain general principles.

(b) with respect to the degree to which differences in perspective can be systematically effected through Subj and Obj assignment. Some languages go much further in this than other languages. The variation across languages has been taken to be describable in terms of the following Semantic Function Hierarchy:

(14)		Ag		Go		Rec		Ben		Instr		Loc		Time
	Subj	+	>	+	>	+	>	+	>	+	>	+	>	+
	Obj			+	>	+	>	+	>	+	>	+	>	+

This schema expresses that Agent terms are the first candidates for Subject assignment, then terms with Goal, Recipient etc. function. Similarly for Object assignment with respect to Goal, Recipient, etc. Languages for which Subj and Obj assignment are relevant are taken to possess an initial subsegment of (14) as defining the possibilities for implementing these operations.

The assignment of a syntactic function to a term has a number of consequences for the formal expression of the underlying predication. Typically these consequences consist of a selection from among the following recurrent properties: (a) neutralization of the expression differences associated with the underlying semantic functions, (b) a marking of the predicate for different 'voices', (c) the incidence of agreement relations between the predicate and the Subj, or the Subj and the Obj, (d) special positions for Subj and Obj terms in the linear ordering of constituents.

Thus, both the accessibility conditions and the expression devices for Subj and Obj function can be formulated in rather general terms. Note that so far no syntactic function corresponding to 'Indirect Object' has been assumed to be required in FG. Indirect Objects are reconstructed as either pure Recipients (as in (12)) or Recipient-Objects (as in (13)).

5.4. Pragmatic function assignment

Even when constituents are equivalent as to semantic and syntactic function, their actual realization may differ in order, intonation, and/or in form. I mean such differences as between:

- (15) a. JOHN doesn't like that book
 b. John doesn't like THAT BOOK
 (16) a. John doesn't like THAT BOOK
 b. THAT BOOK John doesn't like

Although these expressions designate the same set of states of affairs, from the same perspective, they differ in their appropriateness with respect to given configurations of the pragmatic information of speaker and interpreter.

These differences are therefore taken care of by means of differential assignment of pragmatic functions to the constituents of underlying predications. FG distinguishes pragmatic functions external to the predication, and internal

to the predication. The external pragmatic functions Theme and Tail are used in accounting for such structures as:

- (17) *That book of yours, she doesn't seem to like it, my sister*
 Theme Predication Tail

The Theme presents a certain universe of discourse about which the ensuing predication is going to predicate something relevant; the Tail presents afterthought specification or modification of information contained in the predication. Within the predication proper, the pragmatic functions Topic and Focus are distinguished. Topic marks that constituent about which the predication predicates something in the given setting; Focus marks that constituent which, in the given setting, presents the most salient information. Multiple assignments of Topic and Focus function may be required; and the Focus function must be subdivided into several sub-types (cf. Watters 1979). Suppose we have the following question-answer pair:

- (18) Q: Who was kissed by the girl?

A: THE BOY was kissed by the girl

(18A) will now get the following underlying representation:

- (19) $\text{Past kiss}_V(\text{dlx}_i; \text{girl}_N(x_i)_\phi)_{\text{AgTop}}(\text{dlx}_j; \text{boy}_N(x_j)_\phi)_{\text{GoSubjFoc}}$

Pragmatic functions thus specify the structuring of the information contained in a predication with respect to the pragmatic setting in which it is used, and co-determine the formal expression of the predication. They thus serve to explain formal differences between linguistic expressions in terms of the contextual and situational settings in which they can be used.

5.5 Fully specified predications

After pragmatic function assignment we have reached the level of fully specified predications. These are taken to contain, on the one hand, all the information required for determining the semantic content of the expression concerned, and, on the other hand, everything needed for triggering the expression rules which will map the predication onto an appropriate form. Notice that the fully specified predications of different languages will be, not necessarily identical, but quite similar in structure, except of course for the actual content elements (the predicates), which are necessarily language-specific. The underlying predication thus abstracts to a high degree from those properties of languages which, in the end, give them such widely divergent outlooks. Those properties, then, are taken to reside in the expression component, and not in the underlying organization of the grammar.

5.6. Expression rules

Expression rules now apply which take fully specified predications as input and deliver linguistic expressions as output. In order to achieve this, these rules

must determine (i) the form which constituents of the predication are going to take, given their functional and structural characteristics within the predication, (ii) the order in which the constituents can be realized, given their functional and structural properties, (iii) the prosodic properties of the linguistic expression, to the extent that these are not determined by intrinsic properties of the basic predicates.

For detailed examples of expression rules which determine the form of constituents I refer to Dik (1980a). Here I just give an informal sketch of the effect of such rules on a predication such as (19). The operators 'd' and 'l' are mapped onto *the* and ϕ , respectively. 'Ag' is mapped onto the preposition *by*, and 'Top' is expressed by: 'absence of prosodic prominence'. This brings us to the unstressed Agent phrase *by the girl*. 'Subj' is mapped onto ϕ , and 'Foc' is expressed by 'prosodic prominence'. This leads to the stressed phrase **THE BOY**. The correct form of the verbal predicate is determined as follows. When Subj is assigned to Go, a marker 'Pass(ive)' is added to the verb. *kiss*-VPass is then mapped onto *be-kiss* Vpast participle, where the latter is further specified as *kissed*. Past-*be* is in this case mapped onto *was*, because the Subject is singular. The operation of these rules thus gives us:

(20) (was kissed) (by the girl) (THE BOY)

This string is as yet unordered, and will now be assigned an order by means of *placement rules*, which carry the constituents to certain linear positions (alternatively: assign them positions with respect to each other), subject to a number of general and language-particular conditions.

The actual ordering patterns found in a language are taken to be the resultant of different forces, which in part counteract each other and thus require compromise solutions. These forces are, roughly:

- (a) Place constituents with the same syntactic function in the same position;
- (b) Place constituents with a pragmatic function in a 'special' position;
- (c) Order constituents in such a way that they get more and more complex toward the end of the linguistic expression.

Forces (a) and (b) are accounted for by assuming that each language has one or more 'functional patterns' of the following general format:

(21) Theme, Pl (V) S (V) O (V), Tail

in which V, S, and O indicate possible positions for Verb, Subject, and Object, respectively, while Pl indicates a special clause-initial position which is used for special purposes, as follows: (i) to accommodate constituents which must occur in that position (e.g. question words, subordinators, relative pronouns), (ii) to accommodate constituents with either Topic or Focus function.

Let me illustrate these principles with some Dutch examples. Dutch has at least two distinct functional patterns, as follows:

- (22) main clause Theme, P1 Vf S O Vi, Tail
 subordinate clause: P1 S O V

Consider the following examples:

- (23) Het meisje heeft de jongen gekust
 the girl has the boy kissed 'The girl has kissed the boy.'

Here, the Subject has been placed in P1 position. This is very often the case, since the Subject will most often have Topic or Focus function. The Subject position thus remains empty.

- (24) Waarom heeft het meisje de jongen gekust?
 why has the girl the boy kissed

In this case, the question word *waarom*, which has intrinsic Focus, has gone to P1 position, and therefore the Subject ends up in the S position after the finite verb.

- (25) DEZE JONGEN heeft het meisje gekust
 this boy has the girl kissed

This sentence is ambiguous: either DEZE JONGEN is Subject, or it is Object. In either case, it is in initial position on account of its Focus function; and *het meisje*, as either Object or Subject, is in its proper pattern position.

- (26) (Ik geloof) dat het meisje de jongen gekust heeft
 I believe that the girl the boy kissed has
 'I believe that the girl has kissed the boy'

In this case the subordinator *dat* is in P1 position, and all the other constituents are in pattern position, as defined by the functional pattern for subordinate clauses.

Force (c), finally, deals with cases in which we find constituents in positions other than expected on the basis of their functional properties. In many cases such unexpected orderings can be understood in terms of the complexity of the constituents involved. Compare, for instance:

- (27) a. That you can't come is a pity
 b. It is a pity that you can't come

Although the clausal Subject can occur in initial position, as in (27a), construction (27b) will in many conditions be preferred. This is because complex constituents such as these tend to prefer a position later in the clause. A number of such preferences can be formulated in a language-independent way, and can then be taken to interact with the functional patterns and the P1-rules, so as to co-determine the variety of constituent ordering patterns actually found in a given language.

6. Conclusion

Since its original formulation in Dik (1978), the theory of FG has been applied to a variety of problems in different languages. Several extensions, specifications, and modifications have been proposed. Lack of space prevents me from discussing these various developments in the present paper. The interested reader is invited to consult the literature on FG for further information.

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Word Grammar

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1. Introduction

Linguists are probably somewhat better at predicting grammaticality judgments than at predicting the future, but some fairly clear trends seem likely to dominate the development of syntax during the 1980's. Transformational grammar will be less dominant than it was during the 60's and early 70's, though for a variety of reasons trace-theory, (or whatever it has turned into in five years' time) may still be the only cohesive 'school' to which a large number of theoretically inclined linguists pay allegiance. There seem to be four main alternative strands of work in syntax, outside TG. One is the relatively theory-neutral investigation of universals in syntax, such as the work on grammatical relations carried out by linguists like Keenan, Comrie and Plank¹). Another is the search for functionalist explanations of syntactic phenomena²). A third isn't yet so much in evidence, but I very much hope it will materialise: a serious interest in syntactic theory as part of the sociolinguistic study of language which has been one of the main developments in the linguistics of the past ten years³). And finally there is the building of alternative models of linguistic structure, which will be able to take advantage of some of the things we have learned from the other three strands of work, plus the work on TG. This strand is already a reality, though some linguists may not be aware of the extent of this reality—on my latest count there are no fewer than sixteen non-transformational theories of syntax⁴), and no doubt there are others which I haven't come across yet.

I personally welcome all these strands, and hope they will all develop well during the 80's. I shall be disappointed in 1990 if by then the different strands

1) e.g. Keenan & Comrie (1977), Plank (1979).

2) e.g. Givón (1979).

3) See references in index of Hudson (1980b).

4) Theories in alphabetical order; the references are to the most accessible accounts, which may not be by the originator of the theory concerned: Arc-Pair Grammar (Postal & Pullum 1978), Cognitive Grammar (Lakoff & Thompson 1975), Corepresentational Grammar (Kac 1978), Daughter-Dependency Grammar (Hudson 1976, 1980a, Schachter 1978, 1981), Functional Grammar (Dik 1978), Integrational Grammar (Eisenberg 1980), Lexical-Functional Grammar (Kaplan & Bresnan 1981), Lexicase Grammar (Starosta 1979), Montague Grammar (without transformations) (Dowty 1980), Natural Generative Grammar (Bartsch & Vennemann 1972), Operational Grammar (Dahl 1977), Phrase-Structure Grammar (augmented) (Gazdar 1981), Realistic Grammar (Brame 1978), Stratificational Grammar (Lockwood 1972), Systemic Grammar (Hudson 1971, Berry 1975), Tagmemics (Jones 1981).

(including the successor to trace theory) haven't started to get much more closely intertwined than they are now; and I shall be especially disappointed if linguists are still offering alternatives to transformational theory as though no other such alternatives had ever been offered.

My purpose in this paper is to introduce another non-transformational theory of language structure, which I shall call 'word grammar' for reasons that will soon become obvious. I don't expect this theory still to be intact in 1990, since I know from past experience that it's hard to get more than a small part of a general theory right. On the other hand, I don't feel linguistics is just going round in circles or just changing theories in the way that dress-designers change their fashions. I feel that it is possible in the 1980's to produce a theory which is much better than anything that could have been built without the developments of the last ten years, and I am certain that word grammar is a great improvement on the two theories that I advocated in print during the 70's (systemic grammar and daughter-dependency grammar). I hope it is also better than at least some of the other theories available at present, though there are inevitably a lot of points of similarity between it and virtually any of the other theories.

The main attraction of word grammar, for me, is that it presents linguistic facts in a format which can be used equally well for non-linguistic facts. For example, in an account of the theory on which I am working⁵⁾, I have included a formalised analysis of what I know about traffic lights (e.g. where they stand, what they consist of, what they do); and in this analysis I use the same notation as in the description of English morphology, syntax and semantics. For those interested in the uniqueness of language, this approach has the advantage of allowing us to be relatively sure that if we find differences in our analysis between linguistic and non-linguistic structure, these reflect differences in the phenomena themselves. In contrast, if we assume in advance that linguistic structure is *sui generis* and needs a unique notation for its analysis (as in transformational theory), then the properties we find in language which look unique may reflect our own assumptions rather than the facts. My own conclusion is that some parts of syntactic structure are indeed unique to language, but that in most respects linguistic structures are very similar to non-linguistic ones.

The theory is called 'word grammar' because it takes the word as the basic unit of syntax and semantics. In fact, the word is the *only* unit at these two levels, in the sense that all generalisations in the grammar apply to words, either as individual lexical items or as members of larger word-classes. The grammar generates structures by providing 'templates' against which the parts of a structure may be matched, and these templates define the notion 'possible word'. Readers accustomed to phrase-structure grammar, and other theories based on the notion of constituent-structure, will no doubt be sceptical about the chances of using a theory without phrases and clauses to handle the complexities of syntax. However, it has in fact proved possible to write an insightful grammar for English which covers such demanding phenomena as relative

5) Hudson (in preparation).

clauses, comparative constructions and unbounded leftward displacement, and, moreover, which generates complete semantic structures along with the syntactic ones⁶⁾.

2. The nature of sentence-structure

Any sentence is structured on three levels which are purely linguistic: semantics, syntax and form (which may be either phonological or graphological). In addition to these levels of structure there are other types of cognitive structure for the sentence—a variety of 'phonetic' structures reflecting its articulatory and/or perceptual properties, and a similar variety of 'meaning' structures in which its interaction with the extralinguistic context is reflected. I shall assume, for simplicity, that these other structures are related to the properly 'linguistic' structures, but are not themselves properly linguistic; but I have argued at length elsewhere that such distinctions are somewhat misleading⁷⁾. (Since both the phonetics and the context are relatively concrete, and since syntax is related to either mainly through phonology and semantics respectively, it follows that syntax is the most abstract of the linguistic levels—the level at which sentence-structure is furthest removed from 'the world'⁸⁾.) The aim of the grammar, then, is to generate structures which show how linguistic expressions are structured on the three linguistic levels of form, syntax and semantics—and of course to relate the three types of structure to one another.

This brings us to the theory of Word Grammar itself. One of the advantages of basing grammar on the word is that this offers a very easy way to relate the three linguistic levels to each other, since a word is a combination of a form, a meaning and a syntactic specification. Many theories present sentences as having a number of separate structures, one (or more) for each of the three levels, but the problem then arises of how to map these structures onto one another. For Word Grammar, however, this problem doesn't arise, since the job of the grammar is to put words together into sentences, and each word already consists of three linked structures. Of course there are important interactions between the levels which have to be captured by means of some kind of 'rule' (such as the regular patterns of inflectional morphology), but these can all be stated with reference to the word, and as far as sentence-structure is concerned, all the contacts between the levels can be made one word at a time. I shall consider obvious apparent counter-examples to this claim in the following paragraphs.

First we need some notation. Let us represent the words in a sentence by numbers, starting with the first word as 1. Any property of a word is given as $X(n)$: Y , where n is the number-variable standing for the word, X is the name of the type of property (say, 'meaning' or 'class'), and Y is the particular property

6) Hudson (in preparation).

7) Hudson (1980b).

8) Hjelmslev (1943/1961), Lamb (1966), Bartsch & Vennemann (1972).

of the word in question. I shall call X a 'slot', n its 'address', and Y its 'filler'⁹⁾. If we assumed (counterfactually) that all the meaning of the word *ink* could be represented satisfactorily as 'ink', and that its syntactic and formal properties were exhausted by 'noun' and *ink* respectively, then we could represent the structure of *ink* in the sentence *Ink stains* as follows:

meaning	(1):	'ink'
syntax	(1):	noun
form	(1):	<i>ink</i>

One immediate advantage of this notation is that we can replace the number by a variable, and put all this information into the lexicon; so the lexical entry corresponding to the above structure for *ink* would be just the same except that it would contain *i* in place of 1 throughout. This means "there is a word such that its meaning is 'ink', its syntax is that of a noun, and its form is *ink*".

Let us return to the claim that all necessary contacts between levels can be made one word at a time. This claim is meant to be taken seriously: there is no need to postulate any items larger than words. In other words, I am advocating a version of pure dependency theory¹⁰⁾, which is not equivalent to a constituent-structure analysis¹¹⁾ because it provides no 'higher nodes' which could be analysed separately from the word-nodes¹²⁾. This should not be taken in any sense as a denial of structure in the sentence; it is just a question of what kind of structure sentences have. The appendix lists some pros and cons of the two answers to this question, but for the present I shall assume no more than words in sentence-structure (which means, in fact, that I am claiming to be able to analyse sentence-structure without referring to the category 'sentence').

To show what is possible with nothing but words, consider the following partial (and simplified) structure for *Ink stains*:

meaning	(1):	'ink'	meaning	(2):	'stain'
class	(1):	noun	class	(2):	verb
head	(1):	2	modifier	(2):	1
form	(1):	<i>ink</i>	form	(2):	<i>stains</i>

This diagram (with the levels separated by broken lines) links the two words by putting the index representing each into one of the named syntactic slots of the other. (This is exactly equivalent to a diagram with arrows joining heads to their modifiers, but it has certain advantages as the structures get more complex, and is more consistent with the semantic structures generated by word grammars.) Similar connections may be made at the semantic level (where I assume that the category 'subject' belongs—an assumption for which I present arguments in the appendix).

9) See Jones (1981) for the tagmemic terminology; cf. also Hudson (1971), McCord (1980).

10) Tesnière (1959), Korhonen (1977), Mel'chuk (1979).

11) Hays (1964), Robinson (1970).

12) Hudson (1980c, d).

meaning (1): *a*
 quality (a): 'ink'

class (1): noun
 head (1): 2
 form (1): *ink*

meaning (2): *b*
 quality (b): 'stain'

subject (b): *a*
 agent (b): *a*
 affected (b): *c*
 class (2): verb
 modifier (2): 1
 form (2): *stains*

It will be seen that I have extended the use of variables in this diagram by adding non-numerical variables *a*, *b* and *c* to represent the meanings of *ink*, and *stains* and the 'understood object' of *stains* respectively. The main point of this example, however, is that we can treat the arguments of a verb as part of that verb's structure, since it is the verb that defines the 'slots' to be filled, even if the variables that fill these slots are defined by other words in the same sentence (as *a* is defined by the word *ink*).

Given that the meaning of a whole clause in a sense is represented as the meaning of its verb, it is easy to see how larger-than-word items, such as idioms, can be dealt with. It seems to be generally true that an idiom consists of a head plus one or more modifiers, so the meaning can all be located in the structure of the head word, and the peculiarity of idioms will then lie in the fact that their modifiers make no separate contribution to the sentence's meaning (in contrast with *ink* in (3), which defines the variable *a*).

What, then, does the syntactic part of a sentence's structure consist of? The answer to this question seems quite straightforward, once we have eliminated all the factors which belong properly to the semantics (such as coreference and—in my opinion—the category 'subject') or the morphology (notably concord):

- a. word-order,
- b. class-membership, reflecting 'parts of speech' and inflectional morphology,
- c. dependency relations between heads and modifiers.

In the notation used above, word-order is shown by the numbers 1, 2, etc., class-membership by the feature in the slot labelled 'class', and dependency relations by the variables in head- and modifier-slots. These slots thus exhaust the information that a grammatical analysis needs to give about a linguistic expression at the syntactic level (except that a complete specification of class-membership may need more than one slot, and there need to be as many head- and modifier-slots for a given word as it has heads and modifiers). It can be seen, then, that in word grammar the syntactic part of a sentence-structure does not amount to very much—whereas its semantic structure is likely to be very much more complicated. Moreover, since this is a non-transformational theory there is only a single syntactic structure (in fact, just a single linguistic structure) per sentence, so the small amount of information given in each structure is not offset by a large number of separate syntactic structures for the same sentence.

To summarise the notation, the structure of a word consists of an unordered set of elements which each consist of a 'slot' and its 'filler', plus an 'address' showing what the slot relates to. The filler for the slot may be either a variable (i.e. an arbitrary letter) or a constant (e.g. a number or a class-name), and so may the name of the slot, though the simple examples given so far all have a constant as the name of the slot. This notation can be used not only in syntax, but also in semantics (and, I assume, at the level of form). We shall see some of its advantages in the next section.

3. The nature of language-structure

One of the main attractions of this type of analysis is that there need be very little difference between the formal properties of 'rules' in the grammar and those of (partial) sentence-structures. For example, as we have already seen, the entry in the grammar for the word *ink* can be same as the entry for this word in the structure of *Ink stains*, except for having a variable *i* in place of *l*. (In this respect, word grammar is similar to stratificational grammar, which uses the same format for both grammar and sentence-structure¹³.) Similarly, the entry for the lexical item *stain* can be very similar to the entry in the sentence-structure, except that the variables are defined in general terms, rather than linked to particular other words:

meaning	(i):	a
quality	(a):	'stain'
agent	(a):	b, b=subject (a)
affected	(a):	c, c=meaning (j)
class	(i):	verb
modifier	(i):	Ø or j, class (j): noun, $i < j$
lexeme	(i):	i'
form	(i'):	<i>stain</i>

This entry will need a little more explanation than the one for *ink*. Let us start by looking at what it says about the object of *stain*. There are two slots that are relevant, one relating to the semantics, the other to the syntax. The semantic slot is labelled "affected", and tells us that the filler of this slot is the meaning of the word *j*. The modifier slot tells us about word *j* that it must be a noun, that it (normally) follows that verb, and that it is optional. There is a conflict between the syntax and semantics here, since the affected-slot is shown as having an obligatory filler, while the syntactic one need not be filled at all. To cope with such cases, there is a general principle:

Principle of implicit information

If the filler of a semantic slot *x* is obligatory, it must be provided by the modifier which the grammar designates, provided this word is present; but if it is not, a filler for *x* must be provided either by some other word in the

¹³ Lockwood (1972).

sentence, as specified in the grammar, or pragmatically.

This very general rule applies to a wide range of phenomena—for example, it also helps us to deal very simply, and without deletions, with the 'understood' elements in clauses that lack main verbs (e.g. *I can* and noun-phrases that lack lexical nouns (e.g. *the red*, meaning 'the red wine').

A second point about the entry for *stain* in (4) is that it distinguishes between the word, as it appears in a sentence, and the lexeme (i.e. lexical item) on which it is based. The word is represented by the variable *i*, the latter by *i'*. This allows us to deal with inflectional morphology and its semantic and syntactic effects, since it seems clear that when such variation is regular it should be handled by a general statement in the grammar, and not recorded in the lexical entry for each item. Thus, all we give in the lexicon for *stain* is its root form and the distinctive characteristics of its meaning, without saying anything about such matters as tense and subject-verb agreement; and then we need a general statement to cover inflectional morphology.

The notation I have already introduced for both lexical entries and words in sentence structure can also be used for generalisations across words, of the kind we need to make in inflectional morphology. This can be done by leaving the form of the root as a variable, and by leaving the meaning similarly unspecified. The next entry, for example, gives all the relevant properties of finite verbs regarding their meaning, their syntax and their written form.

meaning	(i):	a
time	(a):	b, [I] $b < n$, $n = \text{'now'}$, or [II] not $b < n$
subject	(a):	c, $c = \text{meaning (h)}$
class	(i):	verb
subclass	(i):	finite
tense	(i):	[I] past or [II] present
modifier	(i):	h, $h < i$, number (h): [III] singular or [IV] plural
lexeme	(i):	i'
form	(i'):	d
form	(i):	[I] d & $-(e)d$ or [II & III] d & $-(e)s$ or [II & IV] d

The Roman numerals *I* to *IV* are used to link options, so that if [I] is put in one slot, it must also be put in other relevant ones. For instance, [I] links the semantic choice 'b precedes n' to the syntactic choice of 'past' as tense and to the formal choice of 'd & $-(e)d$ ' in the form-slot.

If a verb is irregular in its morphology, then its own entry will specify not only the form of the lexeme—i.e. its root—but also the form of the irregular variant, and this specific entry will take priority over the general entry by virtue of a very general principle¹⁵:

¹⁵ Baker (1979)—the principle is similar to the well-known Proper-Inclusion Principle of TG.

Principle of priority to the particular

If two entries in the grammar both apply to the same word, and there is a conflict between them, the more particular entry takes priority over the more general.

A third point relating to the entry for *stain*, and also to the general entry for finite verbs, is that the entry may specify syntagmatic relations as well as morphological properties. Thus, general entries do the work of phrase-structure rules, but they only introduce words whose presence can be predicted from the general properties of the head¹⁶—which excludes modifiers such as objects, whose presence is determined by the valency (in this case the transitivity) of the particular verb concerned. This is illustrated here in relation to the subject, which is defined above as a modifier of any finite verb, and therefore need not be defined separately in the entry for *stain*. In the latter entry, the subject is taken as already defined, and it is exploited in order to identify the 'agent': the filler of the subject slot is, according to (6), the meaning of a word preceding the verb (indexed as *h*), (Lack of space prevents me from going into the mechanism for dealing with deviations from this order; there are various types of deviation and each needs a somewhat different treatment.) Similarly, we can use a general entry for verbs as a whole to introduce 'circumstantials' such as reason adverbials.

Unbounded dependencies such as arise in 'wh-movement' and 'topicalisation' may appear to present problems for word grammar, since such dependencies don't seem at first sight to involve the properties of any particular word in the sentence—hence Bresnan's claim that even if governed transformations can be dispensed with, by exploitation of the lexicon, unbounded dependencies involve a fundamentally different type of problem, needing a fundamentally different solution¹⁷). There is in fact a rather simple way of handling these structures in word grammar, by means of a slot which I call the 'visitor' slot, which is present in the structure of any finite verb. This slot contains the meaning of any unattached word which precedes the verb, and (unlike other slots) any item which appears in a visitor-slot of a verb must also appear in at least one other slot, because the visitor-slot isn't otherwise related to the rest of the sentence's structure.

For example, in *Beans I like*, the visitor-slots of *like* contain variables representing the meanings of both *beans* and *I*, but each of these variables also fills at least one other slot—something like 'phenomenon' in the case of *beans*, and 'experiencer' and 'subject' in that of *I*¹⁸). The visitor-slot allows unbounded displacement by virtue of another fact: subject to certain restrictions, any modifier which follows its head may 'inherit' any items in the latter's visitor-slot which haven't yet been assigned to any other slot. Take the sentence *Beans I*

16) Cf. the distinction between basic and derived predicates in Dik (1978).

17) Bresnan (1978).

18) Halliday (1967).

know you try to like. The verb *know* is finite, so it has visitor-slots in its own right, which contain variables for *beans* and for *I*. The one for *I* is immediately assigned to another slot, the subject-slot of *know* itself, but the one for *beans* remains unintegrated when the processing of *know* is finished. The next modifier of *know* is *try*, which is again a finite verb, so it has a visitor-slot which automatically contains a variable for *you*, but it also inherits the 'unused' visitor slot of *know*, the variable for *beans*. *Try* itself has a following modifier, *to* (which I take to be a verb¹⁹), but this isn't a finite verb, so it doesn't have a visitor-slot in its own right; however, since its head (*try*) has a visitor-slot, and this contains an unused item, *to* gets a visitor-slot too, with the variable for *beans* in it. Similarly, *like* has a visitor-slot with the variable for *beans* in it, by inheritance from *to*, but this time there is another slot into which this variable may also be put, namely the phenomenon-slot for *like*, so it comes to rest and is integrated into the sentence structure.

Obviously this process can continue for ever, so it is unbounded, but the analysis differs from Bresnan's in that the displaced item hops from word to word down the dependency chain²⁰ and also from Chomsky's in that the hops are from head to modifier (i.e. in the above example from verb to verb) rather than from 'complementiser' to 'complementiser'²¹.

We have now glimpsed some of the main pieces of apparatus that word grammar uses in dealing with syntax. The main unit is the combination of a slot, an address, and a filler (e.g. 'class' and 'verb'), with all addresses and many fillers represented as arbitrary letters, or by numbers. (One of the facilities which I haven't illustrated is that the same arbitrary letter may be used in two separate entries in the grammar, which allows great flexibility in capturing generalisations.) If a slot allows more than one type of filler, the grammar will specify them by listing them together as a disjunction, but in some cases different fillers have different consequences elsewhere in the grammar; such connections can be handled by means of the roman numeral notation. The main body of the grammar consists of a set of 'entries', each of which is an unordered set of units consisting of a slot, address and filler, but some of these entries refer to particular lexical items and others refer to general constructions or other types of patterns (such as those of inflectional morphology). This greatly reduces the difference between the lexicon and the 'rules', but if the difference turns out to be more important than I presently think it is, it can be made with reference to the formal properties of the entries. In addition to the entries, however, the grammar contains a number of general principles, which control the way in which it is applied. I assume that at least some of these principles are universal to all human languages (and even apply to non-linguistic thinking), but I also assume that many of the properties of entries are universal—such as some of

19) Apparently this analysis was suggested independently by Postal (Gazdar, Pullum & Sag 1980).

20) Bresnan (1976).

21) Chomsky (1977).

the categories used, and of course the formal properties described above.

I believe word grammar is at least as plausible as any other theory of language-structure when taken as a model of psychological reality, in all the three senses of this term. It is easy to believe that people create a single integrated structure for a sentence when *processing* it—I assume that the onus of proof is on those who claim that we construct a number of separate structures. It is easy to believe that we could use a grammar of the type described here, because the information is packaged in large 'chunks' (entries) which can be extracted in toto, and relatively little computation is needed; moreover any given chunk will contain information about meaning, syntax and form, so form and meaning are immediately related as soon as the word is found. It is easy to believe that we *store* the information in this form, since cross-reference between entries can easily be made by means of variables (which means, incidentally, that entries are themselves somewhat less clearly divided from each other than I have implied). And it is easy to believe that a child could *learn* a language organised in this way, as the child's main task is to learn more and more facts about words, both individually and as members of larger classes—so there is no need to abstract larger structures and learn about their properties.

APPENDIX: Some assumptions that need justifying

In this appendix I shall explain why I have made certain controversial assumptions in the analyses presented above.

A. Pure dependency

Contrary to my earlier position²², I now believe that constituent-structure, as such, has no place in linguistic description (at any level). By this I mean that there is no need for a grammar which shows modifier-head relations to refer to syntactic or semantic units larger than the word (though it may be that phonology must refer to intonation units as well as to segments and words). I have given detailed justification for this position elsewhere²³, but I shall briefly explain three points.

(i) *Why dependency and constituency are not equivalent*, contrary to earlier claims²⁴. A dependency structure contains information about the direction of dependency (i.e. which word is head and which modifier) which is absent from a pure constituency structure; and a pure constituency structure contains at least one node in addition to the word-nodes, to which an analysis could be assigned which is independent of the analyses of the word-nodes. No such extra nodes (e.g. NP, S) are available in pure dependency theory.

(ii) *Why dependency relations should be shown directly*. Grammars and theories need to refer to modifier-head relations, without which certain types of generalisations could not be made. An obvious example of the type of generali-

22) Hudson (1976).

23) Hudson (1980c, d).

24) Hays (1964), Robinson (1970).

sation in question is the kind of cross-category statement about word order which is often made in typology (some languages generally put heads before modifiers, and other languages tend to have the reverse order; English is of the first type, with the exception of subjects, and adjectives or adverbs, which precede their heads)²⁵. Many rules refer to the dependency-chain, such as the rule for the visitor-slot, and others relating to reflexives and comparatives²⁶.

(iii) *Why constituency isn't necessary* in a dependency grammar. All the work done by means of constituency in other theories can be done with reference to dependency, given the rich system of slots and fillers described in this paper. E.g. the fact that modifiers take their position next to their head, wherever the latter appears, is explained by the very general principle that modifiers may not be separated from their heads except by modifiers of their own or of the head. Similarly, the fact that a modifier and its head form a single semantic unit is explained by the fact that the meaning of the modifier is part of the meaning of the head, in the sense that a variable representing the former appears in a slot in the structure of the latter.

B. No 'subjects' or 'objects' in syntax

The grammar for English on which this paper is based makes no reference at all to the category 'object' as such, since both direct and indirect objects can be adequately defined in terms of position and class-membership. ('Position' here refers to normal position, to which the lexical entry for a verb can refer; as we have already seen, various types of deviation from the normal position occur, such as those permitted by the visitor-slot, but these can be dealt with without undermining the basic principle that the object can be defined in terms of the position in which it would normally occur. Similar remarks apply to the subject.) This is the same view of 'objects' as I took some years ago²⁷.

As far as the category 'subject' is concerned, however, I have now changed my position²⁸. I still think that this category ought to be recognised directly, rather than derivatively, but I now treat it as a slot in the verb's semantic structure, rather than its syntactic structure. (The difference is shown formally by the kind of filler permitted: as a semantic slot, subject receives a 'meaning' as filler, not a word.) The reason for this is that the elements which most linguists would want to recognise as 'subject' are syntactically very diverse in English, but for any given verb entry, only one semantic slot will be paired with the subject (e.g. in the case of *like* it is the experiencer-slot, whereas for *please* it is the phenomenon-slot). The extreme case of syntactic diversity is that there need not be anything at all in the syntax corresponding to the subject (e.g. imperatives), but this seems to make no difference to the presence of the element in the semantic structure. Moreover, there are rules such as those for reflexives which have to refer crucially to the category 'subject', and for which it

25) Témère (1959), Vennemann (1975), Heine (1975), Hawkins (1980).

26) Hudson (in preparation).

27), 28) Hudson (1976).

again makes no difference whether the subject is overtly realised in the syntax or not. Reasons such as these seem to indicate clearly that a verb's subject should be shown as part of its semantic structure, and then related as necessary to some modifier in the verb's syntactic structure.

Because of these two analytical decisions, the theory proposed here differs significantly from several others proposed recently²⁹⁾ in not treating 'grammatical relations' as basic to syntax; but it also differs from the Chomskyan view that each such category can be given a unified, if derivative, definition within syntax³⁰⁾.

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²⁹⁾ Perlmutter & Postal (1977), Postal & Pullum (1978), Dik (1978).

³⁰⁾ Chomsky (1965).

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Idioms: An Interim Report*

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As the title indicates, this paper is a report on work in progress; we expect that it will become part of a larger work dealing in considerably greater detail with the syntax, semantics, and pragmatics of English idioms. Our purpose here is more modest, namely, to consider the place of idioms in the theory of generative grammar.

Idioms have figured prominently in the generative literature. In particular, they are commonly cited in arguments for the existence of transformations. In view of the recent spate of proposals for non-transformational theories of grammar (e.g., Brame (1978), Bresnan (1981a), Gazdar (1981), Hudson (1976)), the treatment of idioms in generative grammar warrants some attention. In what follows, we will examine some of the better known discussions of idioms, arguing that they are based on faulty (and often unstated) premises. We will then outline a proposal for the treatment of idioms, which is compatible with almost any available theory of syntax.

1. Idiom-based Arguments for Transformations

1.1. One widely repeated argument for the existence of transformations (cf., e.g., Culicover (1976; 168), Keyser and Postal (1976; 250), Perlmutter and Soames (1979; 106-9)) is based on the fact that certain idioms can appear in more than one syntactic form. For example, both the *a* and *b* sentences of (1) and (2) have idiomatic readings.

- (1) a. Pat spilled the beans.
b. The beans were spilled by Pat.
- (2) a. The cat is out of the bag.
b. The cat seems to be out of the bag.

If surface structures are generated directly, so the argument goes, then the *a* and *b* sentences must be distinct idioms. On the other hand, if there are transformations of passivization and raising, then each idiom must be listed only once. They can be inserted into deep structure in their contiguous (*a*) forms and transformationally broken up, yielding their (*b*) forms. This simplifies the

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grammar and captures a generalization. Further, since the parts of the idioms can be indefinitely far apart, as illustrated in (3), it is not possible for the grammar simply to list all the surface incarnations of every idiom.

- (3) a. The beans continue to appear to be certain to be spilled.
b. The cat seems to be believed to be out of the bag.

Thus, positing a single underlying idiom which may be transformationally deformed is claimed to be not only parsimonious but unavoidable.

Not surprisingly, this argument has been answered by some linguists advocating direct generation of surface structures (see especially Bresnan (1981b)). Briefly, the simplest answer to give is that so long as the theory contains rules of some sort relating, e.g., active and passive constructions, then these rules can be used to account for the existence of "transformed" idioms. Specifically, Bresnan and Kaplan's "Lexical Functional Grammar" posits a lexical redundancy rule to account for the active-passive relationship, and Gazdar's "Generalized Phrase Structure Grammar" has a metarule accomplishing the same thing. Either one of these mechanisms would apply to idiomatic as well as non-idiomatic constructions, so they would circumvent the argument given above. In short, the argument only shows that a grammar for English must provide some rules for capturing the relationships which have been expressed in terms of transformations; it does not show that the rules in question must be transformations. And no one ever doubted that such relationships as active/passive are rule-governed.

There is, in our opinion, a more fundamental defect in the argument in question. It is the hidden assumption that idioms are arbitrary associations between forms and meanings. Only this assumption licenses the inference from the existence of two forms for an idiom to the need for the non-transformational grammar to list them separately. On the other hand, if (as we shall argue below) the assumption is wrong—that is, if the meanings of idioms are somehow composed from the meanings of their parts—then the fact that actives and their corresponding passives have the same predicate-argument structures would lead one to expect that active idioms would normally have passive counterparts. We will return to this point below.

1.2. This same assumption of semantic non-compositionality of idioms plays a crucial role in another argument for transformations, namely that offered by Wasow (1977). Since non-transformational accounts of the phenomena dealt with there have been developed elsewhere (Bresnan (1981b), Wasow (1979)), we will not discuss this argument here. We do, however, wish to point out that it assumes that idiom "chunks" are semantically empty. It is for this reason that Wasow claimed that they undergo only "structural" rules (viz., transformations), and not "relational" rules, that is, those sensitive to what Chomsky (1965; 117) has called "the semantically significant functional notions (grammatical relations)" of (deep) subject and object.

1.3. Recently, Chomsky (1980) has offered three new arguments for the existence of transformations¹⁾ all based on idioms. We contend that each of these arguments, upon examination, proves to be based on dubious factual claims and questionable reasoning.

1.3.1. The first argument is that idioms "typically have the syntactic form of nonidiomatic expressions" (p. 149). This "would be an accident if meanings were simply mapped into formal structures. But if the relation between form and meaning is more indirect, mediated through D-structures and S-structures [i.e., deep and surface structures—TW, IAS, GN] it follows that idioms must have the form of independent (sic) generated structures." (p. 151). (Of course, an argument for D-structures and S-structures is implicitly an argument for transformations, since the mapping from D-structures to S-structures is taken to be accomplished by transformations).

The first thing to note about this argument is that there is a substantial number of idioms which do not "have the syntactic form of nonidiomatic expressions."²⁾ (4) is a partial list.

- | | |
|--------------------------|-------------------------------|
| (4) by and large | No can do, |
| trip the light fantastic | kingdom come |
| battle royal | Handsome is as handsome does. |
| Would that it were. | every which way |
| Easy does it. | be that as it may |
| Believe you me! | in short |

Thus, Chomsky's argument is based on a statistical asymmetry, and it is not clear what force this has. The asymmetry could simply be an historical accident (as Gazdar has pointed out in unpublished work), resulting from the fact that most idioms are derived diachronically from non-idiomatic expressions.

Even if it is assumed that the synchronic grammar must explain this asymmetry, it does not follow that deep structures (and hence transformations) are needed. All that is required is that idioms are normally generated by the same

1) Chomsky cautions the reader that there is some question as to whether the issue of the existence of transformations has any empirical content: "should the base generate S-structures directly or should it generate D-structures which are mapped by the rule 'Move α ' into S-structure? It is not entirely obvious that this is a real empirical question. . . . That is, it might be that these two theories are simply two ways of understanding a single and somewhat more abstract theory, yet to be properly formulated, which expresses the fundamental properties shared by these two variants" (p. 149). We will not address this issue here—but see Bresnan (1981a) and Gazdar (1981) for arguments that the question is not vacuous.

2) Chafe (1968) took the existence of such "idioms which are not syntactically well-formed" (p. 114) as an "anomaly in the Chomskyan paradigm." Just what Chafe had in mind is not clear to us. It is certainly true that such idioms receive no systematic treatment in transformational terms, but it is equally true that no one has been able to offer a systematic treatment of them in any terms. Since there is only a relatively small number of such idioms, and since they have little or no syntactic flexibility (see section 3.1 below), any theory of grammar can deal with them simply by listing them. Unfortunately, no available theory can do better than this.

rules that generate non-idiomatic expressions, and this is possible irrespective of whether the theory uses transformations. The proposals below are perfectly compatible with non-transformational theories.

1.3.2. Chomsky's second argument is based on the following claim: "there are idioms that appear at both the D- and S-structure levels, and idioms that appear only at the D-structure level. But idioms that appear only at the S-structure level are very rare; we can regard this possibility as excluded in principle, with such marginal exceptions as should be expected in the case of idiomatic constructions." (p. 152). Since the standard assumption has been that idioms are inserted into deep structures like ordinary lexical items, this asymmetry is predicted, and hence is taken as evidence for the existence of transformations.

However, a close examination reveals that the exceptions are by no means so rare or marginal as Chomsky suggests. Brame (1978) has pointed out examples like (5a), which are grammatical only if the idiom is in preposed position, as the contrast between (5b) and (5c) indicates.

- (5) a. What the hell did you buy?
- b. I wonder who the hell bought what.
- c. *I wonder who bought what the hell.

Other idioms that appear only in wh-questions are *How do you do?* and *What gives?*. There are also a number of idioms which are only possible in the passive. (6) gives some of them.³⁾

- | | | |
|-----|---|-----------------------------|
| (6) | The die is cast. | The race is run. |
| | if the truth be known | hoist with one's own petard |
| | fit to be tied | caught short |
| | born yesterday | have it made |
| | written on water | Rome wasn't built in a day. |
| | may as well be hung for a sheep as a lamb | |
| | when all is said and done | |

Finally, there are several idioms which are limited to constructions associated with other purported⁴⁾ transformations.

- | | | | |
|-----|---|------------------|-------------------------------|
| (7) | hard to take | play hard to get | (tough-movement) |
| | too hot to handle | | (complement object deletion) |
| | Is the Pope Catholic? | | (subject-auxiliary inversion) |
| | Break a leg! | Believe me! | (imperative deletion) |
| | may as well be hung for a sheep as a lamb | | |

3) It is not clear whether fixed expressions like *Children should be seen and not heard*, *A woman's work is never done*, *A good time was had by all*, or *easier said than done* should be included in this list, for while they are conventionally used only in their passive forms, their interpretations appear to be completely literal.

4) While all of the constructions illustrated in (7) have been claimed to involve transformations in their derivations, we are not attributing this claim to Chomsky.

more dead than alive

(comparative deletion)⁵⁾

It is by no means clear that it is desirable for examples like these to be "excluded in principle."

Nevertheless, the relative rarity of idioms like (5)–(7) needs to be accounted for (though not necessarily in the synchronic grammar). One might conjecture that the existence of productive rules like the passive leads to back-formation of active forms for passive-only constructions (and similarly for the other constructions in question). This would also account for the infrequency of passive-only simple verbs (e.g., *rumor*). This asymmetry could, if desired, be built into any theory (transformational or not) which has general rules for deriving passives, questions, etc., by stipulating that the evaluation metric makes such structures more "costly" unless they are derived using the general rules.

There is another important point related to the passage from Chomsky cited above. While making much of the purported absence of S-structure-only idioms, he says nothing about how D-structure-only idioms are to be handled. These are expressions like *kick the bucket* or *saw logs* which lose their idiomatic interpretations when they are deformed, as in the passive. Such "transformational deficiencies" have been a major topic of investigation by generativists studying idioms (e.g., Katz and Postal (1964), Chafe (1968), Fraser (1970), Katz (1973), Newmeyer (1974)). For the most part, it has been assumed that the transformational behavior of idioms simply had to be stipulated, either by means of exception features or through encoding in the syntactic structures. (The notable exceptions to this are Chafe and Newmeyer, who both claim that the syntactic behavior of idioms can be predicted, at least in part, on the basis of their meanings. This is a view we also adopt and expand on below). Considerations of learnability suggest that the syntax of idioms is not so arbitrary as most analyses indicate. It is evident that speakers are never explicitly taught e.g., which idioms passivize and which don't; furthermore, our rather robust intuitions in this domain cannot be the result of which idioms we have or have not heard in the passive, for there is considerable cross-speaker agreement, even on relatively rare idioms which may even be novel to some speakers. This suggests that the syntactic behavior of idioms is more systematic than has generally been assumed. Any adequate theory of idioms must address this point, and provide a principled account of the "transformational deficiencies". We will return to this question below.⁶⁾

1.3.3. Chomsky's third argument is the following: "while some idioms undergo

5) There are many other idioms that involve comparative constructions, e.g., *old as the hills*, *colder than a witch's teat*, *Xer than hell/all get out*, etc. However, these generally can be analyzed as uses of *than* as a preposition (see Hankamer (1973)), in which case comparative deletion need not be involved in their derivations.

6) Although our proposals are not formulated within the theory that Chafe (1968) criticizes (viz., the standard theory of transformational grammar), they are not essentially incompatible. Hence, we see no basis for Chafe's claim that "transformational deficiencies" constitute another "anomaly in the Chomskyan paradigm."

Once again, there are counterexamples to Chomsky's claim, as Berman (1974, p. 261) originally noted.

- (8) Some strings are harder to pull than others. That favor was easy to return.
That nerve is easy to touch. The law can be hard to lay down.
This boat is very easy to rock. That line is hard to swallow.
This barrel is, unfortunately, very easy to scrape the bottom of.
His closets would be easy to find skeletons in.

However, the effect of the movement/deletion distinction can be captured (and, indeed, must be if the standard raising/equi differences are to be accounted for) in non-transformational analyses. This is normally done by distinguishing between NP positions which are arguments of the predicates in their clauses (e.g., subject of *try*) and those which are not (e.g., subject of *tend*). So, assuming Chomsky is right about the *tough*-construction, adjectives like *hard* could be analyzed as binary predicates, with their (surface) subjects serving as one argument. Of course, such an account requires that idiom chunks like those in (8) can be arguments to predicates, and they must therefore be assigned some independent meaning. This brings us to the central point of this paper, and our main criticism of almost all generative work on idioms.

2. The Compositionality of Idioms

It has been taken as axiomatic or definitional that idioms are non-compositional, i.e., that the meaning of an idiom is not a function of the meanings of its parts. (9) quotes a number of well-known linguists to this effect.

- (9) a. "I shall regard an idiom as a constituent or a series of constituents for which the semantic interpretation is not a compositional function of the formatives of which it is composed." (Fraser (1970; 22))

- b. "Idioms... do not get their meanings from the meanings of their syntactic parts." (Katz (1973; 358))
- c. "the meaning of the whole is not a function of the meaning of the parts." (Heringer (1976; 209))
- d. "These are idiomatic in the sense that their meaning is non-compositional." (Chomsky (1980; 149))

As indicated above, we take issue with this fundamental and widespread assumption. There are a number of reasons to believe that parts of an idiom should be assigned meanings, contributing to the meaning of the whole idiom.

First of all, parts of idioms can be modified, either by means of adjectives, as in (10), or by means of relative clauses, as in (11).

- (10) leave no legal stone unturned
beat our nuclear swords into plowshares
make the musical scene
kick the filthy habit
- (11) Pat got the job by pulling strings that weren't available to anyone else.
Your remark touched a nerve that I didn't even know existed.

It is important to note that these are examples of what Ernst (1980) has termed *internal modification*, that is, modification of only part of the meaning of the idiom. Thus, for example, *leave no legal stone unturned* does not mean *legally leave no stone unturned*, it means (roughly) that all legal methods are used, not that it is legal to use all methods. In order to modify part of the meaning of an idiom by modifying a part of the idiom, it is necessary that the part of the idiom have a meaning which is part of the meaning of the idiom.

Similarly, parts of idioms can be quantified, as in (12).

- (12) pull a string or two touch a couple of nerves
That's the third gift horse she's looked in the mouth this year.

Again, the quantification may affect only part of the idiom's meaning: *touch a couple of nerves* is not the same as *touch a nerve a couple of times*. The fact that it is possible to quantify over idiomatic *nerves* and *strings* like this indicates that some part of the meanings of these idioms is identified with these expressions.

Parts of idioms may also be emphasized through topicalization, as in (13).

- (13) Those strings he wouldn't pull for you.
His closets you could find skeletons in.
Those windmills not even he would tilt at.

It would not make sense to bring a part of an idiom into focus in this way unless these parts had identifiable meanings in their idiomatic uses.

Finally, parts of idioms can be omitted in elliptical constructions (what is usually referred to as "VP deletion"), as in (14).

- (14) My goose is cooked, but yours isn't.
 We thought the bottom would fall out of the housing market, but it didn't.
 We had expected that excellent care would be taken of the orphans, and it was.

It has been convincingly argued by Sag (1976, 1981) and Williams (1977) that the antecedents of the missing elements in such constructions must be semantic units. Since the antecedents in these examples are parts of idioms (e.g., *cooked* in the first example), it follows that these idiom parts must have meanings of their own.⁷⁾

This, then, is our fundamental break with the previous generative literature on idioms: we claim that the pieces of an idiom typically have identifiable meanings which combine to produce the meaning of the whole. Of course, these meanings are not the literal meanings of the parts. Rather, idiomatic meanings are generally derived from literal meanings in conventionalized, but not entirely arbitrary, ways. The mechanisms governing this meaning transfer are complex and poorly understood, and it is not our purpose here to attempt to explicate them (see Nunberg (1977) for a discussion of some of the issues involved). Our object here is to show that the syntactic properties of idioms which have been the focus of the generative literature are largely predictable from the semantic properties of the idioms in question.

3. Some Examples

We contend that the syntactic versatility of an idiom is a function of how the meanings of its parts are related to one another and to their literal meanings. We will argue for this contention by examining more closely a small number of idioms with different syntactic and semantic properties. We believe that our examples are representative of the diverse behavior of idioms, though we make no claim that they represent an exhaustive or even a systematic taxonomy of

7) Bresnan (1981b) argues that genuine idiom chunks may not serve as antecedents for anaphors (specifically, pronouns). The examples in (14) provide what we think are clear counterexamples to her claim. She supports her contention with the following examples (her judgements):

- (i) *Although the F.B.I. kept *tabs* on Jane Fonda, the C.I.A. kept *them* on Vanessa Redgrave.
- (ii) **Tabs* were kept on Jane Fonda by the F.B.I., but *they* weren't kept on Vanessa Redgrave.

While we agree with Bresnan that there are some idiom chunks which cannot be antecedents for anaphora (e.g., *the bucket* in *kick the bucket*), we disagree with her claim that *tabs* in *keep tabs on* is one of them. In fact, we find the examples in (i) and (ii) quite acceptable, as do others we have questioned. (iii) sounds still better.

(iii) We thought *tabs* were being kept on us, but *they* weren't.

While we do not explicitly address the question of anaphoric reference to idiom chunks in what follows, we believe that our treatment of idioms could provide the basis for a principled explanation of the behavior of idiom chunks with respect to anaphora.

idiom types.

3.1 Consider first idioms like those in (4), which do not have normal syntactic structure. Since the rules of semantic composition for any language are presumably formulated so as to combine the meanings of the parts of well-formed syntactic constituents (see, e.g., Dowty, Wall, & Peters (1980) or Gazdar (1981)), they should not be applicable to expressions which do not conform to the normal syntactic rules of the language. Hence, these expressions must receive their interpretations in nonstandard ways, e.g., by stipulation in the lexicon.⁸⁾ If this is the case, then we would expect idioms like those in (4) to exhibit little or no syntactic variation, for when their form is changed, the grammar provides no way to interpret them. This expectation is borne out.⁹⁾

(15) a. *It is done by easy.

b. *Handsome seems to be as handsome does.

3.2 Our next example is one of the most extensively discussed idioms in the language: *kick the bucket*. Syntactically, it is invariable, except that *kick* gets conjugated, and the word *proverbial* may be inserted. Semantically, it is rather opaque, although, as Nunberg (1977) points out, it does inherit from *kick* a non-durative sense. (That is, it cannot be used in place of *dying* in sentences such as *Pat is slowly dying*.)

We propose to analyze this idiom as having the syntactic structure of a normal verb phrase. In this instance, however, the idiomatic meaning is not composed from idiomatic interpretations of the parts. Rather, the idiomatic meaning is assigned to the whole phrase, without being distributed to its constituents.¹⁰⁾ The absence of an idiomatic interpretation for *The bucket was kicked by Pat* is attributable to the fact that the VP *kick the bucket* does not appear in this sentence.¹¹⁾ Other syntactic deformations that break up the verb phrase are also ruled out. Agreement, however, will naturally occur, since *kick the bucket* is syntactically a normal VP (see Gazdar, Pullum, & Sag (1981) for a formal account of English agreement). Similarly, nothing in the syntax precludes modifiers from appearing, but, since the proper parts of the idiom have no idiomatic meanings of their own, any modification will have to be

8) This is clearly not the whole story for idioms like *No can do* or *Long time no see*, which, though syntactically nonstandard, appear to be semantically composed. However, there is nothing to prevent a syntactically nonstandard idiom from having a special semantic composition rule associated with it.

9) One counterexample to this claim is *trip the light fantastic*, which permits conjugations on *trip*. Hence, it must be analyzed as having some internal structure, with *trip* labeled as a verb.

10) An alternative would be to assign *kick* an idiomatic sense meaning (non-durative) "die", and *the bucket* no sense at all. This idea gains plausibility from the colloquial use of *kick* and *kick off* meaning "die suddenly". However, it raises the problem of explaining why, e.g., *Pat rested the bucket* cannot be used to mean "Pat rested".

11) We assume (with most current theories) surface structure semantic interpretation.

"external" (Ernst (1980))—i.e., modifying the interpretation of the idiom as a whole (or, in the case of *proverbial*, signaling that a non-literal interpretation is intended).

3.3 A very similar example is *saw logs*, meaning "sleep". Here, too, we have syntactic inflexibility, except for conjugation and external modification (e.g., *Pat sure is sawing a lot of logs* meaning roughly "Pat sure is sleeping a lot"). Formally, we treat this idiom just like the previous one: assign normal syntactic structure with idiomatic meaning assigned to the whole VP, but not to its parts.

What is different about these two idioms, however, is that the relationship between the literal and idiomatic interpretations in *saw logs* is relatively transparent, viz., the sound of sawing logs is similar to that of snoring. Hence, this idiom is probably interpretable to those unfamiliar with it, by means of the normal mechanisms for interpreting metaphors (whatever they might be); in contrast, we presume that *kick the bucket* would be uninterpretable (on its idiomatic sense) to a first-time hearer. From the relative transparency of the meaning transfer involved in *saw logs*, it is easy to see why the parts of this idiom do not correspond to parts of the idiomatic meaning. This, we claim, is the explanation for the absence of idiomatic interpretations for sentences like (16).

- (16) a. Logs were sawed by all the campers.
- b. How many logs did Pat saw?

3.4 Consider next another widely discussed idiom: *take advantage of*. We claim that the meaning of this idiom is a function of the (idiomatic) meanings of its parts. More specifically, *take* is assigned a meaning roughly paraphrasable as "derive", *advantage* means something like "benefit", and *of* marks the source. These paraphrases are not exact; indeed, we maintain that no exact paraphrases of these expressions exist. Further, the idiomatic interpretations of these words are such that they cannot sensibly be composed with anything but each other. Formally, this idea can be expressed by treating the idiomatic intension of *take* as a partial function which is only defined on the idiomatic intension of *advantage*.

This analysis will allow the parts of the idiom to be separated syntactically, so long as their interpretation are composed in the permitted manner. Thus, the fact that this idiom can appear in passive and "raised" forms is accounted for.

- (17) Advantage seems to have been taken of Pat.

Further, it allows for the possibility of internal modification (but only by modifiers with the idiomatic sense of *advantage* in their domains). This possibility is realized in the phrase *take unfair advantage of*.

Finally, this analysis permits ellipsis of part of the idiom in some cases but not in others. For example, (18a) sounds odd because the idiomatic sense of *advantage* cannot be composed with the sense of *shocked at*, but (18b) has no

such mismatch, and is hence good.¹²⁾

- (18) a. *Pat took advantage we were shocked at of some of the tourists.
 b. Students take greater advantage of the recreational facilities than they take of the academic facilities.

3.5 *Spill the beans* is very much like *take advantage of* in almost every relevant way. The most notable difference is that it has a literal interpretation which is related in a fairly transparent way to its idiomatic interpretation.¹³⁾ The analogy between literally spilling beans and divulging information that was to have been kept secret (to paraphrase, roughly, the idiomatic interpretation) is quite intuitive. Hence, the assignment of parts of the idiomatic sense to parts of the idiom may seem more natural in this case. Thus, *spill the beans*, like a great many other idioms, is a metaphor which has become conventionalized.

3.6 A central feature of our analysis of the last two examples is the claim that the dependency between the verbs and their objects is semantic, that is, that the inability of idiomatic *the beans* to appear with any verb other than *spill* is due to the incompatibility of its meaning with any other verb. Thus, such idioms are regarded as the limiting case of selectional restrictions, viz., the case where the semantic domain of a verb is a singleton set. It is natural to ask whether there are any idiomatic verbs with non-singleton domains. Our theory leads one to expect to find them, whereas a theory that claimed that the dependency was purely between the forms supports no such expectation. In fact, there are several examples.

- (19) a. hit the hay/sack
 b. lose one's mind/marbles
 c. take a leak/piss/shit/crap
 d. get off one's ass/rear (end)/behind/tush...

There are also cases where the same NP (with the same interpretation) may be the object of more than one idiomatic verb, as we would expect.

- (20) a. play one's cards close to one's chest/lay one's cards on the table
 b. keep/lose one's cool.

12) Our proposals also lead to the prediction that the *advantage* of *take advantage of*, but not the *logs* of *saw logs*, can be the antecedent of an anaphoric pronoun under certain circumstances. We believe that this is a correct prediction, but the facts are open to debate:

(i) ?*Advantage* was taken of her, but *it* wasn't taken of him.

(ii) *He didn't saw logs, but she sawed them. [idiomatic interpretation]

13) Another difference worth mentioning is that the idiomatic NP's differ with respect to definiteness. This fact (which presumably reflects a semantic difference) accounts for the marginality of *spill the beans* in comparative constructions analogous to (18b):

(i) ??They spilled more of the beans to the *Times* than they spilled to the *Post*.

Note, however, that parts of *spill the beans* can be elliptical.

(ii) I was worried that the beans might be spilled, but they weren't.

Such examples lend credence to our semantic account of idiom dependencies.

3.7 Finally, we want to consider syntactically highly versatile idioms like those in (8). We will take *pull strings* as a typical example. These cannot be analyzed in exactly the same manner as those in the previous two sections because of examples like (21).¹⁴⁾

- (21) a. Pat pulled strings that Chris had no access to.
- b. The strings that Pat pulled helped Chris get the job.

If we were to say that the idiomatic sense of *strings* was in the domain only of the idiomatic *pull* (following our analysis of *spill the beans*, etc.), then sentences like (21) could have no idiomatic readings. We must allow the idiomatic sense of *strings* to be in the domain of other functions, including the intensions of *have access to* and *help Chris get the job*. But if we do this, then we will permit an idiomatic interpretation of *strings* in examples like (22).

- (22) a. Chris had no access to strings.
- b. Strings helped Chris get the job.

This appears, at first glance, to be a *reductio* of our treatment of idioms: either we exclude (21) on their idiomatic readings, or we assign idiomatic interpretations to (22).

However, rather than abandon our approach altogether, we propose to bite the bullet by letting the grammar assign idiomatic readings to (22). Support for this decision is provided by the fact that these sentences may be used in contexts where the full idiom *pull strings* has already appeared.

- (23) Pat and Chris graduated from law school together with roughly equal records. Pat's uncle is a state senator, and he pulled strings to get Pat a clerkship with a state supreme court justice. Chris, in contrast, didn't have access to any strings, and ended up hanging out a shingle.

Hence, it seems that the oddness of (22) is not syntactic or semantic; rather, it is a consequence of conditions of use. It is evident that in actual language use, the distinct readings of an ambiguous expression are not equally salient. The order and degree of preference among readings are determined by a number of complex and poorly understood factors, probably including at least the hearer's beliefs about the speaker and about the world, the topic of conversation, the speech registers in use, and relative frequency of usage of the readings. In particular, in the case of idiomatic expressions, it has been established (Swinney and Cutler (1979)) that hearers exhibit a strong preference for idiomatic over literal interpretations. It seems plausible to conjecture further that they exhibit a very strong preference for a literal interpretation when the idiom is incomplete. This would be the case, for example, if idioms are stored in memory as

14) Examples like these have been attributed to Jim McCawley.

chunks, with the meanings of their parts accessible only through decomposition of the meanings of the wholes. Then all the parts of an idiom would have to be explicitly present for an idiomatic interpretation to be assigned, but once such an assignment had been made, the parts could be used in isolation, wherever semantically appropriate.¹⁵⁾

In short, we claim that the central difference between idioms like *pull strings* and those like *spill the beans* is that the object of the former in its idiomatic sense may be an argument to many predicates other than the verb in the idiom. However, conditions of use, which block access to the idiomatic senses of idiom chunks except in contexts where the entire idiom is present, serve to partially mask the semantic versatility of these chunks.

It is worth mentioning that no previous work we are aware of proposes any sort of a principled account of examples like (21) and (22). Thus, while the proposal in this section involves some hand-waving references to ill-understood conditions of use, it is nevertheless an advance over the previous literature.

4. Conclusions

Examples like those above show that many idioms have both syntactic and semantic internal structure. Further, it is clear that idioms cannot be analyzed in a uniform fashion: there is a range of idiom types, differing along various syntactic, semantic, and pragmatic dimensions. To a large extent, the syntactic behavior of idioms is determined by the semantic relationships among their parts. The failure to recognize this fact has been the central weakness of most generative discussions of idioms.

Of course, what we have sketched here is only a prolegomena to a substantive theory of idioms. In order genuinely to explain the behavior of an idiom, it would be necessary to explicate the meaning transfer mechanism deriving the idiomatic interpretation from the literal one. Hence, a full account of idioms must await a theory of meaning transfers. While no such theory is in the offing, we believe that many of the puzzles about idioms which have exercised generativists are solvable, once fundamental misconceptions are abandoned.

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15) For idioms like *take advantage of* and *spill the beans*, the parts are semantically appropriate only in combination with the other parts (see sections 3.4 & 3.5). Hence, examples like (i) and (ii), analogous to 21), have no idiomatic readings.

(i) ?I will never forgive the advantage you took of me.
 (ii) Pat spilled the beans that Chris had entrusted him with.

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Semantics

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Meaning is a fact of language because of a constant and firmly established association between a given segment of conceptual material and a given piece of linguistic expression. It is always an abstraction from the concrete facts of extralinguistic experience. Meaning must be carefully distinguished from the purport (*soderžanije-namerenije*) of utterances, which can be conceived and passed on only in interpretable contexts—larger chunks of discourse and (or) in special contexts of situation. Concrete utterances with their situation-bound purports are facts of speech.¹⁾

Meaning as the content of words (and word-like nominative phrases or "monèmes")²⁾ cannot be equated with concepts. Although, like the latter, it is a reverberation in the human consciousness of phenomena, relationships, qualities and processes of reality, it becomes a fact of language only when a constant and indissoluble connection is established between the reverberation and a certain sound-complex or (phonetic-orthographic) "caul" or "envelope". The latter is indispensable not only because it is the physical expression of the content and the vehicle for communicating it to other people, but also because without it the particular meaning could not come into being, exist and develop. This is the only way for monemes (primarily, of course, the "monolexic" ones) to be "definitely moulded" and "to take as much or as little of the conceptual material of the whole thought as the genius of the language cares to allow".³⁾ I am pleased to find the same general idea expressed in a recent publication: "...a semantic prototype associates a word or phrase with a pre-linguistic, cognitive schema or image; and...speakers are equipped with an

1) "...An utterance is a unique event which has both sound and meaning. Even the same speaker cannot repeat the same utterance." (Hattori 1964). Words are units of language, recurrent and readymade. This indisputable fact is borne out by billions of dictionaries. *Functionally* the Word is "the potential minimum of the sentence—potencialnyj minimum predstavlenija". Otherwise stated "lexical items are in a sense self-contained units, whose categorization determines their possible function within larger units . . ." (Bierwisch 1982, 112). See also A. I. Smirnicky, 1955. (Concerning metalinguistics and "representations" see, i.a. Akhmanova 1961 and 1977.) The dialectic unity of language and speech is also manifested on the lexical-phraseological level: "Every nuance of the utterance, every subtle distinction in the meaning of every word in the context of the utterance, must be reflected in one of the innumerable number of precomputed senses of the words themselves" (Schank, Birnbaum, and May 1982).

2) "In the beginning was the Word". As a term, however, it is really and readily applicable only to the "monolexeme". Different writers therefore, turn to a variety of descriptions: "complex word-equivalents", "lexical units", "lexical items" etc. What we are actually talking about are "units produced by the first articulation" none of which "can be further analysed into a succession of signs". "We shall refer to them as monemes" (Martinet 1960, 25).

ability to judge the degree to which an object (or, if you prefer, the internal representation thereof) matches the prototype schema or image".⁴⁾ In the Soviet semantic tradition the "semantic prototype" under the name of "basic nominative meaning" (*osnovnoje nominativnoje značenie*) has long been the generally accepted foundation of lexical-phraseological research and (most important) widely ranging lexicographic analyses and descriptions.⁵⁾

As far as ontology is concerned we could go no further in our discussion of the relationship between expression and content in the "moulding" of linguistic meaning. Heuristically, however, it is necessary to dwell on the different kinds, or types, of the connection. Briefly the main varieties can be presented as follows:

1. It is very easy to understand and explain the meaning because side by side with the specific "national" expression there exists a secondary semiotic system. Thus, for example, the fact that *quatre-vingts* and *eighty*, or *soixante-quinze* and *seventy five* mean exactly the same thing is borne out by the generally accepted figures—80 and 75.

2. It is very easy to understand the meaning of, for example, Russian *ruka* or *noga*, in spite of the fact that in English, French, and German there are two different terms to cover (or "mould") each of these bits of reality—main/bras, hand/arm, Hand/Arm, foot/leg, etc. because the referents are directly observable, immediately tangible. Unlike (1) the different structure on the expression plane does affect thinking. Russians when learning English or French do have to accustom themselves to the unfamiliar taxonomies; but no serious "semantic" problems are likely to arise.

3. "Meaning" becomes—and remains a problem with abstract notions which cannot be seen and touched, or conveniently represented by generally accepted symbols: know, *znat'*, savoir/connaître, wissen/kennen; want/wish/desire, vouloir/désirer, wünschen/erlangen, etc. The "reverberation" is a "complicated combination of elements". It follows that a way must be found to isolate the semantic elements, consider them one by one and thus arrive at an overall system of "componential analysis".

The number and variety of publications devoted to the splitting up of the recalcitrant "semantic structures" is legion—from the very interesting and convincing analyses of E. A. Nida⁶⁾ to the unconvincing mechanical "checklists".⁷⁾ The trouble with it—the reason why nothing can ever come of it as far as natural human languages are concerned, is that it violates the basic principle

3) Sapir 1921. "Na różnicach w sposobach kształtowania przedmiotów myśli odpowiadających wyrazom polegają podstawowe, zasadnicze różnice między językami" (Doroszewski, 1970). Cf. Coseriu 1976.

4) Coleman and Kay 1981. "Degree" evokes the concept of "gradience" (Bolinger 1960, 1975). "In a sense...all language is metaphorical" (Schank et al. 1982).

5) As we turn to "conceptual types" and "aitiational structures" (Bierwisch 1982, 109 ff.) we are happy to be able to fall back on the "innumerable numbers" of publications. See especially Vinogradov 1953 and Zgusta 1971.

of indissoluble unity of expression and content. As far as linguistics, *jazykoznanije*, the science of natural human languages is concerned semantic components, semes, etc. simply do not exist, for nobody has ever succeeded in explaining how they could be related to the overall phonetic/orthographic composition of the parent moneme. The human child gradually develops into a full-fledged member of a given speech community because he is systematically subjected to a "two-in-one reverberation"—the extralinguistic object and the word or phrase that naturally goes with it (or "the word and phrase and the prelinguistic, cognitive schema or image").

There are, of course, the morphemes. There can also be no doubt as to the great value of, for example, "The Russian Derivational Dictionary"⁸⁾ which will continue to be the cornerstone of all morphological research in the field of lexical morphological categories for years to come. But even here "The criterion of semantic distinctive features held in common would not in itself be adequate either, since etymologically unrelated synonyms would then have to be listed together".⁹⁾ From a recent dictionary of "semantic factors" (or "multipliers", *množitelej*) of Russian we learn that they must be equated with "...the full words which appear on the right-hand side of the dictionary entry—the definitions".¹⁰⁾ As time goes on, the hope that for *natural human languages* constant and recurring minimal features (analogs of the distinctive features for phonemes) can hardly be held out. First of all there is simply the "numerical factor".¹¹⁾ Most important, however, is the fact that the "two-sided" (*dvustoronniye*) units of the semantic level are global by definition! They are firmly rooted in our consciousness in the globality of the dialectical unity of the given national expression and the given national, socio-historically conditioned content. If the particular "dissection of extralinguistic reality" by the set of global units "comes in natural" we speak of *knowing* the language. Conversely, following A. I. Smirnitsky, it is this knowledge (shared with the other members of the speech-

6) Nida 1975, 1975a. What was needed, however, was a clear-cut distinction between the "interlinguistic" (as well as the "anthropological") approaches and the "linguistic" (*jazykovedčeskij*) ones. In the case of natural human languages "...le signifié est donné par la connaissance de la langue (Coseriu 1982, p. 120) "...les lexèmes primaires correspondent à des intuitions unitaires et ils ne sont dans aucun sens le produit d'un assemblage de traits distinctifs déjà donnés. Ils ne présentent des traits distinctifs que parce qu'ils entrent en opposition avec d'autres lexèmes: ce sont les traits distinctifs qui existent en vertu des oppositions, non pas le contraire" (Coseriu 1982, pp. 123-4). Cf. Akhmanova 1970. Most instructive remains the Discussion to Colby 1966.

7) On second thoughts I would not like to sound categorical. Difficulties do arise. We distinguish between signification and designation, but we cannot divorce them altogether (Coseriu 1982, Note 8). There certainly is something in the idea of a "commercial transaction scenario" (Schank et al., 1982, pp. 136-7).

8) Worth et al. 1970.

9) Karaulov 1980.

10) Ibid p. 8.

11) 10.000 words to one phoneme! (400.000 words in the NED, with only about 40 phenemes in Modern English) Cf. Coseriu 1966.

community) that constitutes the peculiar semiological system, Saussure's "langue".¹²⁾

Having introduced the phrasal term "semiological system" I must hasten to emphasize the modifier—"peculiar and specific" because of the dialectical unity of the "expressionist" and the "functionalist" approaches. Words are normally used in accordance with their "intensional" meanings—a pie is tasty or savoury. A "delicious pie" is normal, but implying almost effusive praise of the food. We would certainly *not* speak of a "delicious road" in the natural order of things. But what if we suddenly came to a neglected part of the road, almost buried in flowering bushes—un lieu délicieux? "Delicious" is a Romance word. It does not have to be kept in check by the rigours of Anglo-Saxon understatement. "Meander" is normally said of rivers and streams, but there is nothing to prevent using it metaphorically, to denote any kind of "proceeding" or "movement", including speech, so long as it is not straightforward and purposeful.¹³⁾ Using a language like English is not fully conditioned by a commonly shared stock of units and patterns. So much depends on the register, the purport, the speaker's (writer's) linguistic sophistication, sense of humour, attitude to quibbles and word-play, age and background knowledge, and many other factors. The more influential the speaker/writer, the greater the impact of the particular *habla* on the system. Our knowledge of language is never complete. It is not a "finite state", because of the perpetual interaction between the processes of speech and the amount of knowledge derived from them by particular individuals at any given time.

So much, then, in so far as the semantics of natural human languages is concerned. I shall now turn to interlinguistics, the science of auxiliary languages, from international *a-posteriori* ones, like Esperanto to the infinite variety of the *a-priori* kind—mathematical mediator, logico-informational, classificatory, algorithmic machine, etc. etc. up to extraterrestrial communication. In extreme cases interlinguistics is conceived as an abstract deductive theory. It sets out to create by abstraction, on the basis of mathematical logic a system of "pure elementary semes", universal semantic factors (multipliers). Natural human language is part of philology, while interlinguistics is part of semiotics. It is extremely important to understand the difference, the implied ontological and heuristic reversal. It is most unfortunate that for decades so many people have striven to do away with the study of natural human languages altogether, abolish philology, give up studying languages and proclaim different varieties of deductive constructs as the only scientific approach to "Linguistic Theory".¹⁴⁾

Now that things are changing for the better (the more arrogant and "phagocytical" theories are being gradually cut down to size) there is time to go a little more deeply into the mutual relationship of linguistics and interlinguistics,

12) Smirnitsky 1954.

13) "A la tendance à l'accord des locuteurs s'oppose leur tendance à la fantasie, à la créativité, à l'anarchie. D'où les incohérences du système" (Buysse 1980).

14) The climax came with generative-transformational grammar.

the dialectics of the *physei* and the *thesei*, the study of human language "in its natural state" and of "what people have tried to do to it" by way of rationally improving (optimising) it as a means of intellectual communication. It is no longer "either-or" (with cavalier attempts at establishing superiorities), but a painstaking investigation of the advantages of give-and-take on both sides.¹⁵⁾

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¹⁵⁾ So much has already been done in this direction that it would have to be treated separately. Cf. Akhmanova 1963, 1967, 1977.

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Formal and Lexical Semantics

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The title of this paper refers to two domains in the study of meaning: the compositional structure of meaning depending on the formal make-up of linguistic expressions, and the interpretation of lexical units entering into the compositional structure. The aim of the paper is to pursue some of the issues arising from the integration of the two domains, so as to account for the way in which the compositionality is anchored in, and sets the stage for, the meaning of lexical items. After some remarks on general and background assumptions, I will mainly be concerned with two topics: the conceptual interpretation of lexical items, and the role of so-called thematic relations in compositional structure. Because of the limited space, my discussion will be somewhat programmatic, relying on exemplification rather than systematic exposition.

1. On the Nature of Semantics

Semantics studies certain aspects of the meaning of linguistic expressions and tries to reveal the laws and principles underlying these aspects by constructing an explanatory theory of its subject matter. There are various ways to turn this general view into more specific proposals. In what follows, I will consider semantics as an empirical discipline whose subject matter belongs to the realm of mental representations underlying human behavior, including in particular the use of natural language. Mental representations are to be thought of as (probably second order) properties of the human organism. They are determined by two types of conditions: (a) the inherited principles of mental organization, and (b) the physical and social environment the organism interacts with. Neither this determination nor the resulting representations or operations on them are amenable to any simple and direct observation. Suffice it to say, then, that I will assume a materialist conception of mental structures—and hence of semantics—in the sense that both the internal and external conditions shaping mental representations are ultimately to be traced to material properties of the universe.

On this account, semantics becomes a branch of the Cognitive Sciences, which is primarily concerned with certain aspects of understanding linguistic utterances, where understanding is now to be characterized by the creation of mental representations of a certain type. Before I am going to make this notion somewhat more precise, let me briefly compare it with the approach developed in Logical Semantics.

Logical Semantics, descending from the tradition of Frege, Carnap, Tarski, and others, is primarily concerned with the truth and reference of linguistic

expressions. The meaning of an utterance—or rather its denotative aspect—is captured on this approach by its sense or intension, which is a function that determines its reference and truth value with respect to a given context and situation. This approach has been developed by Model Theoretic Semantics into a fully explicit formal theory which precisely specifies the combinatorial structure of intensions with respect to arbitrary possible worlds (where the extension comprises both reference and truth value). Semantics thus becomes ultimately a branch of mathematics.

One might be tempted to reconcile the two approaches by interpreting intensions as a characterization of the mental representations in question. To understand a sentence would then amount to having mentally represented its intension. This comes in fact close to Frege's original conception of sense, and to Wittgenstein's notion that to know the meaning of a sentence is to know what is the case if it is true. This interpretation would at the same time provide an explicit formal theory of the structure of the relevant mental representations, and an account of how these representations relate to reality, that is to say how utterances refer to and describe whatever they are about. There are, however, certain difficulties with this reconciliation, one of them concerning the intension of lexical items and thus of direct relevance to the present paper.¹⁾ The problem is roughly this: The meaning of many lexical items, as Putnam (1975) has argued, cannot at the same time be mental representations of some sort and also effectively determine the reference of the term. The simplest case is that of incomplete knowledge of the relevant criteria. Thus someone who cannot tell an elm from a beech tree is not able to determine the extension of these terms, even though he might know that their extension is different. The meaning of an expression therefore cannot in general be a function that effectively determines its extension. Putnam's solution to this problem has two parts. First, a meaning is a mentally represented stereotype, and second, it is related to its extension by a causal connection to an appropriate sample of the extension encountered on the occasion of the introduction of the term. This proposal raises at least two questions, though: (a) What is the structure of a stereotype? (b) How can the sample be identified that serves the initial specification of the stereotype? I will take up these questions in Section 3.

Returning to the characterization of mental representations, I will make the following preliminary assumptions:

- (i) Mental organization is modular, i.e. different systems and subsystems of mental structure interact in specific ways in determining the way in which the organism copes with its environment. (See Chomsky (1980) for further discussion of the notion of modularity.)
- (ii) Each mental system is based on a set of principles which lead, by way of ontogenetic development, to a system of rules or patterns specifying the mental

1) Another problem, about which I have nothing to say, concerns the truth conditions of Propositional attitudes like *know*, *believe*, *hope*, etc. See the papers in Peters and Saarinen (1982) for extensive discussion.

representations underlying the pertinent domain or aspect of behavior.

(iii) Among the mental systems, there is the language system *L* and the conceptual system *C*, the representations of *L* being responsible for the use of natural language, and the representations of *C* determining the way in which real and fictitious situations, perceptions, actions, and systems of belief are conceptualized.

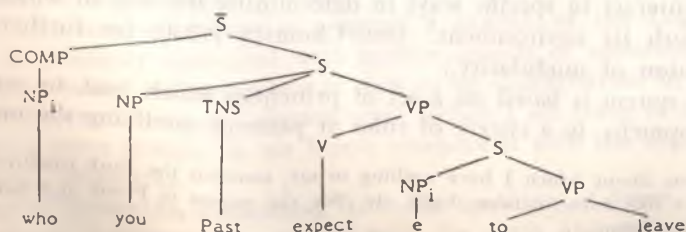
On the basis of these assumptions, the subject matter of semantics can be construed as the principles, rules, and representations that constitute the interface of *L* and *C*, and more specifically as that subsystem of *L* that determines the way in which representations of *L* are related to, or interpreted in terms of, representations of *C*. That is to say, semantics deals with the relation of linguistic utterances to actual or possible situations, objects, events, etc., insofar as these are conceptualized by representations of *C*. How these eventually relate to what they are representations of is an important empirical question, but it concerns the interpretation of linguistic utterances only indirectly, i.e. via the mediation of conceptual representations. It follows from these considerations that semantics is dependent on and contributes to both the theory of the linguistic system *L* and the conceptual system *C*, although both systems have properties that go far beyond the proper concern of semantics.

2. Levels of Representation

In order to narrow down the previous considerations, I will make the following more specific assumptions about mental representations. First we may distinguish what is pretheoretically called the form and the meaning of an utterance. There are (at least) two types of representation involved in the form of an utterance: its phonetic representation *p* and its morpho-syntactic representation *syn*. I have nothing specific to say in the present context with respect to *p* and will simply assume that it captures the segmental and suprasegmental characteristics of the pertinent utterance type. As to *syn*, I will assume that it is to be characterized by a labelled tree (or equivalently a labelled bracketing of its terminal string) with syntactic categories and formatives attached to the non-terminal and terminal nodes, respectively. In addition to the constituency thus indicated, *syn* might also account for binding relations expressed by indices. For the sake of illustration, (2) might be considered as a rough approximation of one of the syntactic representations assigned to the ambiguous sentence (1):

(1) Who did you expect to leave?

(2)

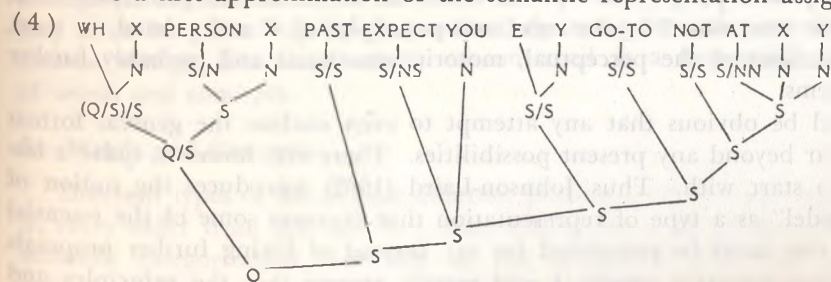


The alternative structure of (1) with transitive *leave* would be something like (3)

(3) [_S [_{NP_i} who] [_{NP} you] Past expect [_S PRO [_{VP} to leave [_{NP_i} e]]]]

There are various ways to specify further the necessary details, the rules that underlie (2) and (3) and that relate both to (1). As I will not be concerned with these problems in any detail, I will simply assume that *syn* can be accounted for by the notion of S-structure developed in Chomsky (1981).

With respect to the meaning of an utterance, I will claim that again at least two different types of representation must be distinguished, which I will call the semantic representation *sem* and the conceptual representation or utterance meaning *m*. As to *sem*, it can by and large be identified with the logical form of an utterance in one of the various renderings of that term. In fact, one might think of *sem* as an elaboration of what Chomsky (1981) calls the logical form LF, where the elaboration concerns two points: first, I will assume a certain amount of internal structure for lexical items, and second I take *syn* and *sem* to be based on different sets of categories. More specifically, *sem* is to be characterized by categorized semantic primitives, either semantic constants or variables, where the categories serve two functions: first they specify the combinatorial structure of *sem* in terms of functor-argument relations leading to categorized trees,²⁾ and second, they determine the type of conceptual units in terms of which basic or complex units of the semantic level are to be interpreted. In addition to constituency determined by the categorization, *sem* is subject to the usual variable binding. Pending further specification, (4) would be a first approximation of the semantic representation assigned to (2):



Here PERSON, PAST, EXPECT, YOU, etc. are first step approximations of semantic primitives, about which I will have to say more below, S (for sentence), N (for name) and tentatively Q (for Wh-Question) are basic categories, S/N, S/S etc. are functor categories.³⁾ The type of representation illustrated in (4) is based on a categorial language. For further specification, see Section 6. Dropping the categorization, the semantic representation as-

²⁾ While the ordering of the terminal string in *syn* is crucial, it is presumably only a matter of notation with respect to *sem*. In what follows I will adopt the convention of placing the functor before its arguments.

³⁾ For the sake of simplicity, I have not indicated the categorization of the binding operators WH and E. Schematically, WH would be categorized as ((Q/S)/S)/N, E as (S/S)/N. Operators like these deserve special discussion, though, which I cannot enter into here.

signed to (3) can be specified by (5)

(5) [[WH X[PERSON X]] [PAST[EXPECT YOU[GO-TO[NOT
[AT YOU X]]]]]]

Both (4) and (5) are based on something like (6) as a rough lexical characterization of (transitive and intransitive) *leave*, where \hat{X} and \hat{Y} are abstractors binding the pertinent variables. (See Section 4 for further discussion.)

(6) $\hat{Y}[\hat{X}[\text{GO-TO}[\text{NOT}[\text{AT } X \ Y]]]]$

Before turning to the level of the conceptual representation m , I will briefly comment on the relation between the various representations and the rules underlying them. Following usual assumptions, I take the triple $[p, \text{syn}, \text{sem}]$ to be determined by the rules and principles underlying L , that is by the grammar G , where the syntactic rules of G specify syn , which is mapped by morphological and phonological rules of G into p , and by rules of construal and binding as discussed in Chomsky (1981) together with lexical rules like (6) and the principles of lambda-conversion to be discussed below into sem . We may now think of sem as determining the conceptual structure m , which is in turn relative to a conceptual context type ct .⁴⁾ In Section 3, I will discuss some of the principles that relate sem to m , indicating the way in which language is, so to speak, hooked upon conceptual structures. Ignoring intermediate representations that are likely to intervene at certain places, we get something like (7) involved in understanding an utterance type:

(7) $[[p, \text{syn}, \text{sem}]ct, m]$

Notice that just as p , syn and sem are based on the rules and principles of L , ct and m are determined by the rules and principles of C and related, in turn, to representations of the perceptual, motoric, emotional and probably further mental systems.

It should be obvious that any attempt to even outline the general format of m goes far beyond any present possibilities. There are, however, quite a few proposals to start with. Thus Johnson-Laird (1982) introduces the notion of "mental model" as a type of representation that captures some of the essential conditions that must be postulated for m . Instead of listing further proposals and specifying tentative details, I will merely assume that the principles and rules underlying representations in C provide among others the types of a basic ontology, i.e. individuals, properties, relations, events, time and space coordinates, as well as the conditions on combining them into propositions specifying more or less complex situations or states of affairs. Thus if *black* represents a property and u an individual in C , then something like *black*(u) ascribing that property to u should be a possible part of an m in C .⁵⁾

4) To put it slightly differently, ct might be construed as the mental model of a situation type with respect to which sem is to be interpreted, resulting in a modified model ct' , such that m characterizes ct' relative to ct .

5) A somewhat more specific assumption about m would be provided by the 'discourse representations' developed in Kamp (1981), according to which the interpretation of a sentence ends up with a structured set of elementary propositions and discourse referents.

Let me conclude this section with three methodological remarks. First, as already mentioned, mental representations, rules, and principles are not available for direct observation. They are rather to be inferred from all kinds of indirect evidence and subject to reasonable idealization.

Second, following this general principle, different levels of representation are to be postulated if (and only if) there are relevant generalizations to be captured by their distinction. Thus, although both *syn* and *sem* are specified by categorized trees, they cannot be collapsed into one level of representation. To mention just two reasons, neither the categories nor the terminal elements coincide in general. Thus, while e.g. *John*, *who*, *everybody* are all categorized as NP for general syntactic reasons, they belong to rather different categories semantically, *John* specifying a name, *who* and *everybody* binding operators of different types. And the semantic representation of basic syntactic units may have an internal structure that by no means coincides with their specification with respect to *syn*.

Third, the complement of this remark is, that two representations of different levels must not be stipulated to be different beyond those distinctions that are motivated on theoretical and empirical grounds. Thus syntactic and semantic representations should have different hierarchical structure only to the extent that is determined by empirical conditions. By the same token, syntactic formatives may be individuated to a large extent by their phonological forms, and similarly semantic primitives may to some extent be identified by conceptual conditions. As a heuristic consequence of this condition, concepts may often be picked up by the words they interpret under certain fixed conditions, though this must not be misconstrued as a theoretical identification of words and concepts.

3. Words and Concepts

Different types of words pose different problems. Words like *although*, *only*, *if*, *every* differ from say, *cat*, *run*, *idea*, *school* not only by their syntactic and semantic categorization, but also by the manner of their conceptual interpretation. Hence there is no simple generalization of the following proposals to lexical units in general.

An important fact about lexical knowledge as opposed to other parts of linguistic competence seems to me its considerable variability, even under appropriately idealized conditions. I take this, in fact, to be an intrinsic feature of lexical competence, which concerns not only the quantitative aspect, i.e. the fact that we constantly acquire new lexical items in a way that does not hold for other parts of the grammar, but also the individual lexical items themselves, although these two aspects are not independent from each other.

I will discuss three aspects of the conceptual interpretation of lexical items that are involved in this flexibility. These I will call conceptual differentiation, conceptual shift, and conceptual specification. The first concerns the nature of concepts *per se*, the last two concern the relation of words to concepts.

I will take them up in turn. I will suppose without further argument that the existence of concepts does not coincide with their lexicalization, that is to say that in principle there may be concepts that are not connected to linguistic expressions.⁶⁾ As a starting point, let us assume the following:

- (8) A concept C is determined by a schema $[t, [a_1, \dots, a_n]]$ where t is a conceptual type, and $[a_1, \dots, a_n]$ is a system of explanatory principles according to which concepts develop.

The conceptual type t is provided by the ontology inherent in the conceptual system C , according to which concepts are sorted into representations of individuals, kinds, properties, relations, events, etc. The system of principles a_1, \dots, a_n accounts for the different dimensions according to which actual and possible experience is organized. Substance, structure, function, causal connection are plausible candidates for these organizational principles.⁷⁾ They provide the framework according to which common sense as well as scientific knowledge develops.

These principles play different roles with respect to different concepts, and they may allow, moreover, for different stages in the development of a given concept. Water for instance might first be characterized as to structure and function, and only later on by something like H_2O for substance. Instead by a fixed and uniform stereotype, a concept will thus be represented by a flexible schema of distinctions emerging from the principles in terms of which experience is accommodated. On this account, common sense and expert concepts are not necessarily separated, but are different, though compatible differentiations with respect to alternative principles.

A crucial assumption underlying these considerations is the interdependence of concepts. Common sense explanations, just as scientific theories of different kinds, are not collections of isolated concepts, but rather connected systems that organize coherent domains of experience. For the sake of illustration, we may assume that C contains e.g. the following fairly different subsystems:

- (9) Spatial structure, determining
- (a) a system of coordinates
 - (b) conditions on orientations, such as verticality, internal vs. external, etc.
 - (c) operations of comparison and measuring
- (10) Social structure, determining (among others)
- (a) dimensions of social relations

6) That is to say that conceptual distinctions need not be reflected linguistically, as is widely documented by pre-linguistic cognitive development, both human and otherwise. Whether in certain cases they cannot be so reflected, may be left open here. Notice, however, that there may be lexical items which, though not synonymous, are not related to conceptually distinct representations either. Putnam's elm-beech-problem is a case in point. The elm-beech distinction provides, so to speak, a slot for conceptual specifications, without actually filling it.

7) This proposal is discussed in Moravcsik (1981), where the Aristotelian notion of "aitia" is interpreted as "explanatory factor" and the four Aristotelian categories of explanation are construed as an "aitiational frame" according to which conceptual structures might develop.

- (b) social institutions and the roles emerging from them
- (c) aims inherent in social behavior

Although hopelessly provisional and incomplete, examples like these allow for some principled remarks. First, concepts acquire their specifications within highly organized systems, composed of conditions or principles that need not correspond in any simple way to separable features of individual concepts. That is, although concepts are compositional in some crucial sense, they are not collections of isolated conceptual components. Second, by the same token, a given conceptual component may determine a whole system of interrelated concepts—somewhat like an axiom determining a whole system of theorems within a coherent theory.

With these considerations in mind, we may think of a concept as picking out—or abstracting over—a certain type of entity by means of its conceptual type, where the specification of the entity in question is provided by the content of its aitiational structure. To give an informal illustration, the concept of height might be something like (11), where $m(x)$ is the type of a function that assigns a measure to an entity.

- (11) [$m(x)$ [a_2 : v is the vertical dimension of x & m is the measure of v]]

Assuming that conceptual differentiation proceeds somehow along the lines outlined so far, we may now turn to the question how lexical items relate to concepts. The basic idea to be pursued in this connection is this: Lexical items, even if they are not ambiguous in any ordinary sense, are in general related not to single concepts, but rather to systematically connected families of concepts. A lexical item, that is to say, generates a more or less diversified family of concepts, each of which may become, depending on the context of interpretation, its conceptual interpretation. The mechanisms involved are conceptual shift and conceptual specification.

To illustrate conceptual shift, consider the following examples:

- (12) I put the letter on your desk.
- (13) The letter has been distributed to the whole faculty.
- (14) The letter finally led to a political crisis.
- (15) For many poets, the letter is a genuine literary genre.

Under the most natural interpretation, *the letter* in (12) represents a physical object of a certain kind, in (13) a set of those objects, (14) the informational content, and in (15) the type of informational structure. Although closely related, these interpretations of "letter" clearly are different concepts amenable for different predications. Notice, first of all, that this kind of variation is fairly widespread. *Book, novel, poem, madrigal, symphony, sculpture, picture* and many others all produce similar, though not always identical variations; similarly *school, university, theatre, parliament*, etc. variably represent an institution characterized by a certain purpose, the location of the institution, the principle underlying the institution. Further types of lexically generated families of concepts can easily be added. Consider for example the particularly intriguing families determined by *word, phrase, or language*. It is furthermore

to be noticed that conceptual distinctions of the type in question frequently remain completely implicit, escaping any explicit discrimination, until particular demands come up. In fact, the recent history of linguistics consists to a reasonable extent in the clarification of the concepts associated with *language*, eventually distinguishing them terminologically by *competence*, *performance*, *dialect*, *idiolect*, *communication*, etc.

Returning to *letter*, suppose that its semantic representation in the lexicon is something like (16), which generates a family of concepts that is centered around (17), the concept showing up in (14). We may then assume that C contains i.a. the principles (18) to (20), which give the interpretations of (12), (13), and (15), respectively, if (17) is substituted for the variable *x*.

- (16) $\hat{X}[\text{WRITTEN INFORMATION } X \ \& \ E \ Y[E \ Z[\text{ADDRESS } Y \ X \ \text{TO } Z]]]$
- (17) $[x[a_2: x \text{ is visually represented information, } a_3: E \ y \ E \ z[y \text{ addresses } x \text{ to } z]]]$
- (18) $[z[z \text{ is physical object} \ \& \ x \text{ is represented in } z]]]$
- (19) $[z[z \text{ is a set of physical objects} \ \& \ \forall w[w \in z \rightarrow x \text{ is represented in } w]]]$
- (20) $[z[z \text{ is the kind of } x]]]$

Although this highly provisional example raises more questions than can be answered or even formulated here, it may suffice to illustrate the problem of conceptual shift, viz. how a (non-ambiguous) lexical item is related to an organized family or type-shifting concepts clustering around a defining core.

Let me now briefly illustrate the related, but different phenomenon of conceptual specification. Consider the different ways of losing involved in (21) to (25):

- (21) John lost his money, as he was not aware of the hole in his pocket.
- (22) John lost his money by speculating at the stock market.
- (23) John lost his friend in the overcrowded subway station.
- (24) John lost his friend in a tragic car accident.
- (25) John lost his friend, as he could never suppress bad jokes about him.

Suppose the lexical semantics of *lose* is something like (26), with *Z* ranging over events to be specified at the conceptual level:

- (26) $\hat{X}[\hat{Y}[E \ Z[\text{CAUSE } Z[\text{GO-TO}[\text{NOT}[\text{HAVE } Y \ X]]]]]]]$

Here different specifications both of the event-variable and of the HAVE-relation on the conceptual level produce the different concepts fitting the contextual settings induced by (21) to (25). The difference between conceptual shift and conceptual specification is, that the latter does not create different types or categories of concepts, but rather various specifications within one and the same conceptual type by filling in open slots, so to speak. Without going into further detail, I will merely point out that the three types of variation I have discussed may interact in various ways. Consider e.g. the systematic correspondence between conceptual shift and specification in cases like (21) and (22). In (21) *money* must be understood as referring to concrete objects and *lose* as a change of location, in (22) *money* refers to abstract exchange value and *lose* to a change in possession. Similarly *letter* and *ignore* in (27) and (28) co-vary between a concrete and an abstract interpretation:

- (27) John ignored the letter that lay on the desk.
(28) John ignored the letter that contained all the missing information.

Let me conclude this section with some remarks on the way in which semantic primitives—or more specifically semantic constants—match with conceptual specifications. From what I said about conceptual components, it should be clear that there can't be a simple isomorphic correspondence between semantic primitives and basic conceptual constituents. In fact, semantic and conceptual representations are organized according to fairly different principles, adapted to different purposes and conditions: *C* has to integrate different modes of experience by means of certain principles of explanation, while semantic representations are basically determined by formal properties of linguistic expressions. To use a by no means superficial analogy: Semantic components accommodate conceptual configurations, which are determined by at least partially independent principles, to the combinatorial principles of language, just as phonological features accommodate motoric and perceptual principles of speech production and perception to the combinatorial structure of linguistic form. This accommodation seems to be achieved by semantic primitives that sometimes correspond rather closely to more or less elementary conceptual constituents, sometimes, however, relate to fairly complex conceptual structures that may contain, moreover, open parameters to be fixed either accidentally in terms of contextual conditions, or systematically according to explanatory principles, the flexibility of stereotypes being a case in point.

4. Thematic Relations

According to what has been said so far, semantic representations are determined by two types of conditions: the principles of conceptual structures which they have to accommodate, and the principles of formal linguistic structure of which they are a part. More specifically, the semantic representations mediate between conceptual representations and the combinatorial structure of syntax. A crucial part in this mediation is played by the lexical items. They are, on the one hand, the basic elements of syntactic representations while on the other hand they are related to fairly complex and flexible configurations of conceptual structure. In view of the latter, semantic representations of lexical items in general have a certain amount of internal structure, i.e. they are configurations of semantic primitives determining the context dependent conceptual interpretation of linguistic expressions. Although more would have to be said about the principles underlying this interpretation, I will assume that they are to be developed along the lines discussed above, and turn to the problem how the internal structure of lexical items is related to their role in syntactic structures.

To begin with, I will assume that lexical items are in a sense self-contained structural units, whose categorization determines their possible function within larger units, while on the other hand the combinatorial structure within and between lexical units is determined by the same principles. In fact, this as-

sumption has already been illustrated by the illustrations in section 2, where (6) was given as the lexical semantics of *leave*, a complex unit of category (S/N)/N, which then occurs in syntactically determined combinations like (4) and (5). In what follows, I will show more specifically how thematic relations as one of the main principles determining the combinatorial potential of lexical items, mediate between external and internal compositionality.

For the sake of illustration, consider (29) with the syntactic structure (30) and the semantic representation (31):

(29) John left the university.

(30) [_S[_{NP}John] [_{TNS}Past] [_{VP}[_Vleave] [_{NP}[_{Det}the] [_Nuniversity]]]]

(31) [[DEF X[UNIVERSITY X]] [PAST[GO-TO[NOT[AT JOHN X]]]]]

Ignoring a large number of problems that are irrelevant to the present issue, (31) derives from (30), if we assume the following oversimplified lexical entries for *John*, *leave*, and *university*:

(32) [/John/; [+NP]; [JOHN]]

(33) [/university/; [+N], []; \hat{X} [UNIVERSITY X]]

(34) [/leave/; [+V], [](_{NP₂}); \hat{X}_1 [\hat{X}_2 [GO-TO[NOT[AT X₁X₂]]]]]

Here JOHN and UNIVERSITY are in fact complex semantic constants of category N and S/N, respectively, while GO-TO and AT (of category S/S and S/NN respectively) come closer to primitive constants to be interpreted by conceptual configurations defined by the systems of (abstract) space and motion. In order to get from (30) to (31) by means of (32) to (34), lambda-conversion according to the following principles must be assumed:

- (35) (i) If a lexical item is subcategorized for NP_i and its semantic representation contains \hat{X}_i , then NP_i specifies \hat{X}_i . (Complement rule CR)
 (ii) The Subject NP of a verb specifies the innermost \hat{X}_i that is not coindexed with a complement NP. (Subject Rule SR)

The notion of a specified \hat{X} in (35) is defined as follows:

- (36) (a) α specifies \hat{X} if α replaces by lambda conversion all X bound by \hat{X} .
 (b) A constituent C specifies \hat{X} if α is (bound by) the semantic representation of C and α specifies \hat{X} .

Consider next (37) with the semantic representation (38):

(37) (a) John left. (b) [_S[_{NP}John] [_{TNS}Past] [_{VP}leave]]

(38) [PAST[E X[GO-TO[NOT[AT JOHN X]]]]]

In order to account for this intransitive reading of *leave*, I have indicated in (34) that its object is optional. The interpretation of the respective abstractor \hat{X}_2 in (34) will now be taken care of by the following clause to be added to (35):

- (35) (iii) If X is not specified by CR or SR, replace $\hat{X} \dots [\varphi]$ by $\dots E X[\varphi]$, where $[\varphi]$ does not begin with \hat{Y} .

(Unspecified Argument Rule UAR)

As can easily be seen, with this extension, (34) and (35) account for (4) as well as (5), i.e. for both readings of (1). Moreover, the rules in (35) generalize in a natural way to lexical items of different semantic categories, in particular to verbs with more than two arguments and to relational nouns. For the sake of

illustration, consider the following rough approximations:

(39) [/bring/; [+V],
[$_\text{(NP}_2\text{)NP}_3$]; $\hat{X}_3[\hat{X}_2[\hat{X}_1[\text{DO-CAUSE } X_1[\text{GO-TO}[\text{AT } X_1X_2]]]]]]]$

(40) [/brother/; [+N],
[$_\text{(of NP}_1\text{'s)}$]; $\hat{X}_1[\hat{X}_2[\text{SIBLING } X_2X_1 \text{ AND MALE } X_2]]]$

(39) would provide, among others, the semantic representation (42) for (41), while (40) accounts for both (43) and (44):

(41) What did you expect me to bring?

(42) [[WH X[OBJECT X]]
[PAST[EXPECT YOU[E Y[CAUSE ME[GO-TO[AT X Y]]]]]]]

(43) (a) the brother (b) [DEF X[E Y[SIBLING X Y AND MALE X]]]

(44) (a) a brother of mine (b) [E X[SIBLING X ME AND MALE X]]

We can now conceive the thematic relations of a lexical item as determined by the lambda-bound variables defining its semantic categorization and, thereby, its combinatorial possibilities. More specifically, we can define the assignment of thematic roles to syntactic constituents in the sense discussed in Chomsky (1981) as follows:

(45) A lexical item LI assigns to a constituent C a thematic role R iff C specifies some \hat{X} of LI according to (35).

A plausible consequence of (45) is the fact that the content of R is determined by the semantic constants in LI whose arguments are bound by \hat{X} . Notice furthermore that on this account thematic roles are not only assigned by verbs, but also by nouns and, of course, other lexical items. Instead of pursuing the problems connected with this generalization, I will briefly consider the fact that not all subjects enter into thematic relations. There are basically two types of non-thematic subjects, viz. those of verbs like *seem*, etc., and of verbs like *rain*. Their respective lexical entries are something like (46) and (47), where RAIN is a (presumably complex) constant of category S, and SEEM of type S/S, i.e. the variable X_1 in (47) is of category S.

(46) [/rain/; [+V], [$_\$]; [RAIN]]

(47) [/seem/; [+V], [$_\text{S}_1$]; $\hat{X}_1[\text{SEEM } X_1]$]

As can be seen, the subject Rule (35ii) cannot apply, as—for different reasons—there is no \hat{X} to be specified by the subject NP. Hence no thematic role is assigned to the subject of verbs like (46) and (47). The syntactic consequences of this fact are then determined by the principles discussed in Chomsky (1981).⁸⁾

Pursuing the preceding outline, we may eventually arrive at a theory that

8) Actually, the situation is slightly more complex with *seem*, as we have to account not only for sentences like (i), but also for cases like (ii):

(i) John seems to have the solution.

(ii) John seems to me to have the solution.

This requires a lexical entry like (iii), where the optional parts in the syntactic and the semantic components are interdependent:

(iii) [/seem/; [+V], [$_\text{(to NP}_2\text{)S}_1$]; $\hat{X}_1[(\hat{X}_2[\text{CONCEIVE } X_2][\text{SEEM } X_1]]]$

I cannot pursue the details of this account any further.

accounts both for the external and the internal combinatorial structure of lexical items and for the way in which this structure is related, primarily by thematic relations, to the combinatorial structure of syntax. In other words, we will arrive at a theory of the structure of *sem* and its relation to *syn*. This leaves us, however, with the still unsolved problem to account for the way in which combinatorial relations in *sem* correspond to the combinatorial structures that connect the conceptual structures in terms of which semantic primitives are interpreted. As illustrated by examples like (27) and (28), the conceptual interpretation of lexical items—or larger constituents—depends not only to their combinatorial relations in *sem*, but to a large extent to conditions in *ct* and *m*, which are subject to the principles and rules of *C*. And there is no reason to believe that the combinatorial structures in *C* are in any way simple correlates to those in semantic or syntactic structure. Hence, even if we were given the principles according to which semantic constants are to be interpreted—by conceptual shift, conceptual specification, etc.—we would still face the problem of accounting for the correlation between the combinatorial principles of semantics and of conceptual structure. I believe that at least the beginnings of a plausible story about that correlation can be told, if one starts with notions like Mental Model in the sense of Johnson-Laird (1982) or Discourse Representation in the sense of Kamp (1981), but that would go far beyond the limits of the present paper.

5. Language and Thought

Assuming that at least in principle an account of the relation between *sem*, the contextual setting *ct* and the conceptual interpretation *m* can be given along the lines indicated above, two final remarks might be appropriate. The first concerns the relation of *m* and *ct* to the “external” reality, i.e. to the things and situations linguistic expressions are used to talk about. The formal aspect of that relation can be captured in a number of ways, the most promising one, to my opinion, being the proposal developed in Kamp (1981) according to which a mental model *m* is true in a model *M* if and only if *m* can be embedded in *M*, that is, intuitively speaking, if *m* is a substructure of *M*, where *M* is the structure of the “external” reality. The substantial aspect of this relation, i.e. the way in which a speaker/hearer construes his experiences in terms of conceptual representations, is an empirical problem to be dealt with by theories of perception, physical action, rational explanation, emotion, social interaction, and is the outcome of the way in which the respective mental systems interact. Notice that on this account truth and reference are defined over conceptual representations, not over linguistic expressions directly. This observation leads to the second remark. As logical relations and operations are to be determined by preservation of truth values, they must be defined over conceptual, rather than semantic or syntactic representations. This can easily be shown by examples like (48), which—unlike (49)—is not a valid argument under any reasonable interpretation.

- (48) The president is elected by public vote. Mary's husband is the president.
Hence Mary's husband is elected by public vote.
- (49) The president is the first speaker. Mary's husband is the president.
Hence Mary's husband is the first speaker.

In other words, logical relations can be construed as relations between linguistic expressions only under highly restricted conditions, as the structure of thought is not in general to be identified with the semantic structure of linguistic expressions. This does not mean that language cannot be a very effective means to articulate and clarify conceptual representations. It indicates, however, that the meaning of a linguistic utterance—construed as the thought it expresses—cannot in general be identified with its semantic representation.

6. Appendix

The formal framework in terms of which I have characterized semantic representations is that of a Lambda-Categorical Language *LC* in the following sense.

- (i) *C* is a system of categories such that
 - (a) *C* contains a finite set of basic categories *S*, *N*, ...;
 - (b) if *a*, *b*₁, ..., *b*_{*n*} are categories, then (*a*/*b*₁ ... *b*_{*n*}) is a category.
 - (ii) *B* is a finite set of basic constants. Each element of *B* belongs to a category *a* of *C*.
 - (iii) For each category *a* of *C* there is an infinite set *V*_{*a*} of variables.
 - (iv) The expressions of *LC* are defined as follows:
 - (a) The elements of *B* and *V*_{*a*} are expressions of *LC*.
 - (b) If *E*, *E*₁, ..., *E*_{*n*} are expressions of category (*a*/*b*₁ ... *b*_{*n*}), *b*₁, ..., *b*_{*n*}, respectively, then [*E* *E*₁ ... *E*_{*n*}] is an expression of category *a*.
 - (c) If *X* is a variable of category *a*, and *E* is an expression of category *b*, then *X*[*E*] is an expression of category (*b*/*a*).
 - (d) If *X* and *E* are of category *a* and *X*[... *X*...] *E* is an expression of category *b*, then [... *E* ...] is an expression of category *b*.
- (iv)(c) defines lambda-abstraction, (iv)(d) defines lambda-conversion.
- Two final remarks. First, the system of categories defines a combinatorial syntactical structure for semantic representations. The way in which this combinatorial structure is related to the syntactic representations of a given language *L* is determined by the lexical entries that correlate expressions of *LC* to syntactic conditions of *L*. Second, the formal structure of *LC* provides a framework of representation, it does not constitute a formal theory of semantics. A theory of semantics must not only determine the substantial content of *C* and *B*, it must also specify substantial constraints over the set of possible expressions which might eventually lead to a formal system of a rather different character.

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Pour et contre l'analyse sémique

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1.1 Dans le langage, tout est sémantique: la grammaire ne l'est pas moins que le lexique, la parole en général et les langues ne le sont pas moins que le discours. Et ce qui n'est pas sémantique en soi-même, le "plan de l'expression", y est déterminé par "le sémantique" et peut d'ailleurs assumer à son tour des fonctions mimétiques de symbolisation directe ou d'évocation. Parler de sémantique équivaut par conséquent à parler de toute la linguistique. Ici, pourtant, on entendra par "sémantique" uniquement la sémantique lexicale des langues, en particulier sous sa forme fonctionnelle ou structurale ("lexématique"); et à l'intérieur de la lexématique on se limitera à examiner brièvement deux questions connexes: celle de la légitimité et validité de l'analyse sémique (analyse des lexèmes ou contenus lexicaux en traits distinctifs) et celle du statut des lexèmes secondaires ("dérivés") vis-à-vis des paradigmes lexématiques de base ("champs lexicaux").

1.2.1. Les principes et les problèmes fondamentaux de la lexématique, nous les avons exposés ailleurs.¹⁾ Rappelons ici que, parmi les contenus linguistiques, la lexématique n'étudie en propre que le *signifié* et que, dans le domaine du signifié, elle ne concerne directement que le *signifié lexical*. Nous distinguons, en effet, trois types principaux de "contenu" linguistique: la *désignation* (c'est-à-dire la référence au monde extra-linguistique: "choses" et "états de choses", réels ou imaginaires), le *signifié* (contenu d'un signe ou d'une construction en tant que donné exclusivement par une langue déterminée, et non pas, par exemple, par la "connaissance des choses") et le *sens* (contenu propre d'un acte de parole ou d'un "discours": par ex. "question", "prière", "invitation", "ordre", "constatation" etc.).²⁾ Et à l'intérieur du signifié nous distinguons: le signifié *lexical* ("das Was der sprachlichen Erfassung": par ex. le signifié que les lexèmes de la série esp. *blanco—blancura—blanquear—blancamente* ont en commun), le signifié *catégoriel* ("das Wie der Erfassung", les catégories verbales: substantif, verbe etc.), le signifié *instrumental* (signifié des instruments gram-

1) Dans une série de travaux publiés, en français et en allemand, à partir de 1964, et réunis, en traduction espagnole, dans nos volumes *PSE* et *GSU*. A propos de l'état actuel de la sémantique structurale, v. E. Coseriu et H. Geckeler, *Trends in Structural Semantics*, Tübingen 1981, et H. Geckeler, "Progrès et stagnation en sémantique structurale", *Logos semantikos. Studia linguistica in honorem Eugenio Coseriu*, Berlin et Madrid 1981, III, pp. 53-69.

2) La désignation devrait être l'objet propre d'une linguistique de la "parole en général" et le sens, celui de la linguistique du discours (ou "du texte"). La plupart des problèmes qu'on voudrait attribuer à la soi-disant "pragmatique" sont, à notre avis, des problèmes concernant le "sens" et, par là, des problèmes de la linguistique du discours.

maticaux: par ex. "pluralisateur", "actualisateur"), le signifié *syntactique* (signifié des constructions grammaticales: par ex. "singulier", "pluriel", "actif", "passif") et le signifié *ontique* (le statut existentiel attribué à l'état de choses signifié par une phrase: par ex. "affirmatif", "interrogatif").

1.2.2. Quant aux types de rapports structuraux propres du signifié lexical, nous y distinguons des structures *paradigmatiques* et des structures *syntagmatiques* (combinaisons de lexèmes déterminées par la langue). Parmi les structures paradigmatiques, nous distinguons deux types primaires: le *champ* (par ex. le champ des adjectifs français concernant la température: *froid*, *tiède*, *chaud* etc.) et la *classe* (par ex. "animé"—"non-animé", "personne"—"non-personne", "transitif"—"intransitif"), et trois types secondaires, correspondant à la formation des mots ou "dérivation": la *modification* (par ex. *maison*→*maisonnette*, *rouge*→*rougeâtre*, *venir*→*revenir*), le *développement* (par ex. *beau*→*beauté*, *partir*→*départ*) et la *composition*, divisée à son tour en composition "prolexématique" ou générique (par ex. "pronom générique"+*jouer*→*joueur*) et composition "lexématique" ou spécifique (par ex. all. *Tier*+*Haus*→*Haus-tier*). Les structures syntagmatiques ("solidarités") sont, selon nous, de trois types: *affinité* (par ex., en latin: "personne"—*senex*), *sélection* (par ex. all. "véhicule"—*fahren*) et *implication* (par ex. fr. *cheval*—*alezan*).

1.2.3. Pour des raisons que nous ne pouvons pas énumérer ici, tous ces rapports peuvent être établis de façon cohérente seulement dans une "langue fonctionnelle", c'est-à-dire dans un système linguistique unitaire et homogène (un seul "dialecte", un seul "niveau" et un seul "style de langue")—et non pas dans une "langue historique" toute entière (comme le français ou l'anglais), qui, normalement, est un ensemble de systèmes connexes—; et à l'intérieur d'une langue fonctionnelle, ils doivent être établis dans la "technique libre" (et non pas, en même temps, dans le "discours répété": locutions figées, "phraséologie") et dans le lexique "de langue" (non pas dans le lexique nomenclatureur et terminologique).³⁾

1.2.4. L'analyse sémique, dont nous nous occupons dans la suite, ne constitue qu'un aspect de la lexématique: c'est l'identification des traits qui fonctionnent dans les rapports oppositifs entre les lexèmes, en particulier à l'intérieur d'un paradigme lexical (mais aussi entre des sections de paradigmes et entre des paradigmes tout entiers).

2.0. L'analyse sémique, sous la forme qu'on appelle "analyse compositionnelle", a été beaucoup critiquée dans les derniers temps et, à notre congrès de Vienne, M. Lyons a pu constater avec une certaine satisfaction qu'elle se trouve à présent en régression. La sémantique structurale, telle que nous la concevons, ne coïncide pas, on l'a vu, avec l'analyse sémique, puisqu'elle englobe tous les

3) Pour tous ces problèmes, v. en particulier nos études "Les structures lexématiques", dans *Probleme der Semantik*, publ. par W. Th. Elwert, Wiesbaden 1968, pp. 3-16, et "L'étude fonctionnelle du vocabulaire: Précis de lexématique", *Cahiers de Lexicologie*, XXIX, 1976, pp. 5-23 (trad. esp. dans *PSE*, pp. 162-184, et dans *GSU*, pp. 206-238).

types de rapports structuraux fonctionnant dans le lexique. Et notre analyse sémique ne coïncide pas, en ce qui concerne son statut et son sens, avec l'analyse "componentielle" répandue parmi nos collègues nord-américains. En outre, la constatation de M. Lyons ne vaut que pour un certain type d'analyse "componentielle" à l'intérieur d'une orientation déterminée de la linguistique. Cependant, étant donné que, sous plusieurs aspects, les critiques avancées visent aussi l'analyse sémique de type "européen" (continental)—que, du reste, elles ne distinguent pas de l'analyse "componentielle"—, nous nous proposons d'examiner leur bien-fondé. Dans ce but, nous choisissons les critiques formulées par M. Lyons et par Mme Renate Bartsch,⁴⁾ non pas, bien entendu, pour faire "la critique de la critique", mais pour préciser et justifier notre propre conception de l'analyse sémique et de ses fondements.

2.1.1. Le point de départ de M. Lyons, c'est le statut qu'il attribue au signifié. D'après lui la notion de signifié ("sense") est une notion "syncatégorématique" (=relationnelle) et, par conséquent, on ne devrait pas l'hypostasier en lui attribuant une existence de "chose", comme on le fait par ex. quand on conçoit la grammaire comme un mécanisme unissant des signifiés déjà donnés à telles ou telles expressions. Le signifié ne serait en chaque cas que la somme des règles de l'emploi d'un signe matériel par opposition au signifié (dans le même sens) d'autres signes de la langue, ou du moins l'on n'en pourrait parler en linguistique scientifique que dans ce sens et sans se rapporter à l'esprit ou à la conscience (*mind*), ce qui impliquerait tous les dangers de l'introspection. C'est, on le voit, le principe du behaviorisme linguistique (que M. Lyons appelle "empirisme"). Quant à la nature relationnelle du signifié, M. Lyons la soutient⁵⁾ au moyen de l'analogie de ce que B. Russell dit à propos de la notion de "longueur". Ainsi, la "longueur de x " ne serait que le résultat de l'appréciation d'une propriété de x par rapport à la même propriété d'un autre objet, prise en tant qu'unité de mesure: de la même façon, le "signifié de x " ne serait que le rapport entre une propriété de x et la même propriété dans y , z etc.

Or, il est bien vrai que "la longueur de x " n'est que le rapport proportionnel entre la longueur de ce x et une longueur prise en tant qu'unité de mesure (ce qui, du reste, s'applique à toutes les dimensions explicitement ou implicitement "mesurées"), mais cette analogie ne dit rien à propos de la nature du signifié; et elle est entièrement inadéquate. En effet, il s'agit, dans l'exemple de Russell, de la notion "longueur de x " et non pas de la notion de "longueur" tout court, en tant que type de dimension différent des types qu'on appelle *largeur*, *hauteur*, *profondeur* etc. De même, le signifié d'un signe de la langue est délimité par le signifié d'autres signes de la même langue, mais il est délimité précisément en tant que signifié, c'est-à-dire en tant que fait d'une

4) J. Lyons, "Basic Problems of Semantics", *Proceedings of the Twelfth International Congress of Linguists*, Innsbruck 1978, pp. 15, 19, et, surtout, *ITL*, p. 470 ss. R. Bartsch, "Comments on Lyons, Basic Problems of Semantics", *Proceedings*, cit., p. 22.

5) *ITL*, pp. 443-444.

nature déterminée: *signifié* n'est pas simplement le nom de l'ensemble de ces rapports. D'autre part, le signifié n'est pas une propriété "adjective" du type de la longueur: lorsqu'on dit qu'un signe matériel ou un "signifiant" a un signifié (façon de dire discutable en logique et en épistémologie, mais consacrée par l'usage dans nos langues), on ne dit pas qu'il présente physiquement une propriété—la "significativité"—mais qu'il correspond ou qu'il est référentiel à un fait mental, à un contenu de la conscience, non constatable en tant que tel dans le "monde" extérieur. C'est-à-dire que le signifié n'est pas réductible aux règles formelles de l'emploi d'un signe matériel, puisque c'est bien plutôt leur fondement: c'est le fait notionnel, le savoir unitaire qui justifie l'emploi d'un signifiant. On sait depuis Aristote que le signifié est le contenu unitaire de la conscience, la notion ($\tau\acute{o} \epsilon\nu \sigma\eta\mu\alpha\tau\acute{\iota}\kappa\epsilon\nu$)⁶⁾ qui possibilite et justifie la désignation à l'aide de la même expression de tous les objets ou faits reconnus comme appartenant (ou rapportables) à la même "espèce": ce n'est pas le fait d'appeler *cheval* un cheval mais la raison pour laquelle on l'appelle *cheval* (et non pas *âne* ou *mulet*). Et M. Lyons le sait bien, puisque dans la pratique il emploie à chaque pas cette même notion de signifié qu'il repousse en théorie. Du reste, même les empiristes les plus convaincus sont arrivés, bien que par toute une série de détours, à une conception du moins parfaitement analogue (sinon identique) à celle d'Aristote et de toute la tradition "rationnaliste"; cf. par ex. la formule de Ch. W. Morris: "Those conditions which are such that whatever fulfills them is a denotatum will be a *significatum*";⁷⁾ formule à laquelle, en linguistique, il faudrait ajouter pourtant qu'il s'agit de "conditions" intuitivement connues des sujets parlant une langue.

De même, le signifié n'est jamais "donné" par le contexte: il est toujours donné par la connaissance de la langue.⁸⁾ Le contexte ne fait que déterminer ultérieurement les signifiés, et préciser la désignation, et, dans le cas des signifiants homophones, il permet normalement d'identifier le signe effectivement employé (avec son signifié de langue). En effet, un signifié peut être employé dans des contextes tout à fait nouveaux ("inédits") et souvent un signifié se trouve en conflit avec le contexte, ce qui implique qu'il est identifié ou reconnu indépendamment de celui-ci; sinon, les expressions "fausses" ou extravagantes ne pourraient pas être identifiées en tant que telles. Sans doute, on apprend les signifiés dans des contextes. Mais il ne faut pas confondre les conditions empiriques de l'apprentissage avec la connaissance d'une langue. L'apprentissage d'une langue est toujours une activité créatrice: ce qu'on expérimente est dans chaque cas une désignation particulière et entièrement déterminée; et ce qu'on apprend (c'est-à-dire, ce qu'on crée à partir de cette désignation particulière)

6) *Soph. El.* 165a, 11 ss. et *Metaph. Γ.* 1006a, 29 ss. Cf. notre article "Bedeutung und Bezeichnung bei Aristoteles", *ZPSK*, 32, 1979, pp. 432-437.

7) *Signs, Language, and Behavior*, N. York 1946, p. 30.

8) Ainsi *get*, dans *I'll go to the shop and get some bread* (ITL, p. 453), n'est pas "synonyme" de *buy*: il signifie simplement "get", et non "buy". Il y a dans ce cas (peut-être) coïncidence dans la désignation, mais non pas dans la signification.

est un signifié, une possibilité infinie de désignation, dont la désignation expérimentée n'est qu'un exemple.

Il est bien vrai qu'il ne faut pas hypostasier le signifié, le considérer en tant que chose existant en soi (comme dans le cas cité par M. Lyons), puisqu'il n'est que le côté entièrement mental d'un signe, correspondant au côté "matériel" de l'expression, dont il ne peut pas être séparé. Mais distinguer—penser un fait comme distinct et autonome—ne signifie pas séparer ni hypostasier. Et, en effet, tout signifié peut être pensé indépendamment de l'expression, c'est-à-dire qu'il peut être dissocié de son expression (et, éventuellement, associé à d'autres expressions). Les linguistes le font à chaque pas: lorsqu'ils en parlent, lorsqu'ils le définissent ou lorsqu'ils constatent des "lacunes" sémantiques (c'est-à-dire, des signifiés possibles mais non exprimés). Et les sujets parlants le font aussi, par ex. dans les cas d'interférence linguistique (la plupart des fois: signifiés d'une langue associés aux expressions d'une autre langue) ou quand ils corrigent des formes "fausses".

2.1.2. Quant à l'analyse componentielle fondée sur l'autonomie admise ou supposée du signifié, M. Lyons la critique en particulier: a) parce que les traits distinctifs identifiés par cette analyse ne sont pas en eux-mêmes universels, comme on le prétend; b) parce que les traits distinctifs, dans la même langue, pourraient être différents chez des sujets parlants différents et, dans des langues différentes, différents selon les milieux culturels extra-linguistiques; et c) parce que le "statut cognitif" des traits distinctifs identifiés serait douteux, le même lexème pouvant être défini par des traits différents dans des proportions sémantiques différentes. Ainsi, dit-il, un enfant anglais pourrait distinguer *man* et *woman* par des traits autres que ceux qu'acceptent les adultes et dans un milieu culturel différent du nôtre "homme" et "femme" pourraient être distingués, non pas par le trait "sexe", mais d'accord aux rôles assumés par les sexes dans la communauté. Et en ce qui concerne le statut cognitif (la réalité) des traits, *brother* et *sister*, par ex. pourraient être définis par des traits différents selon la dimension sémantique envisagée en chaque cas.

2.1.3. Or, tous ces arguments sont impropres, et M. Lyons l'admet lui-même presque explicitement dans plusieurs cas.

En effet, et tout d'abord, l'analyse sémique bien comprise, en tant qu'analyse linguistique, ne préjuge rien à l'égard de l'universalité des traits qu'elle identifie. Elle ne suppose pas l'universalité comme donnée d'avance: ni dans le sens de l'universalité essentielle ou nécessaire, ni dans le sens de l'universalité empirique, ni, finalement, dans le sens des universaux "sélectifs", c'est-à-dire d'un ensemble déterminé de traits distinctifs à l'intérieur duquel chaque langue en choisirait un certain nombre.⁹⁾ Elle n'est pas la conséquence mais la base et le point de départ de la recherche des universaux dans ce domaine. C'est-à-dire qu'elle se limite à constater les oppositions qui fonctionnent dans

9) À propos de ces types d'universaux, v. notre rapport "Les universaux linguistiques (et les autres)", *Proceedings of the Eleventh International Congress of Linguists*, Bologne 1974, en part. pp. 48-54 (trad. esp. dans *GSU*, pp. 151-164).

les langues et les traits distinctifs qui en résultent. Ce n'est qu'après coup qu'on pourra éventuellement constater que certains traits se présentent dans toutes les langues connues et l'on pourra se demander s'il s'agit dans chaque cas d'universalité simplement empirique ou d'universalité nécessaire ou analytique (déduisible de la notion même de langue). Quant à l'ensemble des traits constatés, celui-ci sera en tout cas limité, les langues existantes n'étant pas en nombre infini, mais on aura de bonnes raisons de douter, même d'avance, de l'universalité nécessaire de cette limitation, puisqu'on peut toujours imaginer comme possibles et non contradictoires des traits non constatés dans aucune langue. Le fait de supposer au préalable et sans fondement l'universalité des traits distinctifs ou de certains traits est une erreur ou une illusion—peut-être contingente—d'un certain courant de la linguistique contemporaine et n'affecte en rien une analyse componentielle raisonnable et "réaliste" (respectueuse de la réalité des langues).

En ce qui concerne l'enfant anglais, il y a deux possibilités: ou bien les traits pertinents de la distinction qu'il fait sont au fond "masculin" et "féminin" (même s'il comprend ou conçoit à sa façon la "masculinité" et la "féminité"), ou bien ce sont des traits effectivement différents, et dans ce cas son système est à cet égard différent du système des adultes; mais dans les deux cas l'argument de M. Lyons n'exclut ni l'existence des traits distinctifs ni la possibilité de les identifier: en effet, il dit uniquement qu'ils peuvent être différents, ce qu'on peut concéder d'avance. Il en est de même en ce qui concerne les milieux culturels différents: ou bien les rôles sociaux sont attribués constamment aux sexes déjà distingués et nommés par la langue—et dans ce cas ces rôles n'affectent en rien la lexématique de la langue respective—, ou bien les rôles seulement, et non pas les sexes, y sont distingués et nommés (et dans ce cas, dans la langue respective—en supposant qu'elle ait précisément nos mots *homme* et *femme*—on devrait pouvoir employer ces mots indépendamment du sexe des individus désignés). Et ici encore, l'argument n'infirmes pas la recherche des traits distinctifs lexématiques dans son sens propre.

Quant aux traits définitoires différents dans des proportions sémantiques différentes, il faut remarquer qu'un signifié n'est jamais "défini" (délimité) au moyen d'un seul trait distinctif, et moins encore par un trait commun résultant d'une proportion (comme dans l'exemple de M. Lyons): il n'est délimité que par l'ensemble des oppositions dans lesquelles il fonctionne dans la langue et par l'ensemble des traits qui y correspondent. Par conséquent le statut cognitif des traits distinctifs n'est nullement douteux du fait que certains traits ne fonctionnent pas dans telle ou telle opposition particulière: c'est la prémisse même de l'analyse sémique. Mais, bien entendu, on ne peut pas, se fondant par ex. sur l'analyse des objets réels désignés, admettre des traits distinctifs qui ne fonctionnent dans aucune opposition de la langue considérée; cependant, dans ce cas le statut cognitif des traits n'est pas douteux non plus: linguistiquement ils n'existent pas, même si leur "pendant" réel est constant dans les objets désignés; en sémantique linguistique, ce sont des traits non pertinents.

2.1.4. Au fond, M. Lyons repousse l'analyse componentielle à cause de sa conception explicite du signifié et de la méthode linguistique, c'est-à-dire parce qu'il ne voudrait pas avoir recours à l'introspection ni être "mentaliste". Mais c'est là le défi lancé à la linguistique par son objet. L'intériorité du signifié est un fait dont toute linguistique doit tenir compte et qu'on ne peut pas ignorer. Sinon, on se résigne à parler d'autre chose et non pas de son objet propre et, par là, tout en voulant être plus "scientifique", à ne pas l'être, n'étant pas "objectif" dans le sens propre de ce terme. L'objectivité scientifique, c'est l'adéquation à l'objet de chaque science; de ce fait, l'objectivité de la linguistique ne peut et ne doit pas être celle des sciences naturelles.

2.2.1. Pour sa part, Mme Bartsch n'est pas antimentaliste et elle n'est pas contraire à l'analyse "componentielle". Mais elle critique cette analyse en tant que méthode, en particulier l'idée selon laquelle un signifié lexical ne serait qu'un ensemble non structuré de traits distinctifs additionnés les uns aux autres dans un ordre quelconque. Je ne sais pas si une telle analyse sémique existe; en tout cas, ce n'est pas la mienne; si elle existe, Mme Bartsch a parfaitement raison de la critiquer sur ce point. Mais, à son tour, elle voudrait y introduire de l'ordre en rapprochant l'analyse sémique de l'analyse syntaxique, ou bien même en identifiant les deux types d'analyse, c'est-à-dire en concevant l'analyse sémique comme identification d'une syntaxe implicite dans les lexèmes. Ainsi, dans le cas de *to run*, analysé comme: "to move fast with one's legs covering a distance", on aurait une syntaxe adverbiale, tandis que dans le cas de *bachelor* on aurait une syntaxe prédicative et copulative, puisque *x runs* ne peut pas être analysé comme étant: "*x moves + x is fast + x is with his legs + x covers a distance*", tandis que *bachelor* constituerait effectivement le produit logique de "*x is a man*" + "*x is unmarried*".

2.2.2. Or, dans ces objections—même en laissant de côté le fait que "covering a distance" pourrait bien être interprété comme "*x covers a distance*"—il y a du moins deux points vulnérables. Dans le premier exemple, il ne s'agit pas, en lexicatologie, d'analyser *x runs* mais d'analyser *to run*, de sorte que le *x* déterminé est celui qui, dans la formule respective, est représenté par *to move*; et dans le second exemple il ne s'agit pas du même *x* dans les deux phrases coordonnées: dans la première, c'est un *x* non nommé, tandis que dans la seconde, c'est un *x* qui est déjà "man". Mais ce qui est plus grave, c'est que l'interprétation proposée attribuée au langage primaire ("object-language") ce qui appartient au métalangage de l'analyse. Il est vrai que dans la syntaxe explicite de ce métalangage un verbe sera souvent déterminé par des adverbes et un substantif, par des adjectifs (si la langue employée fait cette distinction). Et dans d'autres cas on aura des agents différents (cf. all. *essen*—*fressen*), des compléments d'objet direct—une langue peut distinguer par ex. "manger (des fruits)" et "manger (de la viande)"—, des compléments indirects—par ex. "donner (à un supérieur)" et "donner (à un inférieur)"—, etc. Mais ces "adverbes", "adjectifs" etc. ne sont que les noms en métalangage des traits distinctifs qui, en eux-mêmes, n'ont aucune nature catégorielle ou grammaticale définie.

(n'étant que des formants lexicaux) et ne fonctionnent pas sur l'axe syntagmatique mais sur l'axe paradigmatique. C'est-à-dire que—erreur typique de la grammaire transformationnelle—l'interprétation proposée projette dans l'analyse l'axe paradigmatique sur l'axe syntagmatique et attribue la syntaxe de l'analyse aux faits analysés; en même temps, cela supprime la différence entre les lexèmes primaires et les mots composés, qui impliquent effectivement une "syntaxe" de type propositionnel. Il est bien vrai que les traits distinctifs ne constituent pas des ensembles désordonnés. Mais, du point de vue lexématique, l'ordre en est donné par les oppositions dans lesquelles les lexèmes fonctionnent, de sorte que l'on peut avoir des déterminations simultanées pour plusieurs lexèmes. Ainsi, dans le cas de *to run*—en admettant pour le moment que l'analyse proposée soit exacte en ce qui concerne l'identification des traits distinctifs—, l'ordre des traits ne serait nullement "fast"—"with one's legs"—"covering a distance", mais bien plutôt "(to move)"—"covering a distance" (trait valable pour *to walk*, *to run*, *to creep*, *to crawl*)—"with one's legs" (trait qui opposerait *to walk* et *to run* à *to creep*, *to crawl*)—"fast" (trait qui opposerait *to run* à *to walk*). Mais en face des emplois apparemment "polysémiques" de *to run*, on pourra se demander par ex. si le trait "with one's legs" y fonctionne effectivement et si ce verbe s'oppose directement à *to walk* et non pas plutôt à *to go* (dans lequel le trait "with one's legs" n'est pas pertinent). Dans l'analyse sémique propre de la lexématique il ne s'agit pas d'analyser un signifié lexical isolé, ni d'analyser les faits désignés qui y correspondent, mais d'établir la structure des rapports lexicaux oppositifs telle qu'elle est donnée par la langue considérée.

2.3. Il faut remarquer aussi que l'analysabilité en traits distinctifs n'est que le corollaire en méthode du principe de l'opposition distinctive et que ce corollaire ne vaut pas dans le sens contraire: il n'implique pas que les lexèmes "se composent" de traits distinctifs ni qu'ils soient synthétisés à partir de ces traits dans l'acte de parole (ou de "production de phrases"). En eux-mêmes, les lexèmes primaires correspondent à des intuitions unitaires et ils ne sont dans aucun sens le produit d'un assemblage de traits distinctifs déjà donnés. Ils ne présentent des traits distinctifs que parce qu'ils entrent en opposition avec d'autres lexèmes: ce sont les traits distinctifs qui existent en vertu des oppositions, non pas le contraire. De même, dans l'histoire d'une langue, les traits distinctifs apparaissent et disparaissent avec l'apparition et la disparition des oppositions: en tant que traits non pertinents, ils peuvent exister avant et après l'époque de leur existence fonctionnelle.

3.1. Mais voyons ce que M. Lyons voudrait mettre à la place de l'analyse sémique. Il distingue, on le sait, toute une série de types logico-formels d'oppositions,¹⁰⁾ types tels que l'hyponymie (par ex. *chêne* par rapport à *arbre*), l'incompatibilité (par ex. *vert*—*rouge*) et l' "oppositeness of

10) *ITL*, pp. 453-470. Nous ne considérons pas ici le modèle élargi que M. Lyons présente dans son ouvrage en deux volumes, *Semantics*. Cambridge 1977.

meaning", distinguée à son tour en complémentarité (par ex. *vivant—mort*), antonymie (*grand—petit*, *large—étroit*) et "converse-ness" (*vendre—acheter*, *husband—wife*).

3.2.1. Ce sont des distinctions intéressantes, bienvenues et utiles à plusieurs égards, en particulier pour l'établissement de la typologie des champs lexicaux et des classes lexicales. Mais, tout d'abord, les relations lexicales visées par ces types—qui, du reste, ne concernent pas seulement le lexique—ne sont pas de la même nature. L'hyponymie et l'incompatibilité correspondent à des relations sémiques à l'intérieur des champs lexicaux, tandis que l'"oppositeness of meaning" est de nature classématique, fonctionnant en principe dans toute une catégorie verbale et même dans plusieurs catégories. D'autre part, ces types sont précisément des types logico-formels, c'est-à-dire qu'ils ne concernent que la forme générique des oppositions sémantiques et non pas ces oppositions en tant que telles: dans le cas du lexique, les différences sémantiques entre les paradigmes lexicaux et entre les lexèmes à l'intérieur de chaque paradigme, différences qui devraient constituer l'objet par excellence de la sémantique structurale. En effet, les signifiés d'une langue (lexicaux ou autres) ne sont pas délimités uniquement par la forme générique de leurs oppositions mais aussi, et en premier lieu, par la forme spécifique de celles-ci et par les oppositions particulières en tant que telles. Ainsi, dire que *caillou* et *galet* sont des hyponymes de *pierre*, ou que *Mann* et *Frau* sont des hyponymes de *Mensch* (et qu'entre eux ils sont complémentaires), nous dit que les bases de comparaison (les signifiés que ces lexèmes ont en commun) sont, respectivement, "pierre" et "Mensch", mais ne nous dit pas en quoi consiste l'opposition entre *caillou—galet* et *pierre*, entre *Mann—Frau* et *Mensch* ni quelles sont les différences sémantiques entre ces lexèmes. Et dire que *neu* et *jung* sont des hyponymes d'un hypéronyme non réalisé, nous dit moins encore à cet égard. Certes, il est intéressant de constater que *long—court*, *grand—petit*, *haut—bas*, *schwer—leicht*, *âgé—jeune* présentent le même type formel d'opposition ("antonymie"), mais par là on ne sait pas quelles sont les différences entre ces couples de lexèmes et à l'intérieur de chacun d'eux. Il peut être légitime, dans le cadre d'une certaine conception, de ne pas vouloir dire quel est le signifié de tel ou tel lexème *x*; mais on devrait du moins pouvoir dire quelle est la différence entre le signifié de *x* et celui d'un lexème apparenté *y*, si l'on concède que la sémantique structurale s'occupe précisément des "différences de signifié". En outre, les langues sont différentes les unes des autres, en ce qui concerne la structuration de leur lexique, précisément au niveau des paradigmes lexicaux et des oppositions lexicales particulières; en principe, elles ne se distinguent pas au niveau des types formels d'oppositions, qui sont à peu près les mêmes (du moins dans les langues que nous connaissons). Ainsi, en face du couple antonyme all. *schwer—leicht*, on a en français les couples *lourd—léger* // *difficile—facile*; en face de fr. *grand—petit*, on a en anglais *big—small* // *great—little*. En roumain *gol* signifie "nu" ou "vide" (non pas "nu" + "vide", ni parfois "nu" et parfois "vide", mais une notion plus générale, sans différence entre "nu" et "vide") et

s'oppose à *acoperit* ou *îmbrăcat* ("couvert", "habillé") et à *plin* ("plein"); entre *gol*, d'un côté, et *acoperit* ou *îmbrăcat* et *plin*, de l'autre, il y a un rapport formel de complémentarité exactement comme dans le cas des équivalents français *nu*—*couvert*, *habillé*, et *vide*—*plein*, mais, évidemment, non pas dans le même sens. Et même s'il y a correspondance parfaite entre des couples de lexèmes, les oppositions peuvent être différentes entre les unités qui les constituent; ainsi, entre esp. *venir*—*ir* et entre it. *venire*—*andare* on a le même rapport formel de "converseness", mais, du point de vue du contenu, ce n'est pas la même "converseness": esp. *venir* signifie un mouvement vers l'endroit de la 1^{re} personne et esp. *ir*, un mouvement vers l'endroit de la 2^{de} ou de la 3^e personne, tandis qu'it. *venire* signifie un mouvement vers l'endroit de la 1^{re} et de la 2^{de} personne, et it. *andare*, un mouvement vers l'endroit de la 3^e personne uniquement. Il faut ajouter aussi que les structures paradigmatiques secondaires ("formation des mots") et les structures syntagmatiques propres du lexique ("solidarités lexicales") ne peuvent pas être étudiées de façon adéquate sans avoir recours à l'analyse structurale des paradigmes lexicaux primaires et des oppositions particulières, et que la sémantique structurale diachronique concerne en premier lieu les changements dans les paradigmes et dans les oppositions à l'intérieur de ceux-ci.

3.2.3. Tout cela signifie qu'on reste, pour ainsi dire, dans les limbes de la sémantique structurale en tant que discipline descriptive et historique si on en exclut l'analyse sémique. En même temps, on se résigne à être de très peu d'utilité dans le domaine de l'application des résultats de la recherche linguistique (en particulier, pour la lexématique, dans l'enseignement et dans l'apprentissage des langues, dans la théorie et dans la pratique de la traduction et dans la confection des dictionnaires unilingues et plurilingues).

4.1. Quant à la seconde question, je voudrais surtout profiter de cette occasion pour éliminer un fâcheux malentendu surgi à propos de ma conception de la sémantique structurale. On a cru notamment que (probablement?) je n'admets dans les champs lexicaux que des lexèmes primaires (non dérivés) et que j'en exclus tous les lexèmes secondaires (modifiés, développés, composés).¹¹⁾ Je ne sais pas comment ce malentendu a pu surgir, mais, puisqu'il est là, je dois préciser que ce n'est pas ce que je pense, parce que, tout simplement, cela ne correspond pas à ce qu'on constate dans les langues. En réalité, tout lexème de langue (non terminologique), un lexème dérivé non moins qu'un mot primaire, fonctionne dans un champ ou dans plusieurs champs, et tout champ peut en principe être constitué par des lexèmes primaires et secondaires. Ma distinction entre structures primaires et structures secondaires est une distinction de types de rapport structurés et non pas une distinction de classes exclusives de lexèmes. Il est vrai que j'emploie le terme de "primaire" aussi bien

11) H. Geckeler, *art. cit.*, pp. 63-64, et L. Lipka, "Methodology and Representation in the Study of Lexical Fields", *Perspektiven der lexikalischen Semantik*, publ. par D. Kastovsky, Bonn 1980, pp. 93-113.

pour les champs que pour tout terme constituant la base d'une dérivation quelconque (et qui, du reste, peut être à son tour un dérivé: cf. par ex. it. *passaggiare*→*passaggiata* et ensuite *passaggiata*→*passaggiatina*). Mais un champ est "primaire" en tant que type de structure, tandis que la base d'une dérivation est "primaire" en tant que point de départ d'une structuration secondaire. Les seuls dérivés que j'exclus effectivement des champs lexicaux sont les dérivés des mots pronominaux (qui n'ont pas de signifié lexical) et les dérivés des noms propres (qui, en tant que tels, n'entrent pas dans des oppositions de champ).

4.2. La question, par conséquent, ce n'est pas de savoir si les lexèmes secondaires fonctionnent dans des champs lexicaux mais plutôt de savoir dans quels champs et de quelle façon ils peuvent fonctionner. Or, à cet égard on ne peut dire que très peu dans l'état actuel de la lexématique descriptive, surtout en ce qui concerne la description exhaustive de différents champs. Nous devons nous limiter à quelques constatations provisoires d'ordre général.

Les modifications (par ex. les diminutifs ou les augmentatifs) fonctionnent normalement dans les mêmes champs que leurs bases, en tant que sous-divisions de celles-ci, et elles perdent, du point de vue sémantique, leur statut de modifications si elles passent à d'autres champs (c'est le cas de beaucoup de diminutifs en français actuel).

Les développements, s'ils ne sont que des conversions catégorielles (substantif→adjectif, adjectif→adverbe, verbe→substantif etc.; par ex. fr. *rouge*—*rougeur*—*rougir*), fonctionnent en principe dans les mêmes champs que leurs bases, en constituant, pour ainsi dire, des "couches" catégorielles superposées¹²⁾ et en rapport les unes avec les autres. Mais par ex. les développements qui impliquent un "cas" (comme all. *auf [den] Tisch*→*aufischen* ou fr. *en barque*→*embarquement*) fonctionnent dans un champ de signification différent de celui de leur base, tout en restant normalement dans le même "champ de désignation" (*Sachbereich*).

Les composés prolexématiques (par ex. les "noms d'agent": all. *lesen*→*Leser*, fr. *pomme*→*pommier*) fonctionnent dans des champs différents de leurs composants lexématiques—champs, du reste, ouverts, instables et génériques au niveau du système de la langue—, mais ils peuvent constituer (ou appartenir à) des champs de désignation contigus à ceux de ces mêmes composants au niveau de la norme de la langue. Finalement, les composés lexématiques (tels que all. *Fahrkarte*, *Apfelbaum*) fonctionnent en principe dans le même champ que leur lexème déterminé, en ne constituant que des sous-divisions de celui-ci, et ils perdent souvent leur statut de composés (on dit qu'ils ne sont plus "sentis" comme tels) s'ils se déplacent vers d'autres champs ou même vers d'autres sections du même champ (cf. all. *Bahnhof*, *Hochzeit*, *Handschuh*); mais ils peuvent appartenir à des champs de désignation tout à fait différents, selon leur lexème déterminant.

4.3. Mais il faut remarquer que, quand on étudie les dérivés dans leur

12) Cf. PSE, pp. 139-140.

fonctionnement dans les champs, on les étudie dans leurs rapports "statiques" avec d'autres lexèmes des mêmes champs (primaires ou dérivés), non pas du point de vue des procédés dynamiques qui les produisent.

5. Je suis heureux d'avoir pu éclaircir ce dernier point parce que cela m'amène aussi à mieux préciser le sens global de la sémantique structurale que je me suis proposé de développer. Cette sémantique, dans sa partie théorique et "générale", n'est pas, à proprement parler, un *modèle*, dans le sens courant de ce terme, mais bien plutôt une *heuristique*: c'est l'identification et délimitation des types de rapports sémantiques fonctionnant dans le lexique des langues.

La sémantique structurale, comme toute la linguistique fonctionnelle, est une discipline rationaliste et réaliste (ce qui, au fond, est la même chose): elle correspond—ou aspire à correspondre—à la réalité des faits linguistiques eux-mêmes et à l'intuition des sujets parlants (non pas à l'intuition qu'ils peuvent manifester de façon explicite, mais à l'intuition qu'ils manifestent implicitement dans leur activité même de parler et de comprendre). De ce fait, il n'est pas surprenant que des amorces de description structurale du lexique se présentent déjà dans la linguistique traditionnelle, par exemple dans les travaux sur les synonymes et les antonymes, et même dans la spéculation ancienne sur le langage: chaque fois qu'on a posé le problème d'une distinction lexicale, depuis Platon et Aristote, on a posé un problème de lexématique. Ce qui manque à la sémantique traditionnelle, ce n'est pas l'intuition des structures lexicales: c'est la délimitation stricte du signifié lexical par rapport à d'autres types de contenu et de signifié et l'identification et distinction cohérente des types des rapports lexématiques.

Ouvrages cités plusieurs fois

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Integrating Semantics and Pragmatics

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Introduction

Whether a given level of linguistic description is autonomous from an adjacent "higher" level is an issue of great concern in linguistics, regardless of the particular levels under discussion. The most visible example of this concern is the dispute within generative linguistics on the relationship between syntax and semantics. We have argued elsewhere that, from the viewpoint of artificial intelligence, these two levels must be integrated in language processing (Schank and Birnbaum, in press). Because language processing requires integrating knowledge of extremely diverse content, credence must be given to the possibility that, regardless of the descriptive utility of distinguishing between linguistic levels, no functional distinctions between them actually exist. Accordingly, in this paper we argue that semantics and pragmatics should be integrated as well.

Artificial intelligence (henceforth, AI) is concerned with developing theories of cognitive processing, and experimenting with computer implementations of those theories. Its emphasis on processing gives AI a unique perspective on language, clearly distinct from other paradigms for linguistic research. From this viewpoint, understanding the relationship between semantics and pragmatics (or any other aspects of language) involves understanding how, and when, each is utilized in the processes of language comprehension and production. An important methodological attribute of AI research is that it *must* address this issue, if it is to lead to process models capable of performing significant linguistic tasks, such as text understanding, question answering, and translation. In AI, one cannot arbitrarily draw boxes signifying modules that contain different kinds of knowledge, with arrows between them. Independent modules must actually work independently; if they do not, the distinctions must be rethought. Other paradigms for linguistic theory do not generally face this methodological pressure.

Implications of an integrated view

Our claim that semantics and pragmatics are integrated is, more precisely, the claim that they are *functionally* integrated. To put this another way, language processing does not employ a distinguishable, independent level of semantic processing or of semantic knowledge. Semantic knowledge is employed in

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language processing no differently from pragmatic knowledge, using the same inferential and memory capabilities as common-sense reasoning. Semantics is an inseparable part of pragmatics, i.e., of our general knowledge of the world and of how language is used.

What are the empirical consequences of this claim? The most profound psychological implication is that language processing does not involve the computation of a level of semantic representation independent from pragmatic knowledge, and serving as input to (or, in the case of production, output from) a separate pragmatic reasoning component. Understanding is achieved by the unified application of semantic and pragmatic knowledge. The meaning of a word or an utterance is represented as an integral part of memory, in the same way that other knowledge is represented. The concept of *memory* employed here is crucial. Linguistic theories can survive without an adequate conception of memory, but human language processors cannot.

For linguistic semantics, this view has two main implications. The first is that there is no "dictionary," only an "encyclopedia." That is, the lexicon is intimately connected with the rest of our knowledge and indistinguishable from it. The second point is that the notion of "literal meaning" is a problematic one. If the representation of the meanings of words and utterances involves structures that are inherently connected with the rest of our knowledge, it may not in general be possible to isolate sub-parts of those structures than can be identified as the literal content of those words or utterances. This view, then, calls into question the foundations of simple "bottom-up," purely combinatorial theories of meaning, such as Katz-Fodor or truth-conditional semantics.

Why should there be an independent semantics, and why have linguists tried to construct theories based on such a notion? The answer seems to be that the idea was a by-product of other doctrines. For example, Hjelmslev (1953) was able to subordinate semantics to an autonomous system of morphology and syntax by postulating a complete parallelism between structures of the "plane of expression" and those of the "plane of content." This rather ingeniously appeared to include semantics, while actually keeping it effectively out—a typical example of "how to do semantics without really doing it." The doctrine of autonomous syntax within generative linguistics has had a similar effect. If one takes the position that syntax is independent of semantics, then it is only natural to assume that semantics is independent of pragmatics as well.

The chief methodological implication of our view is that, since semantics is not independent of pragmatics, it should not be studied independently. Semantic theories must employ the kinds of large structures for the representation of common-sense knowledge that have been investigated in cognitive science, such as *frames* (Minsky, 1975; Charniak, 1978), and *scripts* (Schank and Abelson, 1977). Fillmore's (1976) work provides a particularly clear example of this approach:

There is in English... a semantic domain connected with what we might call the commercial event. The frame for such an event has the form of a scenario containing roles that we can identify as the buyer, the seller, the goods, and the money; [and] containing subevents within which the buyer surrenders the money and takes the goods and the seller surrenders the goods and takes the money... [The] whole commercial event scenario is available or "activated" in the mind of anybody who comes across and understands any of the words "buy," "sell," "pay," "cost," "spend," "charge," etc., even though each of these highlights or foregrounds only one small section of the frame. Each of these words... brings along with it simultaneously a ground and a figure, simultaneously a setting and the piece of that setting to which the word is pointing.

Historically, the standard objection to this conception of semantics appeals to the practical impossibility of actually specifying all of the knowledge that a human being has about the world. For example, Katz and Fodor (1963), in arguing against the utility of pragmatics in a semantic theory, claim that "a necessary condition that any variety of this kind of theory must satisfy is... that it is able to represent all the nonlinguistic information required by speakers to understand sentences..." They then go on to argue that "a complete theory of this kind is not possible in principle because to satisfy the above necessary condition, it would be required that the theory represent *all* the knowledge speakers have about the world." The flaw in this argument is that being *able* to represent all the relevant knowledge is not the same as *actually* representing it. In other words, in order to satisfy Katz and Fodor's condition, an integrated theory of semantics and pragmatics need not actually represent all of this knowledge, any more than a semantic theory of the sort they envisioned would actually need to represent the meanings of all the words in some language. Rather, what is required in either case is the potential to do so in principle. The fact that as a practical matter it would be impossible to specify all of a speaker's world knowledge does not invalidate a theory of the sort we are proposing (Raskin, 1981, makes a similar point). The goal of such a theory is to show *how* some body of knowledge about the world is employed in language processing, assuming that the speaker/hearer possesses it.

A similar confusion underlies the persistent (although, presumably, not deliberate) misunderstanding of AI research on the part of some linguists and psychologists. In order to pursue a research project in AI, whether it be concerned with natural language, planning, or problem solving, some domain of knowledge must be chosen to provide examples and problems. Although the domain may be intrinsically interesting, more often than not it is boring (for example, toy blocks, restaurants, children's birthday parties, and water pump assembly have all been used), and no particular cleverness is needed to uncover those facts about the domain which must be known in order to perform the task. The point of the research in these cases is not such facts themselves, but

rather the methods of representing, organizing, and applying them in order to understand or generate language, or solve problems. To claim that AI research is concerned with "knowledge of the appropriate way to behave in restaurants" (Marshall, 1980) is simply to miss the point.

Inference and conceptual representations

How can large meaning structures of the sort we have alluded to, e.g., the commercial transaction scenario, be represented and used, and why are they necessary for language processing? What we want to illustrate here is that the consideration of processing issues can contribute to the design of conceptual representations. (We refer to semantics and pragmatics jointly as the *conceptual* level.) Although we will restrict our discussion to symbolic, or "propositional," representations, it should not be taken for granted that this is optimal for all domains.

The key role of conceptual representations is to facilitate plausible inference and memory processing. This view differs from the conception, common within linguistics, that the purpose of semantic representations is to explicate properties such as synonymy, anomaly, and logical entailment. Ultimately, either view is only justified to the extent that theories based on it are successful; but what counts as success is debatable. Once plausible inference and memory processing are seen as the essential functions of semantic representations, they become central issues for semantic theory, which must accordingly be concerned with processes that are not, in the conventional view, "semantic." Any semantic theory which fails to address these issues is not likely to shed light on how language is used to communicate ideas, which is, arguably, the fundamental scientific problem of language.

It can be useful to relate this issue to a common-sense appraisal of why and how people use language to communicate. It is a common observation that the utterances of a speaker do not explicitly communicate all that the hearer is intended to understand, and that the implicit content must be inferred. Often, what the hearer must infer does not follow necessarily from what he is told: plausible inference is required. It follows then that conceptual representations must support this inferential processing. Just as obviously, the goal of communication is often defeated if the hearer does not remember what has been communicated. Thus, conceptual representations must play a key role in memory.

The examination of how conceptual representations can be designed to support memory and inferential processing forms the foundation of *Conceptual Dependency* theory (Schank, 1975a). Consideration of these processing issues immediately refutes the possibility that the words and sentences of a natural language are themselves an acceptable representation of meaning, because words and sentences are ambiguous and elliptic. The sentence "Mary gave John a million dollars" implies that John possesses a million dollars, but the sentence "Mary gave John a kiss" doesn't imply that John possesses anything. It would be difficult to prevent erroneous inferences of this sort from being made, how-

ever, if the relevant rules had to be posed purely in terms of the words themselves.

Another serious problem with the use of natural language itself as a conceptual representation is that the same meaning, more or less, can be expressed in many ways that seem superficially quite different. A conceptual representation in which related meanings are represented to the greatest extent possible in related ways facilitates memory search, for example, in answering queries. Furthermore, to the extent that similarities in the meaning of two utterances are reflected in their conceptual representations, the plausible inferences that they share can be performed by shared rules, thus enabling a reduction in the total number of inference rules required. For example, whether you tell someone that "Fred bought a car from Jerry," or instead say that "Jerry sold a car to Fred," you can expect that the hearer understands that the car now belongs to Fred, that Jerry received some money from Fred in return for the car, etc. If the representations of these utterances are sufficiently similar, then the same rules can be used to accomplish this in both cases. This would also allow conceptual information to be shared among different languages in a model with multi-lingual capabilities. The parsimony of such a scheme is not merely an aesthetic feature; it has real processing consequences. For example, if rules are shared, then fewer rules must be learned, and each rule that is learned is of wider applicability and hence greater power.

In order to satisfy the above conditions, the number of primitive symbols out of which conceptual representations are constructed must be restricted as much as possible. "Verbosity" or redundancy in the representation vocabulary immediately cascades, causing further redundancy and complexity in inference and memory processing. The necessity for a restricted representation vocabulary can be further justified by considering the question of how the meanings of conceptual representations are themselves specified. The most coherent answer, from an AI perspective, is that the meaning of a representation is specified by its functional role in mental processing. In particular, then, the meaning of some symbol in a representation system depends on the representational structures in which it takes part, and on the inference rules that mention it. Therefore, as Hayes (1979) has pointed out, each symbol must play a role in many structures and inference rules in order to have some significant content or meaning. The way to accomplish that is to express as many facts and rules as possible using as few symbols as possible. It will not do to simply take every English verb as a predicate of the underlying conceptual representation without further analysis.

Another important way in which a conceptual representation can facilitate inferential processing is by delineating what inferences need to be made. What is already known about the utterance, concept, or proposition being represented, and what remains to be elaborated, should, as much as possible, be obvious by inspection. In slogan form, representations should supply expectations. Case frames are one style of representation that do this: the empty slots (unfilled

cases) indicate, at least in part, information that is missing and remains to be inferred.

Based on these considerations, a system for representing the meaning of natural language utterances was devised as part of Conceptual Dependency theory. The primitive vocabulary of the system allows for the representation of actions, objects, states and state changes, and causal relations. (Most of the design efforts were directed towards the representation of actions and causal relations; states are rather crudely handled, and objects hardly at all, although that has been remedied to some extent by Lehnert, 1980.) Each item in the primitive vocabulary has a set of associated conceptual cases delimiting the roles or attributes that are most crucial. All actions, for example, have an actor and an object; some have a source and a goal; and an instrumental action may optionally be specified. The number of primitive actions in the original system varied between ten and fifteen. Over time, some have proven more useful than others, and new ones have been introduced as new domains have been investigated (see, e.g., Schank and Carbonell, 1979). Five of the most ubiquitous, and hence most useful, are described below:

- PTRANS: to physically transfer an object from one location to another.
- ATRANS: to transfer an abstract relationship, such as possession or control, of an object from a donor to a recipient.
- MTRANS: to transfer information, either within or between individuals.
- ATTEND: to focus a sense organ on a stimulus.
- PROPEL: to apply a force to an object, in a given direction.

Associated with each primitive concept are plausible inference rules that may be useful in elaborating an instance of the concept or relating it to other concepts. To take a simple example, given an instance of an ATRANS one can infer something about the possessions of the actors involved. Rieger (1975) identified sixteen different classes of plausible inference, and developed methods for employing them in a computer program.

Inference and pragmatic knowledge

The representation of causal relations in Conceptual Dependency permits the assembly of individual conceptualizations into larger units called *causal chains* (Schank, 1975b). The importance of causal chains for language processing lies in the fact that the coherence (or lack of it) of the textual description of an episode depends primarily on whether or not a causal chain can be constructed to connect the events and states described. Consider the difference between the following two vignettes:

Mary gave John a million dollars. He bought a new car.

Mary gave John a million dollars. She visited her aunt in Milwaukee.

The first clearly has a coherence that the second lacks, because a causal chain can be constructed to connect the events described in the first case but not the second. The construction of a causal chain representation in this case, as in most, requires that intermediate actions and states, left implicit in the description, be inferred. Here, one must infer the intermediate state that John possessed a million dollars, which was the result of Mary's gift to him, and which enabled his subsequent purchase of the car.

These sort of pragmatic inferences are just as necessary for constructing the conceptual representation of sentences as of texts. For example, consider the sentences formed by conjoining the two sentences in each of the above vignettes:

Mary gave John a million dollars and he bought a new car.

Mary gave John a million dollars and she visited her aunt in Milwaukee.

Once again, the first is coherent in a way that the second is not. This difference makes it clear that the presence or absence of a causal chain can be a crucial attribute of an individual sentence's conceptual representation; and the construction of such a causal chain depends upon pragmatic inference.

The use of inference in constructing causal chain representations of texts is another illustration of its central role in language understanding. This role, in turn, imposes new requirements on our model of the understander's inferential capabilities. In many cases, the result and enablement inferences that can be derived "bottom-up" from the concepts explicit in a text are insufficient for constructing a causal chain even though the text is in fact causally coherent. For example, compare the following two stories (from Schank and Abelson, 1977):

Fred went to a park. He asked the midget for a mouse. He picked up the box and left.

Fred went to a restaurant. He asked the waitress for coq au vin. He paid the check and left.

Superficially these two stories are quite similar, yet the second is coherent in a way that the first is not. We understand the relationship between the events mentioned in the second story because we can bring our knowledge of what normally happens in restaurants to bear. This enables us to construct a causal chain representation, based on the plausible inference of many events which are not explicitly mentioned in the story, for example, that Fred probably ate the coq au vin.

The ability to employ world knowledge in understanding requires large conceptual structures that can be used, when deemed relevant, to supply the necessary contextual knowledge in a "top-down" fashion. Practically the simplest structure of this type is a *script* (Schank and Abelson, 1977), which is used to represent information about stereotypical episodes. A well-developed

script is really just a prepackaged causal chain representing a common sequence of events. Using scripts, an understander can link together concepts that cannot be related by their superficial features, by relying on a memory structure in which they are already linked together.

The prototypical example of a script describes what happens in a restaurant. It is broken up into scenes that describe entering, ordering, eating, and leaving, in terms of Conceptual Dependency actions and states linked by causal and temporal relations. Scripts are employed roughly as follows: When a script is referenced, e.g., by the mention of the word "restaurant," it is activated and the events described in the text are compared to those in the script. If there is a good enough match, then the script is instantiated, i.e., those events in the script which are not mentioned in the text are inferred, resulting a complete causal chain representation. Cullingford (1978) worked out the details of this process, and constructed a computer program (SAM) that used scripts to understand simple newspaper stories.

Reconstructive memory

In experimental research aimed at elucidating the psychological role of scripts, Bower, Black, and Turner (1979) discovered that, when presented with stories about related (but distinct) episodes, subjects exhibited recognition confusions with respect to which story had actually mentioned a scene that both episodes could be expected to contain. For example, subjects would read a story about a doctor's appointment and another story about a dentist's appointment. If one of the stories contained a reference to the waiting room, then even though the other had not, subjects tended, mistakenly, to judge that it had on a recognition memory test.

The implication is that different aspects of a story (or a real experience), once understood, are stored in different memory structures. For example, in the case of doctor's and dentist's appointments, it seems that the part of the experience involving waiting rooms is stored in a structure specific to waiting rooms, which is somehow shared by the conceptual structures concerning doctors and dentists. It follows that the retrieval of the experience must be *reconstructive*, pulling together memories from different structures, with the concomitant possibility for the sorts of confusions that Bower *et al.* observed.

This explanation suggests that the structures which store memories of experiences must be the same structures that provided the knowledge needed to understand those experiences in the first place. In other words, a theory of the conceptual structures used in processing must also be a theory of memory. The view of memory that emerges from all of this is one in which different memory structures organize different kinds of information, and are responsible for supplying that information to enable the interpretation of the various aspects of an input. Thus scripts, for example, are not static data structures that are stored whole in memory; rather, they are assembled as needed to understand an input. Conversely, different aspects of an input will be stored with the

different structures that supply the information relevant to those aspects. That is, memories of a given experience will be broken up and distributed among the structures used in understanding that experience.

The smallest conceptual structure that coherently organizes a set of expectations is a *scene*. A scene is a memory structure which groups together actions and states relating to a shared purpose, and usually in a shared setting in time and space. Specific memories are stored in scenes, indexed by some identifying feature, that is, some way in which they differ from the general actions and states described by the scene. For example, WAITING-ROOM is a scene which is organized around a purpose (waiting for the professional service), and contains such information as what it looks like (chairs and coffee table with magazines) and what you have to do (check in with the receptionist and wait for your name to be called). The WAITING-ROOM scene is useful in understanding any situation involving a visit to a professional for some service in which the client must wait someplace to be served. That is, it is shared by the conceptual structures representing knowledge about different types of professional service.

The conceptual structures that group together the various scenes necessary to understand a given situation (or a text describing that situation) are *memory organization packets*, or MOPs (Schank, 1980). For example, the knowledge needed to understand everything involved in a doctor's appointment centers around many different purposes, of several actors, at different times and locations—e.g., the waiting room; undergoing medical tests; making a new appointment; paying the bill. Some of these scenes are part of the doctor's appointment MOP, for example, undergoing medical tests. Other MOPs also contribute scenes; for example, bill paying is part of the contractual arrangement which is implicitly entered upon when being treated by a doctor. So, MOPs can refer to other MOPs as well as scenes.

While the original motivation for developing a reconstructive theory of memory came from the inability of scripts to account for recognition confusions, explaining these confusions is not really the point of the theory. By organizing knowledge in small, scene-sized chunks that can be shared by different MOPs, we gain flexibility and efficiency (Charniak, 1978). But more importantly, by sharing scenes, information learned in one situation, and stored in the appropriate scene, is made available in other situations which employ the same scene. Thus, the crucial function of reconstructive memory is that it facilitates learning (Schank, in press).

Memory structures and meaning

How can the constructs introduced above be used to address our original concern of representing the meaning of natural language utterances? The application is straightforward when some word in the utterance directly asserts an instance of some large conceptual structure. Consider the sentence "Bruno kidnapped Lindbergh's baby boy." The word "kidnap" points directly to a MOP (let's call it M-KIDNAP) which presumably contains, or else makes reference

to other MOPs which contain, the following scenes: The actor takes control of the victim; hides him; contacts the victim's relatives and demands a ransom; negotiates a deal with the relatives; picks up the ransom; releases or kills the victim; and tries to elude capture. We maintain that the best representation of this utterance is an instantiation of this MOP (Schank, Lebowitz, and Birnbaum, 1980), in which the slots for actor, victim, and relatives are filled by Bruno, the baby, and Lindbergh, respectively, and with the first scene marked as having been accomplished. The fact that the first scene is so marked explains the anomaly of a sentence like "Bruno kidnapped Lindbergh's baby boy, but failed to grab him." The fact that the other scenes are available to supply expectations explains why, in a sentence like "Bruno kidnapped Lindbergh's baby boy and left a note," we so easily understand that the note is a written document (as opposed to a musical note), that it is probably for Lindbergh, and that in fact it instantiates the second scene of M-KIDNAP (contacting the relatives and demanding a ransom).

More complicated cases arise when an utterance does not contain a word which directly asserts the conceptual structure that best embodies the speaker's intended meaning. Consider the following two utterances:

Joe bought his new TV at Macy's.

Joe got his new TV at Macy's.

The first utterance, by use of the word "bought," directly asserts an instance of what Fillmore called the commercial transaction scenario (which we will refer to simply as M-BUY). Hence it is best represented by instantiating M-BUY, with Joe as the buyer, Macy's as the seller, and the TV as the object. If we continue to adhere to the position that conceptual representations should as much as possible reflect similarities in meaning, then since these two utterances are synonymous, or nearly so, their representations should be identical, or nearly so. Therefore, the second utterance should also be represented by an instance of M-BUY.

That could be accomplished easily if the word "got" pointed to M-BUY in the same way that "bought" does. However, since there are many ways to get something besides buying it, this approach would imply that "got" is an extremely ambiguous word, with innumerable subtly different senses. If "got" were unique in this regard, this consequence would be tolerable; but it is not. Perusing any text yields words with the same characteristic, words like "take," "use," "go," "have," "hold," "cut," "send," "carry," and so on. In fact, this technical problem of an explosively large number of distinct word senses (which Rieger and Small, 1979, among others, have embraced and attempted to solve) arises because the entire approach remains, at root, based on the old notion that the meaning of an utterance is a simple, additive function of the meanings of the words it contains. Using this approach, if one takes M-BUY as the representation of "Joe got his new TV at Macy's," and then subtracts out the

disambiguated meanings of all the words in that utterance, nothing is left over. Every nuance of the utterance, every subtle distinction in the meaning of every word in the context of the utterance, must be reflected in one of the innumerable number of precomputed senses of the words themselves.

The alternative we propose is based on our intuition that the problem with a word such as "got" is not that it is enormously ambiguous, with many possible meanings of great specificity, but rather that its meaning is vague and general. What "got" conveys is simply a *crude description* of what "got" might mean in a given context. In order to derive a more highly elaborated and specific representation for the utterance as a whole (e.g., M-BUY) this crude description must be employed as a search key for indexing inside of MOPs activated by the context (Schank and Birnbaum, in press; the importance of crude descriptions as a starting point for understanding was discussed in general terms by Marr, 1977). To see how this might work, let us assume that the meaning of "got" is represented simply as ATRANS (transfer of possession or control). Further, assuming that one knows that Macy's is a department store, M-BUY would be potentially relevant, since department stores are a common setting for M-BUY. M-BUY contains several scenes, among them two which center around instances of ATRANS: One represents the transfer of the goods from seller to buyer, and the other the transfer of money from buyer to seller. Now consider the following informal rule:

If an action occurs in a setting which is commonly associated with some MOP, then look inside that MOP and check whether the action could instantiate one of its scenes. If so, then instantiate the entire MOP, and mark the matching scene as already accomplished.

Since the transfer of the TV to Joe matches a central scene in M-BUY, by using a rule of this sort, the utterance "Joe got his new TV at Macy's" can be understood as an instance of M-BUY, without necessitating that "got" point to M-BUY as a possible sense. This approach does not limit the meaning of an utterance to be simply an additive function of the meanings of the words that make it up. If one subtracted away the meanings of all the words in this example, one would be left with an instance of M-BUY which, although suggested by "Macy's," was not asserted by it or any other word in the utterance.

Utterances that seem to require the sort of processing exemplified above are extremely common; we will present one more example here:

John mailed me a postcard from Mexico.

John sent me a postcard from Mexico.

As above, these two utterances are synonymous, or nearly so. The meanings of both should be represented by an instantiation of the conceptual structure that represents our knowledge of postal service (i.e., M-MAIL). In the first

case, this is directly asserted by the word "mailed." In the second case, the word "sent" has been used instead. "Sent," like "got," has a vague and general meaning; it points to a crude description of what it might mean in some context. Let us assume that this meaning can be represented simply as PTRANS (transfer of location). It seems clear that the word "postcard" suggests that M-MAIL might be relevant. Since the main goal of M-MAIL is to accomplish the PTRANS of some object, and since the action asserted by "sent" is a PTRANS, it is fairly straightforward to conclude that M-MAIL should be instantiated.

This approach presents interesting opportunities for linguistics, since it permits the solution of certain semantic problems by appeal to large conceptual structures. For example, consider the sentence "Joe got his new TV at Macy's for 300 dollars." In order to be properly interpreted, the phrase "for 300 dollars" depends on both the presence of M-BUY and the recognition that Joe's receipt of the TV instantiates the scene in which the seller transfers the goods to the buyer. Without activating M-BUY, we would not realize that we can expect a scene in which the buyer transfers money to the seller. We can assume that any phrase of the form "for <some amount of money>" suggests that M-BUY might be relevant, since, if a speaker doesn't know that money is good for buying things, he simply doesn't understand the concept of money. However, this alone cannot account for our ability to determine that Joe paid Macy's the 300 dollars. That involves knowing who is the buyer, and who the seller, which in turn depends on recognizing that the scene in which the seller transfers the goods to the buyer has been accomplished by Joe's receipt of the TV. In general, determining who is the buyer and who is the seller depends on knowing what counts as goods or services, and who can supply them. In this case, M-BUY specifies ATRANS as the service; but in general, the problem is quite difficult. In the utterance "I painted Fred's house for 500 dollars," it seems clear that the speaker received the 500 dollars, since painting a house is hard work and is therefore probably a service he supplied. If the sentence is changed to read "I painted *my* house for 500 dollars," however, it means that the speaker paid 500 dollars to have his house painted. In order to understand this, M-BUY must contain the knowledge that buyer and seller are usually different actors. The sentence "Fred flew to Los Angeles for 1,000 dollars," does not make clear whether Fred was the buyer or seller of the service, and hence it cannot be determined whether he paid 1,000 dollars, or received it.

Conclusion

One of the more interesting, if speculative, implications of an integrated view is that it makes the process of understanding what is usually considered to be literal language appear quite similar to the process of understanding metaphorical language. (Indeed, psychological evidence for the view that literal and non-literal language are understood using similar or identical processes is accumulating; see Gibbs, in press.) For example, consider the pair of sentences "Joe threw out the garbage," and "Judge Bean threw out the case." The former

would normally be deemed a literal use of the words "threw out," while the latter would be considered metaphorical. In contrast, a theory of understanding based on the sort of processing described in the last section would operate in more or less the same way given either as input, starting from a crude description of the meaning of "threw out," and using that in conjunction with the relevant MOPs to more precisely determine what was being conveyed. In a sense, an integrated theory of semantics and pragmatics leads us to the conclusion that all language is, to some extent, metaphorical.

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Potential Words, Actual Words, Productivity and Frequency

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0. Introduction

Linguistic theory must distinguish between the actual words of a language and the potential words of that language. The two notions are independent. The former class is defined by the lexicon, while the latter is defined by the phonology and morphology. In this paper, I will provide several examples of the utility of the distinction, and of notions derived from it.

It is usually assumed that the actual words of a language are a subset of the possible words, for how could something be actual but not possible? In fact, there are many words in a language which are actual but which go against the rules for potential words. In English, those words with initial [sf] clusters are actual but not potential words of the language (Greenberg and Jenkins 1964). That is why I prefer the term *potential*, for the difference between what is possible and what is potential is that the latter does not exist, though it might. Potential is *irrealis*. Since the classes of potential and actual words do not overlap by definition, impossible words are not a problem so long as we use the word *potential*.

Actual words have certain properties which potential words do not. Most importantly, speakers recognize them as actual. The best evidence for this is the fact that people are sensitive to the frequency of actual words (Taylor 1980; Bradley 1980; Whaley 1978). Potential words by definition have no frequency, since they do not exist. What they have instead is probability, which is to say that though there are infinitely many potential words of a language, some are more likely to become actual words than others. For example, we may rank the three words *twid*, *thwid*, and *pwid* in that order, the first being the most likely and the last being the least likely to be an English word.¹⁾

Actual words also have arbitrary meanings, while the meaning of a complex potential word is motivated by the rule according to which it is formed. Furthermore, the meanings of potential words are much more fluid than those of actual

1) Most linguists nowadays do not like to talk about probability, because it is considered to be a probabilistic or statistical notion rather than a structural one. Instead, linguists use the term *markedness*, but markedness is a way of deriving probability (Kean 1974) so linguists do talk about probability, though they prefer to think otherwise. In word formation, the code word for probability is *productivity*. To say that a given pattern is more productive than another is to say that there is a higher probability of a potential word in the first pattern being accepted in the language than there is of a potential word in the second pattern being accepted.

words. In the most extreme cases, such as the zero-derived verbs discussed in Clark and Clark (1979), the range of potential meanings for a given potential word may be so vast as not to be determinable out of pragmatic context. Yet, even in this class, the meaning of a word will become fixed as the word becomes lexicalized.

Much recent work on word formation has ignored the actual/potential distinction and the related notions of productivity and frequency and has instead treated all words as equal, following the model of syntax and the sentence. This ignorance is convenient, for it permits the investigator to concentrate on purely formal problems without worrying about substantive matters. It is also unfortunate, depriving the theoretician of a good part of the empirical base upon which to anchor his claims. In the interest, therefore, of correcting this bias, I will present a few cases where the actual/potential distinction, frequency and probability shed some light on other matters.

1. Conversion

I will discuss frequency and probability and the relation between them below, but I will begin with the primary distinction, that between actual and potential words, and show how it can be used in solving a theoretical problem about structure. The problem is the structure of *zero derivation* or *conversion*, a common process in many languages whereby a word or stem of one category can be used in another category without there being any necessary overt mark of the category shift (e.g. English *photograph*_{N, V}; *smile*_{N, V}; Biblical Hebrew active participles and agentives; Latin past participles and verbal nouns). Given that there are two coherent categories, how are they to be related formally? If we assume that all morphological relationship is expressed by concatenation, the derived category being the one bearing the affix, then we must resort to zero morphemes. The base is then X and the derived class is X+Ø. However, there are general problems with zeroes of this sort (Nida 1948; Haas 1957; Lieber 1980) and it is difficult to find any empirical evidence for them. There is also some evidence against them. Even in frameworks which allow more than concatenation there are formal problems. For example, in the systems of morphology developed by Selkirk (forthcoming) and Lieber, where phrase structure is expressed in terms of binary branching trees, the relationship between the basic and derived categories is similarly inexpressible without resorting to unmotivated zeroes, since there is otherwise nothing for one branch of the tree to dominate.

The formal difficulty engendered by zero-derivation seems to arise from the necessarily directional nature of the usual means by which derivational relationships in morphology are expressed—the Word Formation Rule (WFR). It is therefore legitimate to ask whether the relationship between zero-related categories is indeed directional, for if it is not, then a nondirectional solution (such as that suggested by Lieber) which easily dispenses with zero morphemes may be correct. What evidence do we have for the necessity of a directional rule,

similar to a normal WFR except for having no visible affix, in cases of zero-derivation?

It is difficult to find good formal evidence, since homophony can always be accidental or the two can always be equal instances of the same form. For example, Lieber claims that the fact that the Latin verbal noun is always identical to the past participle shows that it is derived from it. But this is false, for the two could just as well be derived from a third unlabeled element. There is sometimes semantic evidence. The Latin verbal noun generally denotes an instance of the action denoted by the verb. Thus, *percussus* is a noun meaning 'knock' or 'beating' and the corresponding verb is *percutio* 'to strike': the noun *raptus* means 'abduction' and the verb *rapio* means 'to abduct', and so on. The same is true in English: corresponding to a verb we find a noun denoting an instance of the action of the verb: *hit_v*, *hit_n*; *run_v*, *run_n*; *claim_v*, *claim_n*; *need_v*, *need_n*; and so on. For both these cases it would seem reasonable to claim that the verb is semantically basic and that the semantics should be a criterion for the directionality of the relationship. Semantic evidence of this sort can be found in most cases of zero-derivation, and it is usually the semantic evidence that guides the particular solution provided (cf. the many cases discussed in Marchand 1969). But semantic evidence is rarely conclusive and is regarded with some suspicion by most well-brought-up linguists. Nor is it always clear. For example, is the noun *claim* really derived from the verb? Couldn't it just as easily be the other way around? After all, we have ample evidence for a rule in English which zero-derives verbs from nouns (Clark and Clark 1979).

Enter the actual/potential distinction. Within the framework of Aronoff (1976), a WFR is a directional device which forms potential words from actual words.²⁾ Therefore, for any rule of zero-derivation, if it is a WFR, the base of that rule should consist of actual words and the derived category should contain potential words.³⁾ The actual words should have frequency and lexicalized meanings, while the potential words should have potential meanings. As far as I know, these criteria solve every existing case of zero-derivation. They always tell us which category must be basic and which derived. We must, therefore, assume that there is a WFR at work in all cases of zero-derivation. We must **also** conclude that any nondirectional rule of conversion is impossible, for it always implies that the two related categories are equal, which they are not. For example, Lieber is forced by her nondirectional position to claim that for every German noun in *-el* (e.g. *Deckel* 'cover') there should be a corresponding verbal form (in this case *decken* 'cover'). This is false, for while there is an actual noun in every case, the verb in most is only potential, and there is no

2) There are rare exceptions, instances of new words being formed from potential words, but these are not significant or systematic. Note also that by *word* I mean *stem* or *lexeme* (Aronoff 1978), and that there may be cases of word formation from roots (Drapeau 1980).

3) The output will not consist entirely of potential words, since some members of the output category have been lexicalized.

pressure to form new verbs, which is just what the directional theory predicts.

This leaves us with the formal problem of relating the two sets without resorting to excess zeroes. But there are several formal systems which dispense with zero while still retaining the necessary directionality. Williams (1981) for example, permits non-branching phrase structure rules of the type $X \rightarrow Y$ for zero-derivation. Furthermore, he shows that such a structure without zeroes provides a nice explanation for some of the morphological peculiarities of certain zero-derived categories. Thus it is clear that zero-derivation is different from other formal types of WFRs, but the difference is merely the lack of an affix; there is no need to posit a new device to account for zero-derived categories.

2. Productivity and Foregrounding

Having established the distinction between actual and potential words, I would like to turn to the related matter of productivity. As in the last section, I will deal with interaction, but instead of a formal question, I will treat a functional question, that of foregrounding (Mukarovsky 1970).

It is well-known that the productivity of a WFR varies with the morphological composition of the base. Williams calls this *potentiation*. In English at least, a given affix is usually potentiated by the last morpheme of its base. So, for example, the nominal affix *-ation* is potentiated by the verbal affix *-ize*. Potentiation gives us complex morphological patterns of the general form *X-affix-affix* in English. I will call these patterns, which are actually sub-categories of the output of a WFR, *Word Formation Patterns (WFPs)*. Productivity is, therefore, a property of WFPs rather than of WFRs. For example, the WFP *Xization* is more productive than the WFP *Xizement*.

In a recent series of studies (Aronoff and Schvaneveldt, 1978; Anshen and Aronoff 1981; Cutler 1980) it has been demonstrated that differences in productivity among WFPs are correlated with adult native speakers' willingness to accept potential words formed according to these WFPs as words of their language. For example, given the two WFPs *Xiveness* and *Xivity*, with potential words such as *obsessiveness* and *obsessivity*, speakers prefer *Xiveness* over *Xivity*. This is true regardless of which of the following three questions speakers are asked: is this a word in your vocabulary? is this an English word? is this a meaningful word? On the basis of this phenomenon we may conclude that knowledge of productivity is part of speakers' competence and that it is not a purely historical artifact as it has been traditionally treated.

If we look at the phenomenon in the light of the actual/potential distinction made above, we may say that the more productive a pattern is, the less likely a speaker is to be able to distinguish potential words formed in that pattern from actual ones. Speakers tend to judge potential words formed in productive patterns as actual words, though they are not. The actual/potential distinction is, therefore, not very important for highly productive WFPs. This is a familiar truism, though I don't think that it has been demonstrated experimentally

before.

The inverse of this statement is that speakers are more aware of the actual/potential distinction for less productive WFPs. With these, they know a new word when they hear it. Resourceful speakers exploit this awareness and the actual/potential distinction is thus rendered useful. For example, given the word *reductive* (a technical term used by potters to designate whether a clay or glaze is fit for reduction firing, in which the oxygen in the firing atmosphere is reduced) I may use the words *reductiveness* or *reductivity* to refer to the property itself. The former will go unnoticed by my hearer (and probably by myself), but the latter will not. Indeed, I would only use such a word if I meant it to be noticed. I will, therefore, most likely use the latter term to mean something special, arbitrary to a degree, lexicalizable. In fact, the technical term for the property in question will most likely be *reductivity* rather than *reductiveness*; so we use *productivity*, *sensitivity*, *relativity*, *objectivity*, etc. as special terms, rather than their more productive counterparts.

From a general linguistic point of view, this use of a less productive WFP is an instance of *foregrounding*. Foregrounding is a poetic technique which was first discussed by Mukarovsky. It consists of the use of rare or deviant forms or a disproportionately frequent use of a form, compared to its use in "normal" language.

The use of less productive WFPs for purposes of foregrounding is pervasive. Technical terms, jargon, highfalutin language, advertising, academese, all use less productive WFPs simply because they are more remarkable. It is also of some interest that young children apparently do not know what is remarkable. Instead (Clark 1978; Berman 1980) they coin words at will, regardless of the productivity of the pattern. Children, in other words, are pure formalists.

The difference between children and adults can be traced to the fact that children's command of their language grows independently of their awareness of it (Sinclair, Jarvella, and Levelt 1978). Awareness comes late, perhaps only after the formal system has developed fully. Since foregrounding depends on awareness, children are incapable of it, as they are incapable of most metalinguistic activities.

Foregrounding is a metalinguistic ability *par excellence*, dependent as is any poetic device on the use of language for its own sake. Yet, as Jakobson (1960) emphasizes, metalanguage plays an important role in our everyday language. The foregrounding of a WFP is nice proof of this essential role of awareness in our everyday speech, and it is further proof of Jakobson's claim that the poetic function has an intrinsic place in language of every sort. Finally, the success of such foregrounding underscores the importance of productivity for all mature speakers.

3. Frequency

I noted above that only actual words have frequency. Nonetheless, frequency can be a useful tool in the study of productivity, and it can also shed light on

more basic questions of linguistic theory.

I first became interested in frequency when I was doing the experimental work mentioned above. In work of this kind it is very important to control for frequency, because of its effect on speakers' responses. It occurred to me at the time that frequency might be a simple explanation for productivity: the more frequent WFP is more productive. Practical considerations prevented me from investigating this possibility, there being no available frequency counts of affixes, but I was still intrigued by frequency, so I decided to perform another type of frequency count, which was possible given the materials at hand. I calculated mean frequencies for rival WFPs such as *Xiveness* and *Xivity* and for the bases of such WFPs. For example, for the above two patterns, I calculated the mean frequency of all the words in each pattern listed in Walker (1936) for which the frequency of the base was 1 per million or more, according to Kucera and Francis (1967). The results of this calculation are given in Table 1:

TABLE 1
Xivity vs. *Xiveness*

	MEAN FREQUENCY OF BASE WORDS	SD	MEAN FREQUENCY OF DERIVED WORDS	SD	N
<i>Xivity</i>	27.261	29.218	9.563	23.891	23
<i>Xiveness</i>	13.117	20.445	0.641	3.214	103
	Z-score=2.23 significant (one-tail test)	.02	Z-score=1.79 significant (one-tail test)	.04	

The statistical analysis reveals that individual words of the form *Xivity* are significantly more frequent than individual words of the form *Xiveness*. This result can be explained in terms of the analysis given to the last section. I claimed there that the less productive WFP is more remarkable and that its members are therefore more likely to be lexicalized and assigned special meanings. We now see that this lexicalization is reflected in frequency, for semantic complexity and frequency go hand in hand.⁴⁾

Expecting the pattern to repeat itself, I performed the same calculation for another set of WFPs, *Xibility* and *Xibleness*. In this case, *-ness* is less productive, both in terms of sheer number of words and in terms of native speakers' judgements about potential words, as detailed in Anshen and Aronoff (1981). Yet we find the pattern reversed. In fact, no *ibleness* word has a frequency as high as 1.

4) The well-known connection between semantic complexity and frequency may also explain the greater mean frequency of the base of the less productive WFP. More frequent words tend toward greater polysemy. Since these more frequent words are more likely to develop special senses, the need to distinguish particular senses is more likely to arise for these words. The less productive WFP will, therefore, be more useful with just such more frequent bases, serving to make the semantic distinctions concrete.

TABLE 2
Xibility vs. *Xibleness*

	MEAN FREQUENCY OF BASE WORDS	SD	MEAN FREQUENCY OF DERIVED WORDS	SD	N
<i>Xibility</i>	8.6	15.297	3.32	16.588	50
<i>Xibleness</i>	12.737	17.985	0	0	19
	Z-score = .888 insignificant		Z-score = 1.42 insignificant		

The only explanation for this data is that *Xibleness* is not merely less productive than *Xibility*, it is dead, not a viable WFP of the language. There may be enough *Xibleness* words around for a linguist to posit a pattern, but the pattern is not real. This explanation appears to be correct, for it is corroborated by speakers' judgements of the two pairs *Xibility/Xibleness* and *Xiveness/Xivity*.

TABLE 3
PROPORTION OF ACCEPTANCE BY SPEAKERS

WFP	ACTUAL WORDS	POTENTIAL WORDS	NON-WORDS
<i>Xiveness</i>	.88	.44	.16
<i>Xivity</i>	.90	.38	.16
<i>Xibleness</i>	.61	.37	.19
<i>Xibility</i>	.96	.73	.38

With the *Xive* pair, though speakers consistently prefer *Xiveness* over *Xivity* for potential words (this is true even when the results are broken down by individual words and individual speakers) the preference is not great and it does not have any effect on speakers' judgements of either actual words or nonwords. They must recognize the two WFPs as being operative for potential words and must know that one is more productive than the other. With the *Xible* pair, on the other hand, we find a very different pattern of results. First of all, the difference between acceptance of potential *Xibility* words and *Xibleness* words is much greater than with the first pair. Furthermore, the effect carries over to the actual words and the nonwords, which did not happen in the first case. Speakers are simply rejecting *Xibleness* words out of hand, regardless of their status on the actual/potential dimension. This is evidence for the demise of the pattern as a viable WFP of the language and it squares with the frequency data.⁵⁾ It is also true that *Xibleness* words are never foregrounded.⁶⁾

Productivity and frequency together attest to the death of *Xibleness*. A purely formal study would never have given the slightest suggestion that this

5) Cutler has shown that potential words are usually more acceptable if the base word is transparent. The fact that *Xibleness* words run counter to this general fact is further evidence for my claim.

6) Exactly why *Xibleness* should be dead is not clear to me, though the answer may lie in the historical fact that foreground becomes background. *Xibility* is historically later than *Xibleness* and may have smothered it.

could be the case, for there are no formal grounds for doubting the viability of this particular WFP. Nor should there be formal reasons for its demise. Nonetheless, speakers know that the pattern is not viable, just as they know the frequency of words. Both are part of their linguistic competence.

4. Conclusion

In this paper I have shown that linguistic theory must recognize the distinction between actual and potential words and the related notions of productivity and frequency as part of grammatical competence. I have also shown how these notions can be used in theoretical argumentation. I hope that I have not confused the two.

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On Word Formation in Natural Morphology*

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§ 1 Natural Morphology,¹⁾ although heavily influenced by Natural Phonology (as founded by Stampe 1969), has so far been a predominantly European endeavour.²⁾ Research in Natural Morphology (henceforth NM), as I understand it, can be characterized by the following short check list: It investigates overt and covert universals (cf. Seiler 1975, 1978), which are based on extralinguistic entities (e.g. on perception: Mayerthaler 1981; Wurzel 1977, or on semiotic principles: Mayerthaler 1980; Dressler 1980c), and which are the bases of naturalness scales (most-more-less-least natural). NM is functional (Dressler 1977a, 1980a,c; cf. Dik 1980; Seiler 1976; Stachowiak 1978). One function is served by several techniques (although to different degrees of excellence, cf. the naturalness scales) and one technique can serve more than one function, e.g. in the case of labelling concepts (cf. Stachowiak 1978). Language types (in the sense of Skalička 1979) are established by the choice and application of basic techniques. The typology of domains such as diachrony (Dressler 1980a,c), language acquisition (cf. e.g. Panagl 1977; Chmura-Klekotowa 1972; Stephany 1980), aphasia (Stachowiak 1978; Dressler 1979) must explain domain-specific phenomena with the help of domain-specific intervening variables (cf. Dressler 1980a). In short, language(s) is (are) seen as a system(s) of problem solving (Seiler 1975; introduction to Seiler 1978), i.e. of solving the problems of the functions of human language (esp. cognition and communication). As a consequence there are more adequacies or standards for such research than those proclaimed by N. Chomsky (cf. Dik 1980).

§ 2 Since many linguists think of word formation as something very different from inflectional morphology, I must enumerate a few arguments why I consider both components to be much more similar to each other than to any other component of language, so that what I am going to say about word

* I would like to thank John Rennison (Vienna) for englishing my first version of this contribution.

1) See especially Dressler-Mayerthaler-Panagl-Wurzel, in preparation; Mayerthaler 1980, 1981; Panagl 1977; Wurzel 1977, 1980; Dressler 1977a, 1979, 1980a,b to appear a,b. For related approaches cf. Seiler 1975, 1978; Stachowiak 1978; Stephany 1980; Plank 1981. See more articles on WF in volume XVI (1982) of *Folia Linguistica*.

2) Since the three main reporters are American and working in the sphere of Generative Grammar, I will focus on non-American and non-generative research; nevertheless, lack of space does not allow me to strive toward effectively diminishing what the New Zealander Bauer (1980: 73) has called the general "ignorance of the vast amount of European work on word formation" among non-Europeans.

formation (WF) may go for all of morphology. Singh & Ford (1980) have mentioned that 1) only inflectional and derivational affixes (and rules!) can be intermingled, 2) that both can change word-classes, 3) that both can use suppletion, 4) that inflectional morphemes (and rules!) can become derivational ones and vice versa (whereas there is an unidirectional change from syntactic and phonological rules to morphological rules, cf. Dressler 1976). One may add similarities in 5) analogy and 6) reanalysis in acquisition and diachrony (cf. Plank 1981: 67ff), etc. I.e., although the functions of inflectional morphology and WF diverge quite considerably, the techniques used are similar, and differ only in degree (cf. Plank 1981).

§ 3.1. One typical research strategy of Generative Grammar is to posit an arbitrary (highly constrained) hypothesis (cf. Plank 1981: 206f) as a formal universal, e.g. that all word formation rules (WFRs) are word-based (Aronoff 1976: 21—cf. § 4. below); then universal restrictions on possible WFRs are derived and (thoroughly) checked on a limited number of languages to see whether they generate all and only the correct newly formed complex words (or even all complex words). Undoubtedly this approach has greatly stimulated research on WFRs.

§ 3.2. Another approach, often used in NM (cf. § 1), is to establish (naturalness) scales for linguistic techniques (e.g. conceivable bases for WFRs—cf. § 4 below), to link them with the functions they are supposed to serve and with general principles (e.g. semiotic principles) governing their application, to grade these techniques according to how well they match functions and principles, to try to find typological reasons why languages of a given type use relatively "bad" techniques at all, and to evaluate how bad a conceivable technique must be for no natural language to utilize it. Cf. a similar approach in Seiler's UNITYP theory (Seiler 1975, 1978; Stachowiak 1978; Biermann 1980; Stephany 1980) and the parameters of markedness in Mayerthaler (1981).

§ 4.1. Let us illustrate this difference with the word-based hypothesis. Bauer (1980), Botha (1980) and Carroll (1979) have already given abundant evidence (from English and Afrikaans) of WFRs based on multilexical words, phrases (cf. Guilbert 1975: 155) and sentences, to which one may add Russian examples (cf. Kalnijazov 1978: 10) such as *čert-mne-ne-brat-stvo* 'foolhardiness', from *čert* 'devil', *mne* 'to me', *ne* 'not', *brat* 'brother' (i.e. 'the devil is not my brother') and the verb-nominalizing suffix *-stvo*, or German examples such as *eine Friss-Vogel-oder-stirb-artige Attitüde* 'a take or leave it attitude' (from *friss* 'eat!', *Vogel* 'bird', *oder* 'or', *stirb* 'die!'), and the adjectivizing suffix *-artig*, i.e. the base is an imperative sentence). However, a multi-sentential base seems to be excluded in natural languages, i.e. several connected sentences (in the text-linguistic sense of surface sentence formats, cf. Beaugrande & Dressler 1981) cannot be bases for WFRs.³⁾

3) Although complex words may be derived by WFRs from (the intermediate step of) NPs which co-refer anaphorically to semantic macrostructures, cf. e.g. Beaugrande & Dressler 1981: § IV. 16; Wildgen 1980.

§ 4.2 What reason may be evoked to explain why phrases (and, even more so, sentences) are much more rarely used bases for WFRs? If we take one of the (universally accepted) functions of WFRs, i.e. that of enlarging the lexicon (enrichissement verbal, e.g. Guilbert 1975) by the labelling of concepts, then clearly there is less pragmatic need to label concepts of such complexity that phrasal or even sentential bases must be used. This may justify their rarity of use, but not the fact that certain languages (e.g. Latin, Ancient Greek) exclude multi-lexical verbinflected, modally qualified sentences as bases for WFRs.

§ 4.3. Here the semiotic principle of the optimal size of a sign⁴⁾ may be invoked: Too big a sign(ans) is difficult to perceive for the hearer and to store for the speaker and hearer. Notice that, typically, sentential bases of WFRs are idiomatic sentences which are stored in verbal memory anyway as in the agent noun *do-it-yourselfer* (US News & World Report, 30.8.1982 p. 45) (cf. § 4.1). The perception of the base is necessary if a second universally accepted main function of WFRs is to be served: the motivation of complex words, i.e. the better the bases can be perceived, the better the function of motivation is served (cf. Cutler 1980: 49f).

§ 4.4 The exact nature of this typological scale and of its correlation with the aforementioned psychological explanatory factors remains to be specified. However, one typological phenomenon must be mentioned: 1) Polysynthetic, incorporating languages (cf. Sadock 1980; Biermann 1980) allow larger and more complex bases for WFRs, and in a freer and more regular way, than typologically different languages.

2) WF serves the function of predication much more in polysynthetic languages than in inflectional or other language types—contrast Latin a) ablative absolute constructions such as *te con-ses-sor-e* 'if you are sitting together with (me)', literally 'you (ablative case), with-sit(t)-er (agent suffix *-tor/-sor*)—ablative suffix'; b) compounds as in *vale, denti-frangibul-e* 'farewell (you) tooth-break-er', with Cahuilla (Seiler 1975: 5) *túk-va-s-nek-is* 'blue', lit.: 'having accomplished (absolutive suffix (*i*)s) the process of becoming (*nek*) like the thing where (*va*) something curved is fastened (*túk*, absolutive suffix *s*)'.

3) If a technique serves more functions (purposes, i.e. solves more problems), then it is used more widely.

§ 4.5. As to bases smaller than words, these often occur in non-productive word-formations of long standing (Plank 1981: 204ff, cf. Malkiel's (1970: 7) acephalous word-families), for which the word-based hypothesis is not meant to apply. But, as Bauer (1980: 75ff) notes, learned compounds in languages for special purposes and words from foreign bases can regularly be derived from morphemes (or even pseudo-morphemes), cf. the ease with which new neo-classical compounds are formed from morphemes such as *helio-*, *hydro-*, *petro-* in technical languages throughout the world; the number of stems involved is relatively small, less than 1% of the number of concepts needed (Wüster 1979:

4) More precisely: signans, cf. Dressler 1980c: 55, of which G. Zipf had a presentiment.

79). And this leads to the explanation of the phenomenon: Words are primary signs, morphemes only secondary signs, i.e. signs on signs (words); therefore words are better perceivable than morphemes for motivating derived words (cf. § 4.3). However, in languages for special purposes, terminological morphemes such as *helio-* are as familiar and recoverable to specialists as words, and therefore adequate bases for WFRs.⁵⁾ Moreover, due to their small number, stems must be used frequently in productive terminological word formation.

§ 4.6. Since the early medieval Arabic grammarians, Semitic philology has had the tradition of analyzing simple and derived words of Semitic languages as consisting of a consonantal root (usually 3 "radicals", sometimes 2 or 4, etc.) and a vocalic "scheme" (and possible prefixes and suffixes), e.g. /ktb/ in Arabic *kataba* 'he wrote', *kitāb* 'book', etc. The analysis is at variance with the word-based hypothesis. Hebrew word formation provides two arguments for the root-based analysis:⁶⁾

§ 4.6.1. New consonantal roots can be extracted from foreign words and names in Modern Hebrew, e.g. /pstr/ from *Pasteur* (Werner 1981: 64 n. 38), as in *pister* 'he pasteurized', inf. *le-faster*, past participle *me-fuster* (with morphological spirantization), nominalization *pistur* 'pasteurization'. Also, Ben Yehuda extracted the root /brš/ from Yiddish *baršt* = Ger. *Bürste* = Eng. *brush* (Werner 1981: 47) and introduced *hi-vriš* 'he brushed', *mi-vreš-et* 'brush' into Modern Hebrew.

§ 4.6.2. Even abbreviations have been adapted to radicals and schemes (F. Werner, personal communication): Post-Biblical 'akkum 'idolator' with the root /'km/ acronymically extracted from 'oved 'servant', *kokhavim* 'stars', *mazzalot* 'signs of the zodiac'; Modern Hebrew *makkam* 'radar' from *megalle* 'discoverer', *kiwun* 'direction', *maqom* 'place'; *sakkum* 'cutlery' from *sakkin* 'knife', *kaf* 'spoon', *mazleg* 'fork'; *duah* 'report' from *din* 'judgement' and *hešbon* 'bill'. with its derivation *diwweah* 'he reported'.⁷⁾

§ 4.6.3. These examples show that consonantal radicals are at least as psychologically real for learned Israelis as neo-classical roots are for scientists all over the world (cf. § 4.5). The difference in the technique used follows from the difference in language type: the word formation of international terminology is agglutinative, Semitic morphology is introflective (with vowel "schemes" as "infixes" or "transfixes" in the root-based analysis, as modifications or ablaut in the word-based analysis), thus motivation (cf. § 4.3) is based on roots.

§ 5.1. The goal of perceiving bases easily (for motivation) is also responsible for the following gradient naturalness-scale of WF techniques. The base of a

5) A similar argument holds for poetic 'disverbation', see Dressler 1977a: 22, to appear a: 433ff.

6) Thanks are due to Fritz Werner (University of Vienna) for help and advice in this matter.

7) Although the technique is identical, the semantic structure diverges among these examples. This is another argument for distinguishing semantic derivation and morphological techniques of WFR's. see Beard 1981; Dressler 1977a: 23f.

derived word is better perceivable the more morphotactically transparent it is in the derived form:

I) The best technique is total transparency (pure agglutination), e.g. Eng. *sing* vs. *sing-er*. (For conversion, see § 6.1.C below)

II) The second best is the intervention of morphonological rules (MPRs)⁸⁾ which do not destroy morpheme boundaries (—such MPRs are relatively frequent in agglutinative languages), e.g. Eng. *long* ([lɒŋ]) vs. *long-ish* ([lɒŋɪʃ]). A special (and phonologically fairly or totally unnatural) sub-type are IIa) modifications such as ablaut (*to sing—a song*) and umlaut.

III) Worse is the intervention of MPRs which fuse (e.g. contract) morphemes (typical for fusional/inflecting languages), e.g. Ancient Greek *anakh-s* 'lord', and with *j*-suffixes *anassa* 'lady, queen', *anass-ō* 'I am lord' (with the MPR *kj* → [s:]).

IV) Still worse is infixation, because it destroys the continuity of the base (cf. Plank 1981: 27, 193). (For subtractive WFRs see § 6.1.D below).

V) Worst is suppletion (very marginal in word formation) as in the French place name *Le Puy* vs. *Anicien = Ponot* 'inhabitant of Le Puy'.

§ 5.2. This scale predicts among other phenomena: 1) cross-linguistic implications such as: If a language has only one technique, it is technique I; if it has technique III, then it must also have technique I and II; etc., 2) the relative frequency of techniques within a language. However, Semitic languages use more modification (IIa) or infixation (IV) than suffixation/prefixation (I, II), cf. the figures for Hebrew adjectives in Werner (1981: 215ff). This deviation from universal naturalness is again due to typological adequacy (one form of system-appropriateness as discussed in Wurzel 1980: 109ff), i.e. in introflective languages consonants are favored as radicals (parts of roots) expressing lexical meaning, and vowels (= "schemes") as expressions of morphological meanings, a universal tendency brought to its extreme in Semitic languages.

§ 5.3. The "disadvantages" of the introflective language type (as seen from a universalist point of view and as exemplified by relatively "unnatural" choices of techniques on universal naturalness scales, cf. § 4.6, 5.2) can be understood—metaphorically—as sacrifices in order to obtain advantages on other naturalness scales:

§ 5.3.1. Symbolic use of vowels and consonants (§ 5.2).

§ 5.3.2. Relative good size of words: Hebrew and Arabic words and word forms are usually bisyllabic or trisyllabic, i.e. they are longer than the (generally) monosyllabic words of isolating languages and shorter than the (often) longer words of agglutinating and polysynthetic languages.

§ 5.3.3. Great motivation of the lexicon, since the same verbal roots are used much more as bases of WFRs than in inflecting or even agglutinative languages (but similar to polysynthetic languages such as Greenlandic Eskimo). Notice the contrast between English and Arabic in the following 13 Arabic

8) The less phonologically natural a MPR is, the more morphotactic transparency is blurred, cf. Dressler 1980, to appear b.

words (in English translation), which all belong to the same root /ktb/: 1) to write, 2) writer, 3) writing-school, 4) to correspond, 5) to dictate, 6) inscription, 7) title, document, 8) book, 9) book-seller, 10) book-shop, 11) library, 12) letter, 13) office. And as experiments done by Ghanem (1982) have shown, the productivity of certain Arabic WFRs is very high, in the sense that the number of potential words exceeds (many times!) the number of actual words (derived via the same WFR).

§ 5.3.4. Even textual semantic coherence can be signalled by means of roots better than in other language types (Aphek-Tobin 1981).

§ 6.1. The aforementioned (§ 5) scale of techniques overlaps with another scale predicted by the principle of constructional iconicity.⁹⁾ A semantic derivation (cf. fn. 7) adds intensional meaning to the meaning of the base by which the derived form is motivated, e.g. a deverbal agent noun represents the intensional meaning of the motivating base and adds the agentive meaning. Therefore a diagrammatic relation between meaning and form A) is best achieved if a form element is added to the form of the base, i.e. if there is affixation, e.g. *sing-er* derived from *to sing* (cf. § 5.1 techniques I and II, III, IV). B) Much less diagrammaticity is achieved in the case of modification (IIa), e.g. *sing* → *song* (and to a certain degree in II and III). C) There is no diagrammaticity in the case of conversion such as in Eng. *to cut* → *a cut* (cf. Pennanen 1980). D) The extremely rare technique of subtractive WFRs is even anti-diagrammatic, because "less form contradicts more meaning", e.g. Czech *zelený* 'green' → *zelen* 'greenness', *lovit* 'to hunt' → *lov* 'a hunt' (Dokulil 1968), Russian *logika* 'logics' → *logik* 'logician'.¹⁰⁾

§ 6.2. For lack of space and of other facilities I must leave it to the imagination of the reader to construct a three-dimensional hologram where each dimension represents one of the three scales (cf. § 4, § 5.1, § 6.1); where each cubic cell represents the combination of one variable from each dimension, and is labelled 'very frequent', 'frequent', 'rare', 'non-existent' (in the languages of the world). If my scales are of any value, then the combination of the best variables should be most frequent, i.e. word-based, totally transparent affixation as in Eng. *sing-er*, and the combination of the worst/worse variables should not exist in any language, e.g. sentence-based (or morpheme-based) subtractive WFRs. Moreover, clusters of medium-natural combinations should be type-specific.

§ 6.3. But we can be more specific about the predictions of my model of Natural Morphology. Three claims of different strength can be made:

§ 6.3.1. The strongest claim is that due to diagrammaticity (constructional

⁹⁾ Or diagrammaticity, cf. for inflectional morphology Zwicky 1978: 137ff; Mayerthaler 1980, 1981: 43ff.

¹⁰⁾ Tihonov 1975; or via truncation and haplogy from /logik=a+ik/. But even then, truncation and haplogy result in subtraction. Of course both the Czech and the Russian examples could be considered as cases of conversion of the roots *zelen-*, *lov-*, *logik-*.

iconicity, § 6.1) all WFRs should be affixing, and there should be neither conversion nor subtraction (a claim made by Lieber 1981 about conversion—she forgot about subtraction). Such a claim—typical for Generative Grammar, but not for Natural Morphology (or Natural Phonology, cf. § 3.1–2)—is obviously too strong.

§ 6.3.2. The weakest claims are that worse techniques such as conversion and especially subtraction should be a) less frequent than affixation. This claim is clearly borne out (§ 6.2). b) They should be learned later by children—this has been proven for the scale of transparency (§ 5) in studies on language acquisition of Russian (Sorokin & Tarasov & Šahnarovič 1979: 196), Dutch (Snow et al. 1980), English (as to WFRs of the Latinate component) etc. c) They should be less productive in poetic neologisms, at least in less audacious poetic license (Dressler to appear a). etc.

§ 6.3.3. However, Natural Morphology becomes still more predictive, if in addition to § 6.3.2 a third, stronger claim can be supported: The worst techniques on a scale of naturalness are used only in very special conditions where other factors can be made responsible as intervening variables. And this is possible in all cases of subtractive WFRs I know of (see § 6.4, 6.5).

§ 6.4. Why does Russian allow the subtractive WFR *logika* 'logics' → *logik* 'logician', *fizika* 'physics' → *fizik* 'physicist' (§ 6.1)?

§ 6.4.1. Russian is an inflecting language, where subtractive MRs may occur (though rarely)—this is a typological restriction (typological adequacy).

§ 6.4.2. Russian has similar WF endings in *-ik* denoting human agents as in the agentive suffixes *-nik*, *-čik*, *-ščik*, *-lščik* (for *-ik* see § 6.4.3.). Thus endings in *-ik* are system-adequate (language specific system adequacy).

§ 6.4.3. As to its historical genesis,¹¹⁾ *-ik* originated—according to the etymological and historic dictionaries—in learned words loaned from Latin via Polish or German or directly. Here Latin *-icus* was loaned as the suffix *-ik* as in *akademik* 'academician', *himik* 'chemist' related (then synchronically derived from) *akademija* 'academy', *himija* 'chemistry'. The historic models are Pol. *akademia*, *akademik*, from Lat. *academia*, *academicus* and early Modern German *Chymie*, *Chymiker* from contemporary Latin *chymia*, *chymicus*. There was no way in Polish and Russian to render the Latin inflectional (nominative) suffix *-us* other than by zero, i.e. the nominal class of Polish and Russian zero nominative consonant stems corresponds both genetically and contrastively (including patterns of loaning) to Latin *o*-stems (nominative in *-us*).

Now when Renaissance Latin and German *physica* 'physics' and *physicus* 'physicist, physician' were loaned into Polish (*fizyka* → *fizyk*) and Russian (*fizika* → *fizik*), there was simply no alternative than to continue taking over Latin *-icus* as Polish [ik] and Russian [ik], and this well-established routine, founded in the system-adequate properties of Russian declensional classes, resulted in the awkward synchronic derivation of *fizik* 'physicist' from *fizika* 'physics', and it is now a productive WFR also in cases without any foreign lexical model as

11) Here I would like to thank I. McEuk (Montréal) for his help.

in *organik* 'specialist in organic chemistry', derived from *organika* 'organic chemistry'.

§ 6.4.4. Thus this subtractive device of Russian (and Polish) is a historical accident which can be explained from the diachronic (§ 6.4.3), the synchronic (§ 6.4.2) and the typological points of view. (§ 6.4.1).

§ 6.5. A still better instance are subtractive rules of creating hypocoristic names.

§ 6.5.1. Whereas *Liz, Bet, Mike, Bob* are derived from *Elizabeth, Michael, Robert* etc. rather by analogy than by a productive and fully predictive WFR, there is a similar WFR in Afrikaans. According to Combrink (1982) Afrikaans has a productive and fully predictive WFR of forming hypocoristics such as *Wynie, Swanie, Teunie* from proper names such as *Wynand, Swanepoel, Theunissen*. The subtraction consists in shortening the first part of the name to a consonantal onset, a vocalic nucleus and a consonantal offset.

§ 6.5.2. But notice that afterwards the diminutive suffix *-ie* is added. Therefore this WFR combines subtraction and affixation and thus is no pure case of subtraction.

§ 6.5.3. According to Combrink (1982) the resulting names are emotive. This goes well with the (actual or potential) emotive meaning of diminutives. And in diminutives diminution of content can be iconically (diagrammatically) reflected by diminution in form, based on the metaphoric (=iconic!) nature of diminutives which evoke small pets, babies or objects (all with actual or potential emotional connotation), cf. Mayerthaler (1981: 99ff, 151, 181).

§ 6.5.4. According to Combrink (1982) these emotive names are mostly used as vocatives, and there are good universal reasons (in terms of Natural Morphology, see Mayerthaler 1981: 31ff, 97f, cf. Winter 1966) why vocatives are very frequently short or shortened (subtraction).

§ 6.5.5. Thus the subtractive part of Afrikaans hypocoristic formation is amply justified: it is only partially subtractive (§ 6.5.2); there are conflicts of diagrammaticity (§ 6.5.3) and between Natural Word Formation and Inflection (§ 6.5.4). Both § 6.4 and § 6.5 exemplify the claim (§ 6.3.3) that the worst technique of a naturalness scale (in our case: subtraction on the scale of diagrammatic marking) may occur only under very special conditions.

§ 7 Of course, many more principles and phenomena are dealt with in the literature on NM. I can only finish with an ultrabrief note on the much-debated issue of productivity of WFRs. Elsewhere (Dressler 1977a: 15; 1977b) I have shown 1) that productive WFRs cannot exist in languages which lack the function of lexical enrichment (e.g. jargon stages of pidgins (cf. Mühlhäusler 1979) and decaying/dying languages), 2) that productivity of WFRs is a consequence of the semiotic principle of biuniqueness (one-meaning-one-form, but not in introflective languages, see § 5.3.3); thus fusional/inflecting languages where one meaning is typically expressed by more than one form (Skalička 1979: 451f) have few fully productive WFRs (on a par with, say Ger. *-bar* (=Eng. *-able*), agentive

-er); agglutinative WFRs (where the principle of biuniqueness is better served) are more productive (cf. Szabó 1970). 3) If the number of bases is smaller, WFRs should be more frequently applied in order to serve lexical enrichment (cf. § 4.5).—This has been suggested for Nahuatl by Whorf (1947: 392). Modern Hebrew has followed this technique instead of Ben Yehuda's suggestion of artificially increasing the number of consonantal roots used. In Chinese, the set of possible mono-morphemic words is severely restricted (only monosyllables, only /CV/ and /CVŋ/. Therefore compounding is extremely productive in Chinese (cf. Skalička 1979: 181ff; derivational morphology is only just emerging, according to current analyses).

§ 8. Descriptive work done in terms of NM can use different technical formats (cf. fn. 1) and can be combined with various structural and generative approaches. Here I have stressed properties of NM which are less characteristic of other approaches to WF.

Bibliography

Abbreviations as in the Linguistic Bibliography, plus:

auk	Arbeitspapiere, Institut für Sprachwissenschaft, Universität Köln
LAUT	Linguistic Agency, University of Trier
MPR	morphonological rule
NM	Natural Morphology
WF (R)	word formation (rule)
wlg	wiener linguistische gazette, Institut für Sprachwissenschaft, Universität Wien

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Complex Nominals: New Discoveries, New Questions

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0. The complex nominal is not a grammatical category with which the linguistic world is fully familiar. Yet explorations of the grammar of complex nominals have already raised a multitude of valuable research questions for contemporary linguistics in the areas of morphology, syntax, semantics, pragmatics, and linguistic universals. In this paper I shall describe my own research on complex nominals, and attempt to sketch the richness of theoretical issues that this research has uncovered. In the first part of the paper, I will summarize the major claims of the theory of complex nominals presented in much fuller detail in Levi 1978 (*The Syntax and Semantics of Complex Nominals*, henceforth, SSCN). In the second part, I will attempt to clear up several areas of common misunderstanding pertaining to complex nominals (henceforth, CNs); in the third and last part, I will outline some significant research problems posed by CNs for work in the 1980's and beyond.

1. Major Claims of my Theory of Complex Nominals

1.1. Complex Nominals: A New Unity

Although the term "complex nominal" is, I believe, a new one in grammatical literature, in fact we linguists have spent many years in scrutinizing at least some members of the category of complex nominals, namely, the so-called "compound noun" and, to a lesser extent, nominalizations of various forms and functions. It is my claim, however, that we can reach a deeper and more adequate understanding of both these constructions by recognizing that they belong to a larger set of grammatical forms that I have named "complex nominals" (SSCN, pp. 38-9). Let us first see what the data in question look like, and then review briefly some of the discoveries that support my claim that all these data exemplify a single grammatical construction whose superficial variety only thinly disguises a broad range of linguistic commonalities.

The term "complex nominal" encompasses the three partially overlapping sets of data shown in (1)-(3) below; (1) represents "compound nouns" or "nominal compounds," (2) illustrates nominalizations of different kinds, while (3) shows nouns modified by what have been variously called "nonpredicating adjectives," "pseudo-adjectives," "denominal adjectives," and "attributive-only adjectives."

1. apple cake, atom bomb, daisy chain, student power, autumn rains, color television, surface tension, language problem, girlfriend
2. parental refusal, American attack, musical criticism, constitutional

- amendment, film producer, dream analysis, quantifier lowering
3. electric clock, electric shock, electric gauge, electrical outlet; musical clock, musical criticism, musical interlude, musical talent

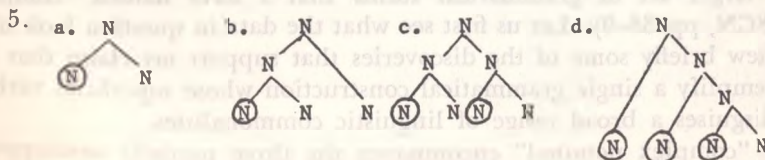
Although the data in these three groups tend to be cited as if they were limited in occurrence to one- or two-word expressions (e.g., *girlfriend*, *student friend*), in fact the recursive nature of CN formation permits us to form longer and more complex examples, of which the data in (4) provide only a suggestion:

4. a. apple cake ingredient list revision requests
- b. industrial engineering night school course curriculum planning
- c. senatorial national export advisory panel investigation coverup scandal reports
- d. musical criticism journal editorial board monetary compensation review panel appointee announcement delays

In most contexts, of course, stylistic and psychological processing constraints prevent us from selecting such lengthy and complex forms for normal communication (although bureaucratic and technical publications provide two familiar exceptions to such standards). Nevertheless, there can be no doubt that the grammar of CNs cannot be arbitrarily restrained in such a way as to produce, say, only three- or four- or five-word constructions of this type.

1.2. Syntax and Word Formation of CNs

Binary structure. One of the striking morphosyntactic features of this superficially heterogeneous category is the fact that all CNs may be shown to share a common internal structure, characterized by binary adjunctions of single nouns and, in English, the possibility of adjectivalizing (where both lexical resources and stylistic preferences permit) any noun that terminates a left-branch of such adjunctions. A few of the possible surface structures of CNs are shown in (5), with circled nouns representing those that may be adjectivalized in English; actual CN forms that correspond to each of these structures are provided in (6), with hyphens added to emphasize binary adjunctions.



6. a. music box (also: musical clock)
- b. bird-sanctuary fund (also: avian-sanctuary fund)
- c. recreation-department finance-manager (also: recreational-department financial-manager)
- d. autumn industry water-pollution (also: autumnal industrial aquatic-pollution)

Syntactic derivations. One major claim of my research is that all non-lexicalized CNs can be transformationally derived by just two syntactic processes:

predicate deletion and predicate nominalization. In all cases, the source structure for a surface CN consists of an underlying NP structure containing a head noun and a full S in either a relative clause or NP complement construction; in the course of deriving the CN, the head noun and the major constituents of the associated S are transformed and restructured in such a way as to emerge on the surface in the classic CN configuration described just above and illustrated in (5a). (For full details of the rules and processes which produce CNs, see Chapters 4 and 5 of SSCN.) CNs may be derived solely by predicate deletion, solely by predicate nominalization, or by a combination of the two processes, depending on the nature of the embedded S. In addition, the derivation may involve just one instance of predicate deletion, just one of predicate nominalization, or more than one of each or both, depending on the complexity of the input structures. The recursive and combinatory possibilities are literally endless, and it is only stylistic or processing-associated constraints that prevent us from more fully exploiting the unlimited productivity that characterizes the grammatical potential of CN formation.

Examples of two-element CNs derived solely by predicate deletion are shown in (7); examples of two-element CNs derived solely by predicate nominalization follow in (8). (Lengthier and more complex examples, involving repeated applications of the rules in question and/or combinations of the two types illustrated, are left to the reader's imagination. See also SSCN, Section 5.4.)

7. Deletion of CAUSE: flu virus, tear gas; future shock, snow blindness
 Deletion of HAVE: picture book, fruit tree; student power, lemon peel
 Deletion of MAKE: music box, honey bee; daisy chain, bronze statue
 Deletion of USE: steam iron, water wheel, faith cure, affixal negation
 Deletion of BE: textbook, student friends, lion cub, head noun
 Deletion of IN: desert rat, summer months, family tension
 Deletion of FOR: nose drops, plant food, bull ring, football season
 Deletion of FROM: grain alcohol, plum wine, store clothes
 Deletion of ABOUT: tax law, history conference, love song, sex scandal
8. a. ACT Nominalizations
 Modifier from subject NP: cell division, papal vow, parental veto
 Modifier from object NP: dream analysis, birth control, car repair
- b. PRODUCT Nominalizations
 Modifier from subject NP: clerical errors, faculty decisions
 Modifier from object NP: oil imports, book requests, food supplies
- c. AGENT Nominalizations (modifier from object NP):
 city planner, blood donor, mail sorter, financial analyst
- d. PATIENT Nominalizations (modifier from subject NP):

student inventions, factory rejects, college employees

Another claim of my theory is that a small and specifiable set of Recoverably Deletable Predicates can be identified such that only members of that highly restricted and very unusual set (or, in the actual derivations of SSCN, lexicalizations of those members) may be deleted in order to form the more compact CN construction. In Levi 1978 I claim that this special set comprises just these predicates: CAUSE, HAVE, MAKE, USE, BE, IN, FOR, FROM, and ABOUT. What is remarkable about this set is not simply the fact that just these nine predicates appear to account for all the systematic patterns of meaning that can be associated with nonlexicalized, nonidiomatic CN forms, but also the fact that these predicates play a very special role in the grammar of the world's languages in areas having nothing to do with CN formation per se. Specifically, it appears that most of these predicates are syntactically distinctive with respect to two features I have called "grammaticizability" and "surface invisibility." "Grammaticizability" refers to the ability of a limited set of semantically very basic predicates to appear as bound morphemes in many languages; for example, the predicate CAUSE is frequently expressed as a bound morpheme within a causative verb (as in Japanese *-(s)ase*) or by a distinctive conjugational pattern (as in Arabic and Hebrew), while the predicates BE, USE, IN, FOR, FROM, and HAVE may surface as Nominative, Instrumental, Locative, Benefactive, Ablative (Source), and Possessive Genitive case markers, respectively. The number of predicates that may be thus reduced, or "grammaticized," is very small indeed, and it is unlikely to be pure coincidence that the set of grammaticizable predicates overlaps so extensively with the set of Recoverably Deletable Predicates involved in CN formation.

In addition, the term "surface invisibility" refers to that quality of a predicate which "permits" it to remain unexpressed in surface lexemes yet be clearly and unambiguously recoverable from the surface structure; the term is intended to apply to predicates which may be analyzed as having been either syntactically deleted or lexically incorporated (in the sense of Gruber 1965). In normal English sentences, the predicates BE, IN, and HAVE are deletable, while CAUSE is incorporable into many verbs; in addition, the predicates FOR and ABOUT are deletable in restricted contextual domains (FOR in signs, and ABOUT in titles of various objects). (See SSCN, pp. 161-4.) Once again, we find a remarkable degree of overlap between the predicates involved in CN formation and a set of predicates that are grammatically distinctive on wholly independent grounds.

The chart shown in (9) (from SSCN, p. 165) summarizes the interrelationships of these three predicate sets; "RDP" here and below refers to the Recoverably Deletable Predicates present in the underlying structures of many CNs.

9,	CAUSE	HAVE	MAKE	USE	BE	IN	FOR	FROM	ABOUT
RDP	✓	✓	✓	✓	✓	✓	✓	✓	✓
Invisible in SS	✓	✓			✓	✓	✓		✓
Grammaticizable	✓	✓		✓	✓	✓	✓	✓	

Evidence for RDP Deletion. Although RDP Deletion introduces more degrees of ambiguity into CN forms than we are accustomed to seeing in syntactic deletion rules outside the area of word formation, support for my analysis of RDP Deletion as one of the two fundamental processes involved in CN formation (the other being nominalization) is extensive and heterogeneous, involving semantic, syntactic, and morphological evidence. To begin with, the detailed analysis of CN formation in SSCN makes a large number of accurate semantic predictions concerning the potential meanings that can be regularly associated with a given CN form, as well as a large number of accurate syntactic predictions about parallel derivations involving predicates which do not delete. The analysis of RDP Deletion is also supported by the fact that it can account for all the similarities in syntactic and semantic behavior of CNs whose prenominal modifier is a nominal adjective (e.g., *electrical engineer*, *industrial pollution*, *thermal stress*) and CNs whose elements are morphologically only nouns (e.g., *mining engineer*, *factory pollution*, *divorce stress*).

Other sources of support include the fact that my analysis not only predicts but also explains the differences in (1) predicability and (2) nominalizability that can be observed between normal adjectives appearing as prenominal modifiers (cf. *her nervous father*, *her father who is nervous*, and *the nervousness of her father*) and the nominal adjectives that substitute for those nouns emerging as prenominal modifiers after RDP Deletion has operated to create a new CN (cf. *her nervous disorder*, **her disorder which is nervous*, **the nervousness of her disorder*). Finally, external motivation for the theory of CNs proposed in SSCN comes from the fact that the analysis of RDP Deletion, originally based solely on English data, appears to provide at least a productive starting point (and, in some cases, a very close to adequate hypothesis) for related research on CNs and functionally-equivalent structures in a variety of other languages.

New analysis of nominalizations. While RDP Deletion is required to generate one major category of CNs ("RDP CNs," illustrated in (1) above), a different syntactic process underlies the second major category of CNS (illustrated in (2) above): this process is that of nominalization. It operates to transform the predicate of an underlying S into the head noun of a surface CN, while the subject and/or the object may surface as prenominal modifiers of that head noun. In Chapter 5 of SSCN, I propose a major new analysis of nominalizations in English, applicable both to nominalizations that underlie CNs and to those that don't. Four semantic types of nominalizations are identified (ACT, PRODUCT, AGENT, and PATIENT, as shown in (8) above), and two related but distinct syntactic source structures are proposed for these four (one taking the form of a relative clause construction, and the other that of

a NP complement construction). This analysis, together with the very detailed derivational path proposed for all CNs derived by predicate nominalization, can account for a wide range of syntactic and semantic facts associated both with nominalizations in general, and with CNs derived by nominalization ("NOM CNs") in particular. These facts concern such phenomena as cooccurrence of NOM CNs with adverbs of different types, nonoccurring forms that might otherwise be expected (e.g., **bank robbers by women*), constituent structure of the many stylistic variants of NOM CNs, word order for preposed modifiers within NOM CNs, assignment and distribution of case markers for subject and object NPs, and the derivation of related forms involving nominalized head nouns with non-CN modifiers (cf. the CN *student evaluations* and the non-CN *students' evaluations* or *evaluations by/of students*).

1.3. Semantics and Pragmatics of CNs

Multiple ambiguity as norm. My analysis of CNs rests on the premise that CN forms are "inherently and regularly ambiguous over a predictable and relatively limited set of possible readings" (SSCN, p. 50). This multiple ambiguity stems from the fact that the CN form might have been derived by the deletion of any one of nine RDPs, or by the preposing of either a subject NP or object NP by one of the rules associated with the formation of NOM CNs. Nevertheless, multiple ambiguity over a predictable and specifiable set of semantic readings (or, in the generative semantic framework of SSCN, semantic source structures) is a far different claim than that put forth by such linguists as Jespersen (1942) and Chomsky (1970) to the effect that CNs were so pervasively idiosyncratic as to defy formal linguistic analysis.

Two of the most telling arguments against characterizing CNs as typically idiosyncratic are based on (1) the universally acknowledged recursiveness of the CN formation process, which would entail an infinite lexicon were we to treat every CN as a clump of idiosyncrasies that must be memorized like other unpredictable lexical information, and (2) the readily observable fact that novel CNs are frequently coined by speakers and understood by hearers with great ease precisely because of the predictable aspects of CN grammar. Of course,

it cannot be denied that there are idiosyncratic aspects to the grammar of complex nominals; these involve such factors as lexicalization, individual variation . . . , historical remnants in contemporary English, and a certain analytic indeterminacy related to the fact that knowing an appropriate referent for a CN is not the same as determining its grammatical derivation. But to conclude from these less tractable aspects of the subject that idiosyncrasy . . . is virtually a defining property of complex nominals is to make demonstrably untenable claims about the grammar of English and to ignore the weight of extensive evidence which in fact attests quite to the contrary (SSCN, pp. 52-3).

Disambiguation strategies. If our theoretical analysis indicates that CNs

are inherently ambiguous over more than a dozen potential readings, yet our daily experience demonstrates that we can both create and interpret novel CN forms with little or no practical difficulty, we must then seek an answer to the obvious question: how do we manage? The answer seems to be that we have mastered a whole complex of disambiguation strategies, both semantic and pragmatic, which are based upon judicious exploitation of the following kinds of potentially pertinent knowledge:

- a) "strictly grammatical" knowledge of the regularities of CN formation (e.g., of rules and derivations like those proposed in Chapters 4 and 5 of SSCN);
- b) knowledge of the most common reading and/or the most common referent in the speech community for a particular CN form;
- c) knowledge of lexicalized meanings, such as those for familiar CN metaphors (e.g., *iron curtain*, *hairpin turn*) or even more idiosyncratic CNs (e.g., *duck soup*, *horsefeather*);
- d) knowledge of naming patterns based on semantic class of head and modifier nouns, such as naming artifacts by their purpose (e.g., *culinary tools*, *study lamp*), living things by their habitat (e.g., *desert rat*, *water lily*), and human activities by time and place (e.g., *morning lectures*, *campus riots*) or by agent and patient (e.g., *royal orders*, *papal appeal* vs. *peasant conscription*, *child abuse*);
- e) knowledge of pragmatic principles governing the most effective and "cooperative" way of using CN forms to achieve successful reference (including the convention of endocentricity, the use of distinctive and permanent features as preferred sources of modifiers, and the avoidance of negative or predictable relationships on which to base the surface form).

Illustrations of the way we exploit these different kinds of knowledge in specific contexts may be found in SSCN, Sections 4.4 and 6.3. Rather than repeat these examples here, I will conclude this section with the following characterization of the semantic and pragmatic principles that make CN disambiguation possible:

The basic function of these principles, when used in conjunction with the syntactic and semantic constraints on CN formation, seems to be one of helping the listener to select from the grammatically possible semantic structures the one reading that is contextually most plausible, and then (or, more probably, simultaneously) to figure out what real world object could be appropriately named by such a form... Although these principles do not preclude the possibility of misunderstandings and misinterpretations of CNs in discourse, they nonetheless reduce it sufficiently so that the expressive concision and stylistic flexibility that the unique CN construction has to offer can be systematically and creatively exploited in natural language (SSCN, pp. 238, 244).

2. Some Common Misunderstandings About CNs

2.1. Meaning \neq Referent, Form Function

It should be clear from the preceding discussion that a theory of the grammar of CNs must necessarily encompass a wide range of linguistic facts, including morphological, syntactic, semantic, and even phonological data. On the other hand, there are also many phenomena that, I believe, fall outside the set of facts that any theory of CN grammar (as opposed to a theory of the creation, use, and interpretation of CN tokens in specific communicative contexts) can be expected to account for. For example, no theory of CN grammar can predict such phenomena as (1) lexicalized meanings that come to be associated with CN forms, (2) which CN forms are "actually occurring" as opposed to CNs that are "potential but nonexistent" in the language (see 2.2 below), or (3) the fact that one reading, out of the entire set of predictable readings associated with a given form, has become the most common or institutionalized reading for that form at a particular time and place (e.g., the fact that the most common readings for *music box*, *tool box*, and *metal box* in my community correspond to 'music-making box,' 'box for tools,' and 'box made of metal,' respectively), even though alternate, less common readings remain part of the native speaker's competence, and indeed form the basis for puns and other linguistic jokes.

The development of an institutionalized or customary reading for a particular CN form usually reflects the prominence in a particular speech community of a particular referent or referent set which is regularly referred to by that CN form. However, it is essential that linguists not confuse the real-world properties of that customary referent, or of some prototypical member of that referent set, with the components of meaning that can be predictably associated by the grammar with that CN form. Instead, we must remember that what a grammar of CNs must account for is all the possible matchings of CN forms and predictable readings that the competence of the native speaker permits, regardless of which pairings happen to be in use in a particular speech community during a particular period.

Of course, it may also happen that a given CN form is never used by speakers in a community to name an object at all; this would be likely in such situations as these:

- 1) No appropriate referent for that CN seems to exist at the time (e.g., an electricity-generating egg that could be called an *electrical egg*, using the same reading as that which underlies the more familiar CN *electric eel*, used to name a creature we see mostly in aquariums).
- 2) Speakers believe no appropriate referent for that CN could ever exist (e.g., eggs produced by paramecia, to be called *paramecium eggs*).
- 3) Speakers happen not to be familiar with an appropriate referent in plentiful existence elsewhere (e.g., the egg-shaped plastic containers seen in American supermarkets whose contents are pairs of stockings mar-

keted under the punning brand name of *L'eggs*, and which could appropriately be referred to using a CN such as *hosiery eggs*).

However, if we wish to discuss the grammatical status of CNs such as *electrical egg*, *paramecium egg*, and *hosiery egg*, we must not base our conclusions about their grammaticality or about the possible meanings that a grammar might associate with these forms on the accidental, unpredictable, and basically non-linguistic facts concerning the presence or absence of appropriate referents for these forms in some particular speech community.

One case where just such an error appears is in the otherwise insightful study by Downing (1977) of the pragmatics of complex nominals. Downing appears to have misinterpreted the responses of her experimental subjects in a number of instances that suggest that she has failed to see the importance of the distinctions drawn in the preceding paragraphs.

In her study, Downing reports on several psycholinguistic experiments she conducted to determine some of the pragmatic factors that influence the choice by speakers and interpretation by hearers of (mostly) novel CNs. One of these experiments was a "context-free interpretation task," in which CN forms varying in familiarity from *lipstick* and *bullet hole* to *pea princess*, *bird door*, *night Democrat*, and *cow tree* were given to ten subjects, who were then "to provide one or more interpretations for any compounds they judged to be existing or possible" (Downing 1977: 818). In a second experiment, "subjects were asked to rate as likely, possible, or impossible a number of proposed interpretations for 18 compounds of varying degrees of lexicalization," such as *cow-pony*, *cousin-chair*, *egg-bird*, *fork-spoon*, and *pumpkin bus* [ibid].

In her discussion of the subjects' responses, Downing at times appears to be confusing (a) the semantic relationship that characterizes a possible source structure for a given CN form, and (b) the real-world relationships that may or may not characterize a possible referent for that form. For example, Downing first reports that although some subjects rejected as "unacceptable" CN forms such as *hog-pork*, *time-hour*, and *head-hat* on the grounds that the modifier noun provided redundant information, others recognized that the form would be not only meaningful but also non-redundant in certain contexts, as suggested by these explanations (taken from subjects' comments):

- (21) ... *hog-pork*: When soy pork is a common dish, *hog-pork* will describe the genuine article.
- (22) *hog-pork*: pork from a hog, as opposed to a sow or piglet (Downing 1977: 833).

However, Downing concludes this section by saying:

It is thus apparent that the appropriateness of a given underlying compounding relationship cannot be evaluated absolutely, in the absence of contextual considerations. A relationship which carries useful information in one situation may be semantically empty in another (Downing 1977: 834, emphasis added).

I believe Downing is confusing linguistic and extralinguistic facts in this

quotation. That which we call a "semantic relationship" can never be "semantically empty," or it would be a non-relationship. In the data of (21)–(22), the CN *hog-pork* has been interpreted by both speakers as a realization of an underlying form comparable to *pork from (a) hog*, that is, as a CN based on the semantic relationship of Source (encoded as FROM in SSCN). In fact, all the subjects reported on implicitly matched the form *hog-pork* to this reading, which is one of the readings predictably associated with that form by the grammar of CNs. Note, however, that the matching of this form to at least one such reading is in fact a prerequisite step to any subsequent judgment about whether using that CN form on that reading would be useful in communication or not. The only difference among the subjects is that only two of them recognized that the form *hog-pork* might be used to draw an effective contrast between its plausible referent and other related food types. Nevertheless, their comments, like those of the other subjects discussed, are not comments about the underlying semantic relationship of Source, which is certainly not an "empty" one, but rather about the appropriateness of choosing that CN within one context or another. (Similarly, when Downing's subjects produce a judgment that CNs like *bird-door*, *night-Democrat*, or *cow-pony* are "unacceptable" CNs because, in essence, "no such things exist," they are not judging the grammaticality of the forms but rather the likelihood of their needing such forms to describe things in their own world. Here too, they could not have made their judgments about the existence of any appropriate referent had they not already correctly identified one of the grammatically predictable readings that English grammar regularly associates with the CN in question.)

Downing might thus better have concluded that section by saying what she appears to recognize tacitly in other parts of her article, namely, "A CN form interpreted on the basis of one semantic relationship might be useful for achieving successful reference in one situation, but may be redundant, misleading, or otherwise uninformative in another." In other words, the choice of when to use certain CN forms, like the choice of when to use certain non-compound words or even certain sentences, is a pragmatic choice which must not be confused with the question of what relationships that form (or word, or sentence) can in general express.

I have discussed the examples from Downing 1977 not in order to criticize the overall value of Downing's work, which I consider to be extremely helpful, but rather to emphasize the importance of preserving a distinction, in every discussion of CNs, between CN forms and CN functions. My own work has focused on the syntax and semantics of the former, while the work of Downing and others (e.g., Zimmer 1972), has emphasized the latter. While I believe each of the two approaches is necessary and complementary to the other, it is nevertheless important that the objectives and findings of the one not be mistaken for those of the other.

2.2 One Consequence of Unbounded Productivity

The process of forming complex nominals in English is universally acknowledged to be a recursive one: the output of one derivation can be the input to another, with the result that there are no grammatical limits on the length or complexity of possible CN forms, as hinted at earlier by the data in (4). Moreover, as Downing points out (1977: 836-7):

Because the compounding process is extremely productive, and because compounds are considerably more transparent than novel monomorphemes, compounds are ideally suited to serve as ad-hoc names.

As a result, the already large number of CN forms in common use is continually augmented by the creation and interpretation of novel CNs in everyday discourse.

One important consequence of this unbounded productivity is that any analysis that seeks to categorize a given CN form as "attested" or "existent" as opposed to "possible but nonexistent" (as in Roeper and Siegel 1978) or as "[not] actually occurring" (as in Jackendoff 1975) or, worse yet, as "unacceptable" or "impossible" (as suggested in Downing 1977) is inherently a misguided one. The reality of the open-endedness of CN formation is such that we must recognize the fact that:

...any attempt even to specify which CNs are attested at a given point in the history of a language is doomed to failure since (a) there is no way to prove that the absence of a form is due to anything but the finite size of one's corpus of examples; and (b) the set of CNs in the repertoire of any individual speaker (or linguist) is limited to a high degree by extralinguistic factors such as education, social class, ethnic background, occupation, geographical location, and even hobbies, so that a CN which strikes one speaker as meaningless (and hence "unacceptable" or "impossible") may be fully comprehensible, transparent, and in frequent use for the next (SSCN, p. 56).

Thus, it is hard to imagine what linguistic criteria could provide motivation for the asterisks assigned to such CN forms as *ant man*, *garbage gingerbread*, and *garbage tree* in Jackendoff 1975 (p. 655), or for the author's assertion that:

we would say that someone did not know English if he (seriously) used *garbage man* to mean 'a man made out of garbage', by analogy with *snowman* (*ibid.*).

Actually, it would be more accurate to say that "someone did not know English" if he did not recognize that all four CNs just cited are perfectly possible forms in English; the productive semantic source for *garbage man* is provided by Jackendoff himself, while the productivity of the patterns for the other three is attested to by three remarkably parallel CNs that really are "actually occurring" forms in contemporary American usage, namely, *spider man* (name of a character with spider-like qualities in American children's comics), *garbage pizza* (name used for a pizza with a large number of added ingredients), and

junk sculpture (term used to refer to sculpture whose basic components are literally junk, i.e. discarded items).

Jackendoff's error, which we might call the Omniscient Linguist Fallacy, is only compounded when he moves from the particular cases just cited to the statement of his general theoretical position, namely, that "in the lexicalist theory... we simply give each **actually occurring compound** a fully specified lexical entry" (Jackendoff 1975: 655). The error committed here is much the same as that referred to by Zimmer in another but related morphological context (Zimmer 1964: 32):

A listing of semantically transparent attested forms (which in any case is in practice bound to be incomplete) is hardly less futile than an attempt to count the drops in a pool during a rainstorm.

Any Scrabble player knows that there are thousands of "actually occurring" words in the language that he or she has never personally encountered, but whose "existence" cannot be denied. What I have tried to emphasize in this section is that, *a fortiori*, any linguist ought to realize that the open-endedness of CN formation makes the set of "actually occurring" CNs in the language not just more difficult for any one person to identify in practice, but rather, impossible in principle.

3. Research Problems for the 1980's and Beyond

In this concluding section, I would like simply to sketch out some of the theoretical questions raised by the study of complex nominals that constitute important research areas for linguists to explore in the years ahead. These questions primarily concern (1) the relationship of word formation to syntax, (2) the roles and interactions of semantics and pragmatics in regard to CNs, and (3) linguistic universals involving CNs.

3.1. Word Formation and Syntax

The study of CNs raises a number of difficult questions in the more general area of the relationship of word formation to syntax. Two of these are the superficially simple but rarely well-answered questions, "What is a word?" and "What is the domain of word formation?" In regard to the first question, although linguists generally recognize that defining characteristics of words must vary from language to language, very few have examined the problem of how CNs might or might not fit into the category of "words" as defined for any specific language.

The related question of "What is the domain of word formation?" is even more problematic. One traditional approach to word formation is to view it as a domain consisting of two subparts: (1) "morphology" (the study of words formed from single stems or roots by means of affixation, internal changes, zero morphemes, and other such adjustments to that single stem or root), and (2) "compounding" (the study of words formed by adjunctions of free forms

only). Under this view, CN formation would fit squarely in the domain of word formation and consequently CNs would be classified as words. And yet, a consideration of the full range of possible CN forms (such as those shown in (4) above) suggests that we may not really wish to treat all CN forms as representing single words, if only because the recursive processes that produce CNs, and the remarkably complex forms that can thus be created, are qualitatively different in significant ways from the morphological processes and outputs typical of a language like English. One solution might be to recognize that the word stock of a language like English must draw on two distinct categories of words, namely, "basic words" and "compound words." Such a distinction might permit us more easily to recognize the syntactic and functional properties that CNs and other compounds share with non-compounded words, as well as the ways in which the former differ systematically from the latter. (This distinction corresponds, of course, to the traditional divisions of word formation just cited, but the consequences of this distinction have seldom been explored in at least contemporary American linguistics, and thus present important challenges to recent linguistic theories.) The question then arises as to whether this distinction is as valuable, or as clear-cut, for languages other than English, and whether other languages present additional complications in regard to where either division of word formation should be "located" in the grammar.

These and other questions raised by CN research will certainly not be answered in this paper, but it is equally certain that they will not be answered until linguists have carefully re-examined the traditional distinctions between morphology and syntax or, more broadly, between word formation and syntax. Sadock 1980, for example, uses data from Greenlandic Eskimo, a polysynthetic language, to show how that language "falsifies pronouncements concerning the independence of syntax and word formation based on data from languages that are typologically very different from Greenlandic" (Sadock 1980: 300). I suspect that crosslinguistic data on CNs may be equally important in achieving a more sophisticated view than our current one(s) concerning the domain of word formation in the world's languages.

3.2. Semantics, Pragmatics, and Complex Nominals

The discussion in Section 2 concerning the difference between the meaning(s) of CN forms and the properties of CN referents should amply illustrate the difficulties in maintaining a clear separation between a semantic analysis of CN form types and a pragmatic (or psycholinguistic) analysis of the use of particular CN tokens in specific communicative contexts. The problems in this area are of course compounded by rampant disagreement among linguists as to the nature of, and relationship between, semantics and pragmatics. Rather than prodding this particular hornet's nest further in this paper, I shall instead refer the reader to some pertinent discussion elsewhere (SSCN, Sections 3.3 and 6.3) and conclude briefly with the suggestion that the data on CNs appear

to provide an unusually rich source of material for studying the interaction of morphological, syntactic, semantic, and pragmatic principles.

3.3. Complex Nominals and Linguistic Universals

My research on CNs has uncovered a number of facts suggesting that: the process of CN formation in any natural language is governed in part by a network of syntactic, semantic, and pragmatic universals and in part by a system of language-specific modifications and preferences (SSCN, p. 254). Although I can make only the most preliminary of proposals in this area, I would like to present here a small number of the more promising hypotheses. In the area of syntax, I would propose the following two possible universals: (1) that all CNs are formed by the adjunction of two nouns at a time, thereby producing the binary branching structure illustrated earlier in (5); and (2) that all CNs for which we can justify a syntactic derivation can be generated by the two processes of predicate deletion and predicate nominalization. In the area of semantics, I would propose the following as good candidates for universals: (1) that there exists a universal set of Recoverably Deletable Predicates that underlie CN formation in all languages that have CNs; (2) that many of the naming patterns based on semantic class of head noun and modifier noun, described earlier as one component of our disambiguation strategies, will be found universally; and (3) that certain of the semantic relationships expressed by the nine RDPs will be found universally to be considerably more productive than others (e.g., the relationships expressed by HAVE, FOR, and IN seem far more productive than those expressed by CAUSE or MAKE). Finally, in the area of pragmatics, I would propose that the "principles of cooperative naming" alluded to earlier would remain the same across languages, such that we would expect to find everywhere a pragmatically-motivated preference for CNs that are endocentric, and whose modifiers reflect a distinctive, positively-expressed, and habitual relationship between the referents of the two component nouns. (For further discussion, see SSCN Section 4.5 and 6.4)

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An Expanded View of Morphology in the Syntax-Phonology Interface*

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Introduction

The view that morphology constitutes a separate level of linguistic structure, hence a separate component in linguistic description, has a long tradition, and a great many proposals have been advanced, in an assortment of theoretical frameworks, as to the details of a morphological component. Here I will be adding to this literature, in an attempt to work out some of the consequences of four very general assumptions about the character of linguistic descriptions, within the framework of generative-transformational grammar (broadly conceived).

The first two of these assumptions, taken together, constitute what Hale, Jeanne, and Platero (1977) have referred to as the Autonomous Systems view of language structure: 'According to this view, language consists of a number of distinct systems, each possessing inherent principles of organization that are essentially independent of factors relating to any other linguistic system or to extralinguistic considerations' (379). The first assumption here is of *high modularity*: language consists of distinct systems, and a linguistic description consists of correspondingly distinct modules, or components. These systems are distinct in the sense that the principles governing linguistic structure in one system may be of quite a different character from those governing structure in any other system, so that one component in a description will have its own formal constraints and internal logic, potentially quite different from those of any other component. Moreover, according to this first assumption there are a number of such systems. I will in fact pursue a strategy of splitting rather than lumping: that is, I will tend to assume additional modules in linguistic description whenever there is evidence to support this move.

As a result, I will assume (without argument, given the space available to me) that the traditional domain of morphology encompasses several distinct

* Many of the ideas and analyses reported on here were developed in collaboration with Geoffrey Pullum during work on our forthcoming book, *The Syntax-Phonology Interface*. Pullum has not seen this paper, however, and it presents some proposals not (yet) in our manuscript, so that he cannot be held responsible for any of its shortcomings. National Science Foundation and Fulbright grants, as well as support from University College London and the Ohio State University, have aided our research, and we have benefitted from the advice and criticism of a great many colleagues and students.

modules: a component of *word formation rules*¹⁾ (which ought to be termed *morphosyntax*, but the word has already been taken) specifying what morphemes can be combined with one another and what the sequence and structure of the result is; a component of *allomorphy rules*²⁾ specifying the morphophonemic shapes associated with morphemes and their combinations; and a component of *morphophonemic rules*³⁾ specifying the phonemic shapes so associated. Morphophonemics is in turn distinct from a *phonological* component proper, and the word formation rules distinct both from a component comprising the rules classed as cyclic syntactic operations by some writers and as lexical operations by others, and also from a component of (postcyclic) syntactic rules. There are no generally accepted names for these last two components; in what follows I shall refer to them as the *relational* and the *syntactic* component, respectively. Finally, I assume a *lexical* component, where lexical items are assembled and implicational relationships among their various properties ('lexical redundancy rules') are expressed.

The second assumption in the quotation from Hale et al. is of *autonomy*: linguistic structure as a whole is independent of extralinguistic factors. That is, the operation of rules in any module of a linguistic description will not depend upon whether an associated piece of discourse is bizarre in meaning, pointless, lacking in grace, hard to comprehend, rude, hard to pronounce, metrically regular, devious in intent, previously encountered, novel in form, or open to multiple interpretations, to mention just a few factors assignable to accounts of the purposive use of language, speech perception, speech production, the social 'meanings' of linguistic forms, stylistics, and poetics, rather than to an account of the structure of language.

A third assumption, one that Hale et al. would undoubtedly subscribe to, concerns the way in which modules can interact with one another. There would

1) The term is from Aronoff 1976, though I do not necessarily subscribe to all the details of Aronoff's proposals. I take no position here on several important questions—in particular, whether derivational and inflectional morphology should be distinguished as separate components, as has often been suggested; and whether compound formation and/or incorporation should be treated as word formation rules, as syntactic rules, or as a separate component. (Sadock 1980 provides arguments that the first of these positions is undesirable for noun incorporation in Greenlandic).

2) The term is taken from Aronoff 1976, where it has a somewhat narrower sense ('allomorphy rules' and 'truncation rules' together constituting a component of 'adjustment rules'). The proposal to distinguish operations with the combined effect of 'spelling out' abstractly represented morphemes from morphophonemic rules has been made by several scholars, using somewhat different criteria and terminology; in addition to Aronoff, I should also cite Matthews' (1972: sec. 5.2 et passim) distinction between 'morpholexics' and 'morphophonemics' and Dressler's (to appear) distinction between 'morphology' and 'morphonology' (see also the earlier presentation in Dressler 1977).

3) Orthodox generative phonology lumps morphophonemic and allophonic operations together as 'phonology', but I will subscribe here to some version of the traditional morphophonemics/phonemics distinction, Stampe's rule/process distinction (Donegan and Stampe 1979), or Dressler's (to appear) morphonology/phonology distinction. The literature on the question is by now too vast for brief summary or citation.

be little point in distinguishing components if in general the application of a rule in one component could be contingent upon representations available only in another component.⁴⁾ Instead, it is customary to see each component as a device for relating two (or possibly more) types of linguistic representations and to require that these levels of representation serve as the only available interfaces⁵⁾ between components. For instance, the morphophonemic component relates morphophonemic representations and phonemic representations, so that according to the assumption of *limited interfacing* only these two types of representations may link the morphophonemic component to any other; for the purposes of other components, the morphophonemic component is a black box, the internal workings of which are not determinable, which supplies pairings of morphophonemic and phonemic representations.

Finally, I will make an assumption of *directionality* with respect to the way in which any particular component operates to relate its two types of representations. I assume that one level of representation is logically prior to the other, so that the component can be seen as taking one level of representation as input and as mapping it into the other as output. The interface between two components will then be a level of linguistic representation serving as the output of one component and the input to another, and an asymmetrical relationship of applicational precedence will hold between the two components: operations in the first component can feed or bleed operations in the second, but not vice versa. In the case of the morphophonemic component, morphophonemic representations serve as input and phonemic representations as output; for the phonological component, phonemic representations are input and phonetic representations output; the level of phonemic representation is the interface between the two components; and we predict that morphophonemic rules can feed or bleed phonological rules but cannot be fed or bled by them. Similarly, the level of *shallow structure* is the interface between the relational and syntactic components I mentioned earlier; and we predict that rules in the relational component can feed or bleed syntactic rules but that the reverse is impossible.⁶⁾

All this is by way of preface to my central question in this paper: What lies in the interface between syntax and phonology? Clearly, all of traditional

4) The position rejected here is that components interpenetrate, a position explicitly championed by Hetzron 1972, Awbery 1975, and Tegey 1975 (among others) for syntax vis-à-vis phonology, and by R. Lakoff (1974: XVIII-40) for components in general:

Very simply, there is no separation of levels: a single, highly abstract, underlying structure underlies the semantics, the syntax, and the phonology, and further, syntactic information may be used in the statement of phonological or semantic rules, and conversely.

5) This use of the term *interface* is due to Awbery 1975, who refers to the assumption of limited interfacing as the Interface Model.

6) Lapointe 1980: 67 similarly proposes that syntax and word formation rules are autonomous and have a limited interface:

the syntax and the morphology are distinct parts of the grammar, each with its own system of categories and PS rules governed by restrictions unique to each component; the two parts interact only at the point in a syntactic derivation where lexical insertion takes place...

morphology: word formation rules, allomorphy rules, and morphophonemic rules work together to convert syntactic representations into strings of phonemes. But can the output of the syntactic component be identified with the input to the word formation rule component? Does this (already rather large) list of components exhaust the set of components intervening between syntax and phonology? I will suggest that the answer to both questions is no, that in fact a number of components, in addition to the three of traditional morphology, are located between syntax and phonology—at least those containing surface filters, readjustment/cliticization rules, sentence prosody rules, and free deletion rules—and most of these components appear to be located between syntax and the components of traditional morphology, while one (surface filters) appears to be located among the components of traditional morphology. As a result, I conclude that the appropriate sense of 'morphology' in linguistic description is much wider than is customarily thought⁷⁾ and includes a number of principles usually classified as 'syntactic'.⁸⁾

In what follows I will depend mainly on observations about the applicational precedence of various types of rules. Such observations are relevant by virtue of the assumptions of high modularity, limited interfacing, and directionality described above (the assumption of autonomy serving primarily to restrict the domain of inquiry to what is presumably a coherent set of phenomena). My suggestions will necessarily be somewhat tentative and incomplete, given the many possible interactions between components and the difficulty of assigning particular phenomena confidently to one component or another.

Surface filters

Along with the first extended arguments (in Ross' 1967 dissertation and Perlmutter's 1968 dissertation, published as Perlmutter 1971) that the syntactic component should be supplemented with a set of filters ('surface structure constraints') applying after the operation of all syntactic rules came the obser-

7) Similar ideas were advanced by early investigators of morphology in generative grammar, in particular Kiefer 1973 (drawing on such works as Bierwisch 1967 and Wurzel 1970), as well as in Zwicky 1969: 453 and later in Rivero and Walker 1976.

8) This proposal resembles in certain respects a version of 'core grammar' as set out by Chomsky in a number of recent publications, for instance Chomsky 1980: 3—

base rules	
transformational rules	
deletion rules	construal rules
filters	interpretive rules
phonology and stylistic rules	conditions on binding

(it is the left, or nonsemantic, side of this diagram that interests us here). In what follows I agree with the ordering of transformational rules (my 'syntax') before deletion rules (the 'free deletions' discussed below) and of these in turn before filters. But I interpose further components between syntax and Chomsky's 'phonology' (= morphology plus phonology), locate filters after some parts of 'phonology', and deny the existence of a component of stylistic rules, much less such a component ordered as part of, or after, 'phonology': rules in *any* component may be optional/variable/restricted to certain styles or registers.

vation that some of these filters are sensitive to certain phonological properties of the morphemes involved. Stress level, number of syllables, and the phonological identity of morphologically distinct elements all appear to play some role in surface filters; and the filters are 'blind' to morphemes with a null phonological realization (Perlmutter's chapter 3 gives examples of reference to phonological properties and of filter blindness).

It follows that surface filters do not apply *at least* until some aspects of phonological form are associated with morphemes. Given my assumptions, surface filters should apply after all allomorphy rules, or after some component fed by allomorphy rules. Schachter 1974 provides evidence from Tagalog that surface filters do indeed follow certain rules of allomorphy. Tagalog clitic combinations obey a constraint that requires monosyllabic pronoun clitics to precede clitic particles, and these in turn to precede disyllabic pronoun clitics. Moreover, the combination of the two monosyllabic pronoun clitics *ko* and *ka* is realized neither as *ko ka* nor as *ka ko*, but rather by the suppletive portmanteau *kita*, a disyllabic form. It turns out that *kita* behaves like one disyllabic pronoun for the purposes of the ordering filter, and not like a sequence of two monosyllabic pronouns; the suppletion feeds the filter. Szamosi's (1976) discussion of a filter on inflectional forms in Hungarian leads to a similar conclusion.

There is some evidence from English suggesting that filters are located *between* the allomorphy and the morphophonemic components. The facts in question have for some time been seen, correctly in my opinion, as problematical for the view that surface filters apply to the output of the syntactic component (Schmerling 1973, Rivero and Walker 1976). English is among the languages exhibiting a Subject Requirement Constraint: Any sentence other than an imperative in which there is an S that does not contain a subject in surface structure is ungrammatical (adapted from Perlmutter 1971: ch. 4). Despite the general applicability of this filter in English (**Is a doctor*), in informal styles pronoun subjects are sometimes deletable: *Have to go now* 'I have to go now', *Like the weather?* 'Do you like the weather'. Schmerling suggested that the Subject Requirement Constraint is a *shallow structure filter* (and I would accept the possibility of a component of shallow structure filters as well as surface filters).⁹ However, there is reason to think that the deletion of pronoun subjects and auxiliary verbs in informal styles is achieved by morphophonemic rules. There is not space to develop this argument in detail, but it is based on observations in Akmajian, Demers, and Harnish 1971: ch. 9 and depends on the claim that the deletions affect only unstressed proclitics and on the observation that one of the deletions (of auxiliaries) not only affects a proper part of a word but also is fed by an undoubted morphophonemic rule (progressive voicing assimilation, so that '*S really cold in here*, with deleted *it*, begins with /srili/

9) G. Lakoff 1971: 250 and McCawley 1976 provide evidence that the English constraint against double negatives in one clause (**I didn't see nothing*) applies at the level of shallow structure, and Rivero and Walker 1976: 112f. extend these arguments to the corresponding constraint in Spanish.

while *'S really no hope*, with deleted *there*, begins with /zrili/).

The placement of filters between morphophonemics and phonology predicts that no syntactic, or even morphological, rules can apply after filters do. I believe this prediction is well supported. Clitic metathesis rules ordered after surface filters have occasionally been proposed (for instance, by Emonds 1978 for French), but I do not believe they survive scrutiny (in the French case, I would argue that there are separate filters for proclitics and enclitics, and no metathesis is called for to convert one into the other).

Sentence prosody

Here I refer to principles assigning intonational contours, stress patterns, and rhythms (timing and pause patterns) to sentences. I know of no firm evidence that these principles are ever sensitive to phonological properties of particular morphemes or to the morphological structure of individual words, while there are numerous cases in which the phonological realization of morphemes depends on sentence stress, at least—consider, in particular, the many morphemes with 'weak' and 'strong' forms. There are numerous cases as well in which syntactic movement rules feed these principles; note, for example, Bing's (1979: ch. 7) arguments that the intonation contours of the constructions in *Mary plans for John to marry her, and marry her he will* and *Here comes John* are assigned after the movement rules they illustrate. I conclude that any sentence prosody component follows syntax and precedes word formation rules and allomorphy rules.

Free deletion

By *free deletion* I mean any deletion rule not subject to a condition that the deleted element be identical to some specified element; the rule might have conditions of other sorts on it, of course. The paradigm example of a free deletion rule is one deleting subject pronouns (in the many languages, like Finnish and Spanish, not exhibiting a Subject Requirement Constraint). So far as I know, such rules always delete specified morphemes or words, and not larger constituents.

The component I intend to distinguish here is 'syntactic', in the sense that the rules involved must be stated so as to delete whole words. Many free deletions are 'morphological', however. The deletion of certain verbal prefixes in Sarcee (as discussed by Cook 1971b), for instance, is clearly not syntactic: proper subparts of words are deleted; the deletions are subject to a condition referring to number of syllables; and (according to Cook 1971a) one of these deletions must follow word-level tone rules. Presumably these rules are morphophonemic, as are the English informal style deletions I mentioned above, and also several Swahili prefix deletions (Brandon 1975). The rules of subject pronoun deletion and initial particle deletion in Welsh (Awbery 1975) also appear not to be syntactic. These deletions are fed by the mutation rules of Welsh, which are allomorphy rules. They involve specified words—but words which are also

clitics, hence in one sense proper subparts of words.

To take up a case of syntactic free deletion: in analyses of the English auxiliary *do* that assume it appears in underlying structure, a free deletion rule must be posited to account for the nonappearance of its forms in sentences like *He likes raccoons* (as opposed to *He does like raccoons*, *Does he like raccoons?* and so on). It seems that this rule applies only to *do* when it lacks sentence stress (**He does like raccoons*), a fact that suggests that free deletions follow the sentence prosody component.

As for the interaction of other components with (syntactic) free deletions, I know of no clearly syntactic rules that free deletions feed or bleed (which suggests that syntax precedes free deletion), nor do I know of any clearly morphological rules that feed or bleed free deletion (which suggests that free deletion precedes allomorphy and morphophonemics). The only 'phonological' condition on rules of free deletion that I know of involves reference to sentence stress, and in fact most free deletions seem to be free even of this condition (this seems to be so for the rule deleting the English pronoun *one(s)*, as in *You take the blue pencils, and I'll take the red (ones)*). Finally, free deletion clearly precedes surface filters, since the latter are blind to deleted material. The (tentative) conclusion to be drawn from all these observations is that free deletion is located between sentence prosody and the components of traditional morphology.

Readjustment and Cliticization

It has often been observed that the structures assigned by syntax are not precisely those required for the purposes of prosody, morphology and phonology. To begin with, syntax provides *more* structure than ever seems to be relevant for prosody rules (Bierwisch 1968)—this despite the fact that rules in later components can be sensitive to quite complex properties of syntactic structure (as in the well known case of French liaison, or for the Italian *raddoppiamento sintattico*; see Napoli and Nespor 1979). In addition, syntax sometimes provides the *wrong* structure, as in the familiar example of English embedded relative clauses (Chomsky 1965:13): the syntactic structure of *This is the cat that caught the rat that ate the cheese* simply does not locate major constituent breaks at the places required by principles governing timing and pausing in English. Rather than viewing the structures relevant for syntax and those relevant for prosody as two entirely different organizations of the same morphemic material, I will assume, with most other writers on the subject (e.g., Langendoen 1976 on the English relative clause case and Grosjean, Grosjean, and Lane 1979 on English pause phenomena in general), that the structures relevant for prosody are those supplied by syntax *as amended* by certain principles of readjustment. I will also make the simplifying assumption (implicit in most work in this area—e.g., Cooper and Paccia-Cooper 1980) that these readjusted structures serve equally for all aspects of sentence prosody (rhythm, intonation, and stress), as well as for all aspects of morphology and phonology. The assump-

tions could turn out to be false, but I am opting here for the simplest scheme consistent with the facts I am aware of.

I have now posited a separate readjustment component, located (essentially by definition) after syntax and before sentence prosody. The examples mentioned so far all involve phenomena with a *phrase* domain, but the readjustment component would also be the natural home for certain operations creating (phonological) *words*: the English contractible auxiliaries are syntactically phrased with following material—(*The girl in pink*) (*is asking to leave*)—but in their contracted forms are attached to the preceding word, as in *The girl in pink's asking to leave*; and German definite articles are syntactically phrased with a following noun—(*zu*) (*dem Bahnhof*) but in some cases can attach to a preceding preposition to create a portmanteau preposition, as in *zum Bahnhof*. Formally, these reattachments for phonological purposes can be expressed as readjustment rules Chomsky-adjoining material to a lexical (rather than phrasal) category, that is, as especially simple *cliticization* rules.

Next, I observe that it is not uncommon for clitics to have alternative loci of attachment, either to a lexical *or* to a phrasal category (a small sampling of cases is given in Zwicky 1977; Pullum 1981 develops a fairly detailed analysis of one instance in Luiseño). The existence of such phenomena suggests that cliticization processes in general should be incorporated within the readjustment component. Such processes create clitic elements as copies of, or replacements for, specified constituents, position the clitics within syntactic structures¹⁰)—locating them, for instance, next to the verb, as in French, or after the first word of the sentence, as in Tagalog—and attach them to the word (or phrase) they form a phonological unit with.

Though this is not the place to develop an account of cliticization rules (for some important preliminary observations, and a survey of the literature, see Klavans 1980), a few remarks about cliticization in relation to other components are obviously in order. I know of no clear cases in which cliticization rules feed or bleed syntactic rules¹¹) (though the reverse is commonplace). And, somewhat surprisingly, I know of no good cases of phonological properties conditioning cliticization (though the relevance of cliticization for allomorphy and morphophonemic rules is widespread, clitic elements often being subject to idiosyncratic rules of these types). Cliticization obviously precedes surface filters, since many such filters deal specifically with the combinability and sequencing of clitics. The relation of cliticization to sentence prosody is a vexed question,

10) Formally, these processes probably resemble rules of government and agreement.

11) There are several celebrated putative cases: in particular, English negative cliticization as treated by Zwicky 1969: 440–51 and by Selkirk 1972: 96–109 (where cliticization of *n't* feeds subject-auxiliary inversion) and English verb-complementizer contraction as treated by Bresnan 1971 (where the cliticization of *to* to a preceding *go*, *got*, *have*, etc. is assumed to be cyclic, that is, located within the relational component). I believe that both analyses are demonstrably wrong and that better ones can be devised, though there is not sufficient space to develop these ideas here; see Pullum and Wilson 1977 and Postal and Pullum 1978 for some presentation of the alternatives.

since lack of sentence stress is often supposed to be a condition on cliticization. However, the approach of Selkirk 1972 assumes the opposite—that cliticization leads to destressing¹²⁾—and there is much to recommend this idea within the framework I have been developing here, since readjustment rules also precede the sentence prosody component. I will conclude, tentatively, that a readjustment/cliticization component follows syntax and precedes sentence prosody and all of traditional morphology.

Summary

The framework of description sketched here is one with at least ten modules, strictly ordered: relational rules, syntax, readjustment/cliticization, sentence prosody, free deletion, word formation, allomorphy, morphophonemics, surface filters, and phonology. Further modules might need to be added to this list (a shallow structure filter component, between the relational rules and syntax proper, is a prime candidate). In any case, at least seven lie in the syntax-phonology interface, in my view.

Various aspects of this proposal are problematic. For instance: With so many components available in the general theory, it is not easy to assign a particular rule to one component or another, and the way is open to saving the framework by *stipulating* that such-and-such a rule belongs to such-and-such a component. Abuses of this sort could be avoided if we had substantive proposals about the form and internal logic of each component.

In addition, certain components seem clearly not to stand in a purely linear relationship to the others. Word formation rules, for instance, must be sensitive in some cases to the phonological properties of constituent morphemes; e.g. the English *-en* inchoative/causative suffix (*blacken*, *brighten*, *madden*, *loosen*) may attach only to a monosyllabic base ending in a single obstruent (**afraiden*, **highen*, **laxen*).¹³⁾ This despite the fact that word formation rules must obviously feed allomorphy and morphophonemic rules. It could be, of course, that the surface filters component covers morphological as well as syntactic well-formedness. It could also be that the word formation component has access to several other components (including of course semantics). The sentence prosody component is in a similar state, since it sometimes appears to need access to the input of deletion rules (English VP Deletion and Gapping, for instance; see Bing's discussion), or—in a somewhat different theoretical framework—to the output of semantic rules interpreting null anaphors. I see these problems as real, but not necessarily fatal for the approach I have sketched: the relationships among components might be more complex than I have assumed, but still be consistent with autonomy and limited interfacing; and certain relationships might involve more than limited interfacing (though here I would hope that which relationships are exceptional and the way in which they are exceptional could be specified universally and are not a matter of language-particular

12) *Ceteris paribus*: see Wanner ms. 1978 for some complicating factors.

13) See Jespersen 1942: 351–9 for a discussion of the constraints.

option).

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Phonetics and Phonology in the Eighties: Prospects and Problems

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It is ten years since the Congress of Linguists devoted a Plenary Session to Phonology. At the Eleventh Congress in Florence in 1972 Morris Halle addressed a Plenary Session on Generative Phonology on the subject of "Morphology in a Generative Grammar". At the last Congress, in Vienna in 1977, there was no Plenary Session devoted to phonology, let alone phonetics. That the present Congress should convene a Plenary Session on Phonetics and Phonology is no doubt to some degree a reflection of the long-established and abiding interest of Japanese scholars in phonetics and of their leading role in some of the advanced technological research and experimentation currently taking place in the speech sciences. To some degree also, however, the choice of topic reflects the concern felt by linguistic phoneticians at the widening gap—not yet a gulf—between the fruits of such research and the traditional assumptions upon which phonologists rely so heavily.

The omission of phonology from the plenary sessions selected for Vienna cannot be ascribed to a lack of interest in the subject by linguists in general, since there was a great proliferation of publications on phonology throughout the 70's. A great deal, some might say an excessive amount, of attention was paid, particularly in the first half of the decade, to what one might term "rules about rules",—to the many different types of rules, their status, their application, their ordering, re-ordering, "local ordering", and indeed absence of ordering. The positive result of all this has been that phonological descriptions have been presented with (in Stephen Anderson's words) "a formal precision quite unprecedented in discussions of phonological structures" (Anderson 1979a: p. 3). On the negative side it is to be remarked that this precise and elegant formalization has almost always been of data already described in traditional terms, and that while it may present a more satisfactory description, it does not of itself add anything new to what was known before; still less does it offer an explanation of what is described. As the decade wore on, criticisms of alleged inadequacies or gaps in the *Sound Pattern of English* (SPE) model, which had, it should be remembered, been put forward modestly as "an interim report on work in progress" (Chomsky and Halle 1968: p. vii), gave rise to a number of more or less radically revised models, so that we have seen the birth of natural phonology, natural generative phonology, atomic phonology, autosegmental phonology, upside-down phonology, metrical phonology, and more besides. Some of these new approaches continue to be largely concerned with

the type and application of rules; others are more concerned with the very nature of the items upon and with which rules are supposed to operate, and it is these latter which seem to foreshadow what are likely to be the major preoccupations of the linguistic phoneticians and phonologists in the 80's.

Common themes discernible in these pointers to the 80's include the perennial one of the relationship between phonetics and phonology, and speculation about the nature and range of phonological entities, with the concepts of "segments" and "features" coming under special scrutiny. These are manifestly not new topics as such but they acquire a new perspective in the light of the work of the immediately post-SPE period. Many linguists have perhaps been inclined to the comfortable belief that the old problem of the relations of phonetics and phonology, so often a source of debate in the past, had been more or less satisfactorily resolved. The "unique contribution" of the standard theory of generative phonology (SGP) as set out in SPE, which has without any doubt been the dominant influence of the last ten years, has recently been described as its "concentration on making explicit the principles governing the association of phonological with phonetic representation, conceived as a system of rules or algorithm for converting one into the other in a series of steps" (Anderson 1979a: p. 3). SPE transmuted the original strictly limited set of purely phonological features of Jakobson and his collaborators into a putative universal set of phonetic features with both a classificatory and a phonetic function. The fact that the universal phonetic features are also nominally at least the same as the features used for classificatory purposes, appears to offer a welcome and easy bridge between phonetics and phonology. However, the result has sometimes been to blur rather than to clarify the relations between the two. Despite SPE's critical comment upon some linguists' "failure to differentiate sharply between abstract phonological features and concrete phonetic scales" (p. 297, fn. 5), very few phonological studies in the SGP framework have ever attempted to follow an investigation through from the classificatory to the scalar stage: rules are presented in the form $A \rightarrow B/X-Y$, in which both A and B are features or matrices of features with plus or minus values, and that is usually as close to the phonetics as we are allowed to get. The impression given is that the linguists concerned feel that that is as close to the phonetics as we *need* to get; such a view-point, if this is indeed what underlies such presentations, must give rise to uneasiness among those of us who regard ourselves as linguistic phoneticians, since it leaves so much of the phonetic substance unaccounted for. To take a simple example, a satisfactory system of conversion from phonological to phonetic representation in English and French should be able to indicate that French [i] is consistently higher than English [i]. SGP theory provides for this by the concept of "phonetic scale", but it is noteworthy that no one has found it worthwhile to pursue the conversion process thus far despite Postal's *claim* that phonetic representations "must be interpreted as the instruction which indicate the way the physical system of articulation is to perform" (Postal 1968). Speaking of the structural linguists, Chomsky and Halle say in SPE (p. 293,

fn. 1): "As an illustration of this lack of interest [by structural linguists] in phonetics we may cite the numerous articles on phonological subjects that have appeared in the last thirty years... in which information concerning the phonetic properties of the phonemes of a language is often restricted to a simple listing of alphabetic symbols." In many articles of the past fifteen years this state of affairs has only changed to the extent that the restriction has been to a simple listing of plus or minus values for features from Chapter Seven in SPE.¹⁾

While we may agree that linguists are "primarily concerned with the structure of language rather than the acoustics and physiology of speech" (SPE p. 293), features specified in terms of the articulatory processes assumed to be involved in their production cannot escape being evaluated in the light of advancing knowledge about speech mechanisms in general. The Proceedings of the Ninth Phonetics Congress in Copenhagen in 1979 in some respects make daunting reading for linguists who are not participants in the technological revolution at present taking place in the field of speech science. The battery of new laboratory techniques will probably generate a fair amount of information which the linguist may feel is not directly pertinent to his own sphere of interest, but the increased understanding of the processing of speech that they are providing is bound to affect the credibility of phonological concepts that are at present framed in concrete or quasi-concrete terms of one sort or another. On the positive side, the new knowledge should also advance the search for explanations of the sound patterns of languages. This explanatory aspect of the phonologist's task has hitherto lagged behind the progress made in the formalization of phonological statements. The task of finding phonetic explanations of phonological patterns and processes, both synchronic and diachronic, is at last attracting the attention it deserves (see, for example, Ohala 1974, 1975).²⁾ It may turn out that this line of research will ultimately dispose satisfactorily of the problems that gave rise to the notion of markedness as expounded in SPE—a notion which, as has been pointed out (Anderson 1979, p. 136), has not played the fundamental role in subsequent analyses of phonological phenomena that might have been expected in view of the initial enthusiasm with which it was greeted.

Inextricably linked with the issue of the phonetics/phonology relationship is that of the nature of the units and processes with which we should be dealing. If the overriding aim is elegance and economy of presentation, there is much to be said for a single invariant abstract "underlying form" from which both phonetic realisations and morphological variants can be derived by a series of neatly ordered linear rules. Many phonologists, however, have not been satisfied with what amounts to a kind of notational adequacy, no matter how elegant,

1) The wide interest displayed at this Congress in phonetic research and its implications for phonology suggests that prospects for the next few years are brighter in this respect.

2) See also Dinnsen (1980).

but have been concerned that their concepts and constructs should also have some claim to "psychological reality" (whatever meaning they may attach to this term) or to some fairly direct relationship with the mechanisms of speech production and reception. The two aims are, however, almost certainly incompatible. There seems no reason whatever to suppose that our psychological and physiological processes operate in a way that closely matches the tidy, economical formalizations that we set to paper. What evidence there is seems to point the other way: speech of its very nature, and to good purpose, abounds in "redundancies", notwithstanding our eagerness to avoid them in our notation, and to exclude them from serious theoretical consideration.

At the present time there appears to be a swing away from the highly abstract constructs proposed in earlier studies towards phonological forms which are felt to be closer to the phonetic surface forms. Two movements which immediately spring to mind here are those of Natural Phonology (NP) and Natural Generative Phonology (NGP). Whilst the latter of these two is avowedly a modification of the SGP approach, NP reaches back to such traditional phoneticians and linguists as Sweet, Sievers, Passy and Sapir for its forerunners. Both NGP and NP share the basic thesis that "phonological systems are phonetically motivated" (cp. Donegan and Stampe 1979: p. 169). Where NGP operates in terms of rules, Stampe's NP is principally concerned with natural phonetic processes. NP's starting point is the universal system of processes that reflect the phonetic limitations of the infant (Donegan and Stampe 1979: p. 126). It is claimed that as infants we gradually learn to constrain those natural processes that do not apply to the adult language around us. The devoicing of final obstruents, for example, is held to be a universal natural process rather than a language-specific one; we do not have to *learn* to devoice final obstruents, but depending upon our native language we may have to learn *not* to do so in some instances. A distinction is drawn between *processes*, which apply involuntarily and unconsciously, and *rules*, which have to be learnt. Hooper (1979: p. 106) declares that "the major claim of natural generative phonology is that speakers construct only generalisations that are surface-true and transparent." "Phonological forms of underlying representations are closely related to surface phonetic forms" (Hooper 1976: p. 111), and should be based on the most careful style of naturally occurring spoken forms. This recalls the "phonetic plans" discussed by Linell (1979, 1980). NGP posits two distinct types of rules: P-rules, whose statement contains only phonetic information, and which are automatic, i.e. there are no exceptions; and MP-rules whose statement requires some reference to morphological, syntactic, and lexical features. The fronted [k] of *key* is the result of a P-rule, whereas the [k~s] alternation in *electric*, *electricity*, is the result of an MP-rule. MP-rules frequently have exceptions. An interesting further distinction between the two types is that the variation produced by a P-rule is a "phonetic continuum", whereas variation produced by an MP-rule is not a continuum, e.g. in Hooper's pronunciation of a word like NATO, a P-rule may produce "a whole range of

pronunciations from a full [t^h] to a weak flap."³) There is no such phonetic continuum between the variant forms *monologist* (with [g]) or *monologist* (with [d₃]) that might arise from the application of an MP-rule. The "continuum" as opposed to the quantal approach to the output of phonological rules could confer greater phonetic credibility on some of the standard treatments of assimilation such as that in *ten pence*, in which the quantal approach forces us to posit $n \rightarrow m$, ignoring all the intervening possible degrees of coarticulation and overlapping that may occur.

Despite their overt concern with the phonetic foundations of phonology and with the search for phonetic explanations of phonological processes, both NP and NGP appear content on the whole to operate with segments, and to a lesser extent with features, as commonly conceived. NGP, however, departs from orthodox SGP practice by firmly taking the syllable as "the basic phonological unit" and devotes a great deal of attention to strength hierarchies. This revival of interest in strength hierarchies is shared by other phonologists such as Foley (1977) and Drachmann (1976).

Are there more radical changes that a closer link with phonetics might bring about in our conception of the most appropriate phonological units? What have the experimentalists to offer here? Experts in the speech sciences admit that they are still relatively uninformed about some aspects of the basic speech mechanisms. Current research in speech production as reported by MacNeilage suggests that instead of thinking in terms of (some) invariant context-independent motor commands for particular sounds we need to be thinking in terms of interacting context-dependent spatial and auditory targets. Speakers required to produce vowels with bite-blocks up to 25 mm. in size inserted between their teeth managed to achieve the correct formant frequencies virtually immediately. X-rays showed that despite the presence of the bite-block a close approximation to normal vocal tract shapes was achieved, thus suggesting some invariant spatial target. (MacNeilage 1979: pp. 18-19, reporting on Lindblom and Sundberg 1971, Lindblom, Lubker and Gay 1978). However, in other experiments which involved interfering with the raising of the lower jaw for the closure of bilabial stops, speakers compensated for this by lowering the upper lip, thus suggesting in this case a target in terms not of absolute space but perhaps one in terms of such factors as "articulatory contact, or intraoral pressure" (Folkins and Abbs (1975), reported by MacNeilage). Convincing evidence has also been produced in other instances for auditory targets. These are, after all, the source of the child's acquisition of the spoken language of his community. It has been shown that there may be—and often are—different spatial ways of achieving the "same" auditory target. It has been reported for example that different motor-control gestures of vocal fold abduction and adduction may be used in

3) Note that there are problems here with British English, in which the intervocalic stop rarely, if ever, reaches the flap stage. Indeed, in many varieties of British English a glottal stop is used medially in such forms, which runs counter to description in terms of an articulatory continuum.

English for a single intervocalic [p] and for the [p] in English *upbringing* respectively. This has been interpreted as showing that in this case it is the auditory goal that remains invariant "at the expense of invariance in spatial configuration" (MacNeilage 1979: p. 19). A similar conclusion was reached in experiments which showed that if rounding of the lips is mechanically prevented, the auditory effect of "rounded" vowels can be produced by lowering the larynx to achieve the required lengthening of the vocal tract. (Riordan, 1977.) The relationship is thus not one-to-one but many-to-one.

Where does this leave the phonologist? First, it should be said that, to adapt a remark of Ladefoged's (Ladefoged, 1977: p. 409), it does not mean that we must assume that the units and concepts used by speech scientists are necessarily those most appropriate for use by linguists. What it *does* seem to mean is that whatever relationships are to be found between phonetic and phonological entities should be expected to be many-to-one rather than one-to-one. It is expressly stated in SPE that phonological representations are not necessarily submatrices of phonetic representations. "We do not, in other words, impose the conditions of linearity and invariance on the relation between phonological and phonetic representation. The indirectness of this relationship must be purchased at the cost of adding rules to the grammar." (SPE, p. 297). Linguists in eager pursuit of simplicity and economy have perhaps sometimes lost sight of this. Part of the "cost" may in the present decade turn out to be the need for a thorough re-thinking of the whole feature framework. Even allowing for the fuller exploitation of the possibility of multi-valued features, the present framework cannot for example, as Ladefoged has pointed out, distinguish between the velar ejectives in Hausa and Navajo, or the glottalized plosives in Kalabari and Hausa respectively, although the difference between the sounds in the two languages is clearly audible in both instances. Of these and other similar cases, Ladefoged says: "There is no doubt that speakers can make, and listeners can hear, at least some of these differences with complete reliability. Therefore this degree of phonetic detail must be included in linguistic phonetic descriptions of languages." (Ladefoged 1980: p. 501).

Various attempts have been made over the years to revise or augment the set of features proposed in SPE, but patching up is no longer good enough. The time has come for a thorough overhaul, and perhaps for a completely new approach. There is no doubt in some minds that useful insights were lost when the Jakobson Distinctive Features, with their two-fold articulatory and acoustic specifications, were replaced in SPE by features specified in articulatory terms only. Basbøll has recently approved suggestions that a hybrid solution may be needed, since the phonetic correlates of some features (e.g. "grave") are acoustically simple but articulatorily complex, while others such as "labial" and "nasal", are the reverse, (Basbøll 1979: p. 122). He claims that this approach is in harmony with the original Jakobson position that "the features are above all perceptual", and it is to be remarked that in Jakobson's recent statement of his own position he maintains his earlier view of the primacy of perception

(Jakobson and Waugh 1979). Ladefoged, who has striven as hard or harder over the years than anyone else to reconcile an SPE-type feature inventory with phonetic "reality", saw early the need to distinguish clearly between phonetic and phonological features without expecting a one-to-one relationship between the two. He suggested at one stage (Ladefoged and Vennemann 1973) that there were "prime features" which were definable in terms of the physiological and acoustic properties of sounds, and "phonological features", some of which would be "cover features", involving certain values of the prime features. "Consonantal" was cited as an example of a cover feature, since there is no single measurable property which would distinguish all consonantal segments from non-consonantal ones; "Consonantal" has to be defined in terms of particular combinations of values of prime features. Similarly, [+labial] would be a cover feature, defined in terms of the prime features [+bilabial] or [+labiodental] or [+round]. (Ladefoged and Vennemann 1973; Ladefoged 1972). Hooper has also proposed a cover feature "Strength" in her handling of Strength hierarchies (Hooper 1976: p. 205-7).

Ladefoged has recently moved in the direction of even sharper division between phonetic and phonological features (Ladefoged 1980). He now claims that adequate descriptions of the phonetic aspects of languages require about 17 articulatory parameters and about the same number of acoustic parameters. These 2 sets of parameters which are interconvertible, "are not things like features, but rather things like formant frequencies of parameterized vocal-tract shapes." In addition to these phonetic parameters we still require a set of phonological features to describe the sound patterns of languages. We must be able to map these features onto the basic phonetic parameters but we should not expect there to be a one-to-one relationship between the two sets; "the majority of phonological features are in a many-to-one relationship with the minimal set of acoustic parameters, just as they are with the minimal set of articulatory parameters" (Ladefoged 1980: p. 494).

Feature phonology of whatever kind is on the whole based upon traditional notions of linear letter-sized segments with static properties—upon what one may call the "ABC syndrome"—but the one-tier uni-linear string imposed upon the phonological component of transformational generative grammar is also under challenge from several quarters. Acoustic phoneticians and those working in speech perception have continued to take syllables for granted as essential elements in their own field, and Studdert-Kennedy in his report on Speech Perception to the Phonetics Congress declares that "the primary unit of perception is evidently the unsegmented syllable" (Studdert-Kennedy 1979: p. 72). Hierarchies of higher level constructs such as syllables, tone groups, phonological phrases etc. have never been excluded from phonological paradigms outside the GP tradition, and are now beginning to be revived within it in order to handle prosodic and rhythmic phenomena which resist convincing treatment in purely segmental terms. (See, in particular, Lehiste's account of important recent work in this field in her contribution to this Session.)

Autosegmental Phonology (AP) which is regarded by its proponents as a revised version of SGP rather than as a completely new departure, does not seek to set up higher levels above segments but rather to propose parallel strings of segments arranged in two or more tiers. Goldsmith rejects the term "suprasegmental" as a misleading label from his point of view, since pitch, for example, itself forms a sequence of tonal segments. He thus prefers the picture of parallel strings of segments, none of which "depend" or "ride upon" the others, each being independent in its own right (Goldsmith 1976: p. 21). Features especially suited to autosegmental treatment include tone, intonation, vowel harmony and, in special cases, nasality. AP appears to have been triggered off by difficulties encountered in the SGP framework in dealing with tone, especially with contour tones on short vowel syllables, and with such phenomena as tone-spreading. Contour tones are regarded as sequences of tone features labelled High and Low, and since a single vowel segment cannot, in Goldsmith's view, be marked both +High and +Low, a problem arises as to the appropriate treatment. The autosegmental solution is to assign the tones to a separate tier, having an association, not necessarily one-to-one, with the tier of consonant and vowel segments. Thus CVCV implies a disyllable with the tonal pattern

$$\begin{array}{cc} \text{H} & \text{L} \\ | & | \end{array}$$

[- -], while the addition of an association line, viz. CVCV, implies one with the pattern [-\].

$$\begin{array}{cc} \text{H} & \text{L} \\ | & | \\ \hline \end{array}$$

The theory allows for the possibility that some segments may not be "marked for tone" in the lexicon, but may in utterance "borrow" a tone from neighbouring syllables. This technique has been applied to a wide range of tonal phenomena from African languages, to the pitch of Japanese, and to English intonation. There is also a persuasive application of AP to vowel harmony in Clements (1980). The AP approach to tone appears to work quite well for the African languages dealt with so far, but one would like to see attention extended to some of the more complex tone languages of East and South East Asia (not confined simply to such well-worked samples as Standard Thai, Mandarin and Cantonese), in which there may be up to five different level tones and several different kinds of rises or falls. If the output of phonological rules is to be capable of presenting something approaching the consistent and regular pronunciation of native speakers, the static tone features so far proposed are quite inadequate to the task. It will not have escaped notice that the features that lend themselves to autosegmental treatment are also those found particularly suitable for treatment in terms of Firthian prosodic phonology. There is however a crucial difference between the two: despite its innovations, AP is still, as its name proclaims, firmly segmental, and equally firmly "static" in outlook. The problem with contour tones arises directly out of the assumption that these must be viewed as a sequence of static features, High and Low. It is not at all clear why we should not have dynamic features, Falling and Rising. (For a discussion of the arguments for and against, see Anderson 1978.) The same

stops in Guarani, in which the prenasalisation is handled as a sequence of the (static) features Nasal and Oral (= - Nasal) in their own autosegmental tier, viz, C V.

N O

A further symptom of the ABC syndrome is that strings must be read from left to right, which means that, counterintuitively, the rule formulated above has to be labelled "Postoralisation Rule". This brings us to another issue which has been exercising both phoneticians and some phonologists recently, namely the role of the temporal aspects of spoken language which have hitherto, despite lip-service to the "continuum" of speech, been largely neglected in phonological theory.

Fowler (1980) complains about "extrinsic timing theories of speech production" which assume that the time dimension is excluded from the specification of phonological segments in the articulatory plan for utterance, and calls for an *intrinsic* timing theory. This is supported by MacNeilage's suggestion that in the production of diphthongs there must be some "specification of dynamic properties" underlying their production as contrasted with the static spatial or auditory targets aimed at in other cases. (MacNeilage 1979: p. 17-18.) It may be argued that the time dimension in the production of aspirated and un-aspirated stops has been recognized in the concept of Voice Onset Time. A similar approach to other phenomena might prove interesting. Why, for example, should prenasalisation, and indeed vowel nasalisation also, not be regarded in terms of Velic Raising Time, or some such concept? Contour tones are also candidates for treatment in temporal terms, if the right formulation can be found. Coates has however warned against "the tendency towards equivocation on the nature and status of time in phonological representations". Features such as [\pm long], [\pm delayed release], [\pm continuant] etc. are "temporal in reference without the implications of this assumption being developed further". He makes the point that time in the sense of inherent segment time properties is ignored in current phonological theory, despite the invocation of "tempo" and of temporal sounding feature labels. (Coates 1980)

In conclusion, brief mention may be made of Griffen's highly idiosyncratic but interesting Dynamic Non-segmental Phonology, which attempts to accommodate within a single model several of the issues raised above. The model is claimed to be dynamic, following recent proposals, notably those of Mermelstein, in physiological phonetics; segments are rejected as mere convenient fictions devised by people shackled by historical chance to an alphabetic writing system. Claiming to pursue the ideas of Firth to their logical conclusion, Griffen proposes a three-fold hierarchy with vocalic features forming the syllabic base, "realised in parallel dynamically" with laryngeal features and with "obstruction" or consonantal features which are constraints upon the base. It is claimed that since "alleged variants . . . can be shown to be simply coarticulated features in the syllabic environment", the concept of allophony is eliminated,

and the "structural distinction between phonology and phonetics which has been at the very core of all phonological models of all linguistic theories, suddenly disappears" (Griffen, forthcoming). Strong words indeed, which will arouse equally strong assent or dissent among other linguists!

We can certainly look forward to continuing vigorous debate in the 80's on old as well as new problems. This is to be welcomed since, as Goldsmith has remarked: "Problems are the stuff of which advances are made".

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The Role of Prosody in the Internal Structuring of a Sentence

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A spoken sentence contains a linguistic message in phonetically encoded form. Normally the sentence consists of more than one word. Words constitute strings of segmental sounds (vowels and consonants), tied together according to the rules of word-level phonology; their lexical meaning can ordinarily be extracted from the phonetic realization of a sentence on the basis of their spectral structure. Prosody certainly plays a part in word-level phonology; but what I want to discuss in this paper is the role of prosody in the internal structuring of a sentence. Thus I am concerned with the ways in which words enter into higher-level units, and with the characteristics of such units. These units may be defined in various ways. A syntactic analysis of the sentence establishes its syntactic structure. There is a hierarchical relationship between syntactic units, but when the sentence is spoken, the syntactic units are produced in linear sequence. The relationship between the units must nevertheless be recoverable from the phonetic properties of the spoken string of words; this is a necessary prerequisite for understanding the meaning of the sentence.

The spoken sentence also has a number of characteristics that may be described without reference to its syntactic structure or to the segmental identity of the sounds that make up the words of which it consists. We may observe a succession of peaks of prominence and the rhythmic structure imposed on the sentence by their alternation. We can describe the fundamental frequency at which the sentence is spoken and the resultant intonation contour. The changes in pitch and relative prominence take place in time; the sentence thus has an overall temporal structure. All these prosodic characteristics may interact with each other in various ways, and they may also be involved in manifesting the syntactic structure of the sentence. It is the purpose of the present paper to review and discuss some of the recent research concerning the relationship between subparts of a sentence that are defined syntactically on the one hand, and phonetically/phonologically on the other hand.

I have pursued this line of research in a series of experimental studies carried on over a number of years; my results are summarized in Lehiste (1977) and Lehiste (1980). These articles also contain references to many other publications which for lack of space cannot be discussed in the current paper. Briefly, my position is as follows:

In English, the syntactic structure of a sentence is expressed phonetically primarily through the controlled timing of articulatory gestures that speakers

use when they produce spoken utterances. Speech is a rhythmic activity, as are most motor activities performed by human beings. In English, the controlled timing of articulatory gestures takes the rhythmic structure of speech into account. Stressed syllables carry the greatest amount of information; therefore, attention has to be focused on the stressed syllables, and this is facilitated by setting up an expectation as to when the next stressed syllable is likely to occur. Producing sentences in such a way that stressed syllables occur at regular, isochronous intervals contributes to optimal perception by the listeners whose attention is cyclically directed to the points in time at which the stressed syllables can be expected to be found. (Martin 1972; Cutler and Darwin 1981). Furthermore, a disruption of the expected pattern—lengthening of an interstress interval—can be used to convey crucial information about syntactic structure, namely the placement of a syntactic boundary. The syntactic structure of an English sentence is thus primarily manifested in the timing pattern of that sentence when produced orally by a native speaker of the language.

The rhythmic structure with which I operate is basically the Abercrombian metric foot. According to Abercrombie (1964), spoken utterances are structured into isochronous metric feet consisting of a stressed syllable and successive unstressed syllables (if any), up to the next stressed syllable. The stressed syllables follow each other at isochronous intervals. In my 1977 article I reviewed most of the evidence for and against the existence of isochrony in English that was available to me up to that time; in the present paper, I shall consider some of the more recent studies devoted to this topic, which apparently continues to attract the interest of a number of scholars.

Scott (1980, 1982) focused her study on the perception of phrase boundaries. It is well known that phrase boundaries are often accompanied by phrase-final lengthening. I had claimed in 1977 that listeners judge a boundary to be present on the basis of the lengthening of the interstress interval in which the boundary occurs; this lengthening can be brought about by phrase-final lengthening, or pause, or a combination of the two. Scott labels this the Foot Hypothesis. A simpler hypothesis, labeled the Phrase-Final Lengthening Hypothesis, suggests that listeners use the duration of the lengthened phrase-final stressed syllable directly (together with any subsequent pause), perhaps on the basis of comparing it with some ideal duration that would be expected if no phrase boundary were present. Scott associates this position primarily with Klatt (1975). She notes, however, that Klatt himself has pointed out a circularity about the use of timing information both in establishing the identity of the vowel and in determining whether phrase-final lengthening has taken place. The perceived duration of a vowel depends on several factors, such as the number of preceding syllables within the phrase and the word, presence or absence of stress, the tempo of the utterance in which it is produced, and its inherent duration. Listeners have to take all these variables into account when they decide whether the duration of the vowel is "normal" or not. All these factors affect the identification of the vowels, but it is only after the vowel has been identified that one can refer

to its expected duration in judging whether it has been lengthened or not.

Scott ran a series of experiments testing the two hypotheses. She used syntactically ambiguous sentences of the type *Joe or Patricia and Steve will go* (modeled on my sentence *Steve or Sam and Bob will come*). The sentences were read by a male speaker of Southern British English, who was provided with structural bracketings of the sentences and asked to convey the expected meaning by whatever means he deemed appropriate. The tape-recorded sentences were analyzed in terms of pitch, rms intensity, and twelve Linear Predictive Coding parameters representing the spectral characteristics of speech. A resynthesis program reproduced the waveform, unchanged in other respects except for pitch, which was set to a constant fundamental frequency value of 133.5 Hz. Measurements were made of the duration of each foot, each syllable within the feet, and of any pauses within them. The sentences were then temporally manipulated by adding pauses, sections of closure intervals, or pitch periods (of 7.5 msec each) to specified areas of the waveform. Listening tests employed various manipulations of the test sentences as well as distractor sentences, presented in random order. The results were submitted to statistical analysis.

The results of Scott's study (which cannot be reported here in detail) showed that listeners do indeed base their judgments of the position of the phrase boundary on the rhythm of the stress beats of the sentence and not on phrase-final lengthening *per se*. Listeners were shown to perceive a phrase boundary in a lengthened foot even when lengthening was distributed throughout the foot rather than confined to possible phrase-final stressed syllables or pauses. In responding to versions of test sentences that contained the same amount of phrase-final lengthening but different foot ratios, listeners interpreted the sentences differently; according to the Phrase-Final Lengthening Hypothesis, such sentences should receive the same interpretation. The Phrase-Final Lengthening Hypothesis was therefore rejected in favor of the Foot Hypothesis as an account of how listeners use temporal information to decide on the location of a phrase boundary in a sentence.

Scott's results thus support both the idea that the phonetic structuring of a spoken sentence into subunits is basically temporal, and the hypothesis that syntactic boundaries are manifested with reference to this temporal structure. Scott used sentences whose intonation contours had been transformed to monotone. Since sentences normally carry intonation, her results do not speak to the potential role of fundamental frequency in the internal structuring of a spoken sentence.

The relative importance of phrase-final lengthening and intonational cues in the location of syntactic boundaries in Dutch was one of the topics treated in a recent study by de Rooij (1979). De Rooij used a technique that employed nonsense imitations of meaningful utterances with specified syntactic structure. The imitations consisted of strings of /da:/ syllables, spoken by an experienced reader who marked the syntactic boundaries with a continuation rise or a non-

final fall, pitch movements that are expected, according to several previous studies, to signal prosodic boundaries in Dutch. The stimuli were edited by deleting and reassembling parts of the waveform; an intonator (essentially a channel vocoder) was used to provide the nonsense sequences with pitch contours.

The stimuli represented five categories. The baseline was provided by a sequence with no internal boundaries. This sequence consisted of seven syllables in which the first and last syllable carried greater length and a pitch contour designed to make these syllables to be perceived as stressed. The intervening unstressed syllables were all 90 msec long and were produced on a slowly and uniformly declining pitch. The presence of a phrase boundary marked by a continuation rise was simulated by a temporal marker (lengthening of the phrase-final syllable to 180 msec) and a pitch rise of four semitones on the lengthened syllable. The non-final fall consisted of a five-semitone fall following a syllable lengthened to 150 msec. The four stimulus categories prepared in addition to the first category (which contained no prosodic phrase-boundary markers) consisted of the following: temporal and pitch markers combined, temporal markers alone, pitch markers alone, and conflicting temporal and pitch markers. The task of the listeners was to assign each stimulus to one of nine Dutch sentences that had been typed out on cards. Six of these sentences contained one major syntactic boundary, each in a different position.

The results showed that a temporal marker was a far more powerful cue for signalling a prosodic boundary than a pitch marker. The perception of a prosodic boundary can be inferred from the syntactic boundary in the selected response sentence. Judging correctness of response in these terms, temporal markers combined with pitch markers produced an average of 88% correct responses; temporal markers alone, 85%; pitch markers alone, 33%; temporal markers conflicting with pitch markers, 85%; and pitch markers conflicting with temporal markers, 29.5%.

These results appear rather surprising in view of the fact that quite a few scholars (Dutch scholars among them) consider intonation to be the major determinant of the internal structuring of a spoken sentence. Elisabeth Selkirk (1981), for example, postulates a hierarchy of prosodic categories of which the highest unit below the utterance is the intonational phrase, defined as the unit with which the "primitive intonational contours" are associated. Selkirk's theory is essentially a revision of the theory presented in Liberman (1975) and Liberman and Prince (1977). Selkirk's main contribution is the addition of the notion of prosodic categories. Selkirk's prosodic hierarchy is based on segmentally defined strong and weak syllables, which in turn become constituents of prosodic feet, prosodic words, phonological phrases, and intonational phrases. (Note that the prosodic foot used by Liberman and Selkirk is not identical with the Abercrombian metric foot.) Selkirk argues further that there is no isomorphism between prosodic structure and syntactic structure, and a mapping must be defined between the two. (This view is very similar to mine.)

Selkirk assumes that the intonational contour of a whole utterance may be segmented into intonational phrases, which constitute the domain over which an intonational contour is manifested. The minimal intonational phrase is a single phonological phrase; the maximal intonational phrase is one that includes all the phonological phrases of a sentence. Any intermediate groupings are also possible. The intonational phrases are partly determined by syntactic factors: preposed adverbials, non-restrictive relative clauses, and parenthetical expressions must each correspond to an intonational phrase. But an intonational phrase does not necessarily correspond to a constituent of syntactic structure, and the boundaries of intonational phrases do not have to coincide with syntactic boundaries.

In Selkirk's presentation, the phonetic properties of intonational phrases are not very clearly defined. She quotes Breckenridge (1977) to substantiate her claim that intonational phrases constitute the domain of the declination effect: at the start of a new intonational phrase, the range is reset, the declination effect is observed across the phrase, and at the beginning of the next intonational phrase pitch is reset again. "Boundary tones" (Liberman 1975) are associated with the limits of the intonational phrase. Potential pauses are found between intonational phrases (and only between them), and pre-pausal lengthening is found at the end of intonational phrases. Selkirk quotes Klatt (1976) for the observation that pre-pausal lengthening is correlated with the location of possible pauses.

To support her claim that the intonational phrase is a rhythmic entity, Selkirk cites her observations concerning the flapping of /t/ in such sentences as *Go to the store, go today, give the money to Mary*. The flapping of /t/ in such contexts is evidence of "refooting": the weak syllable *to* has been associated from a following prosodic word or phonological phrase to the foot of the preceding prosodic word. If /t/ were initial, it would be aspirated. The result of "refooting" the weak syllable containing /t/ is that the /t/ may become part of the preceding syllable and thus be in the proper position for the flapping rule to apply.

Selkirk also hypothesizes that the units of prosodic structure postulated by her are the appropriate units in production and perception models.

Selkirk did not offer any direct phonetic evidence for her theory, but there exists at least one study which appears to support her claim that intonational phrases constitute rhythmic units (Darwin and Donovan, 1979, to appear). In this study, Darwin and Donovan investigated the relationship between isochrony and intonation. In the first of three experiments, the authors had listeners adjust the two intervals between three noise bursts until they matched the intervals between the /k/s in each of the following utterances: *The crew claim cargo*, *The crewmen will claim cargo*, and *The crew claiming the cargo*. The subjects were encouraged to pronounce the sentences to themselves while listening to the noise bursts; these productions by the subjects were likewise recorded. The matched durations turned out to be significantly more isochronous

than either the original or the subjects' own productions. In the second experiment, subjects listened to two sentences, one of which contained both a tone group boundary and a syntactic boundary in the middle foot. The subjects heard the second sentence as more isochronous than it actually was, but there was no such tendency in the sentence that contained a tone group boundary in the middle foot. Darwin and Donovan interpret this result to mean that the domain of perceptual isochrony is the tone group. These results were supported by the outcome of the third experiment, in which again the number of tone groups had a distinct effect on the perceived rhythm, but the syntactic structure did not. The authors conclude that within a tone group, the perceived rhythm is more regular rhythmically than the speech actually is. Between tone groups, this tendency toward perceptual isochrony is not found. Darwin and Donovan consider their results to be supportive of models of speech production which recognize an underlying rhythm in speech, in which the interval between stressed vowel onsets is more regular than in the surface form. They also see their results as supporting those linguistic theories that distinguish between prosodic and syntactic structure and recognize a relation between intonational and rhythmic units.

Darwin and Donovan equate Selkirk's intonational phrases with tone groups, but these two concepts are not necessarily identical. The term *tone group* was proposed by Halliday (1967) to refer to units of information, which, according to Halliday, are realized phonetically as tone groups. A tone group has a phonetically specifiable intonation contour which is organized around the tonic syllable (the syllable containing the greatest pitch movement within the tone group), which forms part of the focused word in the information unit. Every tone group contains one tonic, which may be preceded by a pre-tonic and followed by further words which will continue the direction of the contour initiated in the tonic. The tone group is normally isomorphic with the clause or simple sentence, but there may be more than one tone group in a sentence. In unmarked cases, the last lexical item in the tone group will be marked by the tonic as "new" information. In marked cases, the tonic appears on some item other than the last lexical item, and in this case any following lexical items are treated by the speaker as "given". In sentences out of context, the readers are expected to produce tonics on the last lexical item.

Halliday's theory of tone groups has recently been subjected to experimental testing (Brown, Currie, and Kenworthy, 1980; Currie, 1980; Currie, 1981). Crucial for Halliday's theory is the listeners' ability to recognize tonic syllables. I will review just one of the experiments reported by Currie (1981).

The material employed in this experiment consisted of cleft sentences and contrastive sentences, obtained in a game situation in which the first player was given a story to read, the second player received a sheet of paper containing a list of characters and a list of actions, and the second player was asked to match up the characters and actions and to attempt to reconstruct the story asking yes-or-no questions. This technique produced the expected kinds of

sentences, for example a cleft sentence such as *Was it the rich farmer who had three sons?* and a contrastive sentence such as *Well did the old man have three sons?* Twenty sentences were selected from three sets of games, representing three speakers. There were eleven examples of cleft sentences, seven non-cleft contrastive sentences, and two non-cleft non-contrastive sentences. Twenty-five judges, who were confident that they could easily identify tonics, were asked to identify the "tonic of the sentence". If they felt that there was more than one tonic, they were asked to rank them in order of importance.

The sentences were analyzed acoustically, and the syllables were identified which carried maximum pitch height, maximum pitch movement, and maximum intensity. Seventeen of the twenty sentences contained an element carrying all three phonetic maxima. All sentences with elements in lexical contrast had the cumulation of physical maxima realized on the contrasted element, and all contrasted elements were judged to be the tonics of their respective sentences by at least 23 out of 25 judges.

However, only five out of eight sentences that contained the cumulation of physical maxima on the clefted element had this element chosen as the tonic. Three of the five were such clefted sentences in which the clefted element was also an element in lexical contrast. In the other cases, the judges apparently followed different strategies. Some chose the rightmost lexical item as the tonic; others were evidently influenced by the cumulation of physical maxima; and still others seemed to choose the tonic according to clause structure. The judges were not always consistent in their choice of cues.

On the basis of a series of experiments (of which the one reviewed here is but a sample), Currie concludes that Halliday's position that the tonic is the realization of information focus is too strong. When the information focus is a contrasted element that contradicts a previous element, the information focus is consistently identified as tonic. When information focus is a "new" element realized as the leftmost lexical item, it is normally identified as tonic. But when the information focus is a "new" element realized as the rightmost lexical item in a sentence, more than one tonic is selected if the sentence is presented out of context: one tonic is chosen on the "given" element in the leftmost position, and one tonic is chosen on the "new" element in rightmost position. If judges are asked to identify tonics in utterances that do not have a clear "given-new" structure, several tonics are identified, associated with peaks of prominence realized on the stressed elements of the utterance.

In order to preserve the notion of one tonic per unit, Currie proposes an abstract unit called *tone unit* which contains only one stressed syllable and any following unstressed syllables. The tone unit will normally be coterminous with the Abercrombian foot; in fact Currie states that it may be unnecessary to differentiate between the two. Tone units may then combine to form tone groups. The tone group in its unmarked form will contain several peaks of prominence.

We have thus arrived back at the Abercrombian metric foot, which is defined

by isochronous stresses. The nature of this unit has also received intensive scrutiny in recent years (Morton, Marcus and Frankish, 1976; Fowler, 1977; Fowler, 1979; Tuller and Fowler, 1980; Tuller and Fowler, 1981). It appears nevertheless that some problems connected with isochrony remain to be solved.

Morton et al. (1976) reported that when sequences of digits are presented to listeners with temporally equidistant acoustic onsets, listeners do not perceive them as isochronous. When the listeners are allowed to adjust the intervals between successive digits, they introduce systematic departures from acoustic isochrony before judging the sentences to be isochronous. The authors suggest that listeners judge the timing of the word sequences on the basis of certain reference points within each word. These reference points, labeled "perceptual centers" or "P-centers", constitute the "psychological moment of occurrence" of a word. The critical variable affecting the temporal alignment of a digit with respect to its neighboring digits was the duration of its acoustic energy prior to the acoustic onset of its vowel: the longer the acoustic duration of the initial consonant, the longer the interval between the acoustic onset of the word and the location of its perceptual center.

Fowler (1979) found that when subjects are asked to produce isochronous sequences, they generate precisely the acoustic anisochronies that listeners require to hear a sequence as isochronous. This observation led to the suggestion that listeners judge isochrony on the basis of acoustic information about articulatory timing, rather than on some articulation-independent acoustic basis. In a subsequent study, Tuller and Fowler (1980) tested directly whether perceptually isochronous sequences have isochronous articulatory correlates. They used electromyography of the orbicularis oris muscle, while speakers were producing test sentences devised in such a manner that lip-muscle activity was related to the syllable-initial consonant, the stressed vowel, or the stressed vowel and final consonant. The results indicated that when subjects are asked to generate isochronous sequences, their muscular activity is indeed isochronous, regardless of whether the resultant acoustic signal is isochronous or not. The outcome of the experiment thus supported the interpretation of the perceptual phenomenon reported by Morton et al. (1976) to the effect that listeners judge isochrony with reference to the talker's articulations as they are reflected in the acoustic signal.

The question which is not completely solved is what properties of the acoustic signal convey this information about articulatory timing to the listeners. Morton et al. (1976) had found that the location of the P-centers was not equatable to the onset of the word, onset of the stressed vowel, or peak intensity; they hypothesized that the location of the P-center might be associated with peak increment in spectral energy in the first and second formants. Tuller and Fowler (1981) tested this hypothesis, using infinite peak-clipping to control changes in spectral energy. Infinitely peak-clipped syllables have their peak increment at syllable onset. If the perception of isochrony depends on the location of the peak increment, sequences of acoustically isochronous peak-

clipped syllables should sound more perceptually isochronous than sequences of the same syllables reproduced without peak-clipping. The results of this experiment showed that the subjects' perception of isochrony was unaffected by the infinite peak-clipping of syllables. Thus peak increment of spectral energy is not a perceptual correlate of the "psychological moment of occurrence" of the word.

Other investigators (Rapp, 1971; Allen, 1972) have identified what is probably the same reference point as the P-center, and have called it a "stress beat". None of these studies has discovered how a stress beat is marked acoustically.

At this writing, the final answer is not available. It appears indeed to be true that listeners perceive such sequences to be isochronous that have been produced with isochronous muscular activity, but we do not know how they are able to extract this information from the acoustic signal.

Most of the studies concerning the role of prosody in the internal structuring of sentences have been carried out with so-called stress-timed languages like English, Dutch, and Swedish. Time does not permit me to give full (re)consideration to the distinction between stress-timing and syllable-timing. Before attempting a final generalization, I would nevertheless like to review some work done with respect to at least one language usually considered to be syllable-timed. French is a good example, since French has been subjected to most of the same kinds of analyses as described above, and many phonetic, phonological and syntactic facts about French have been known for a long time.

According to one of the most recent analyses (Vaissière 1980), a French utterance consists of prosodic words, which normally extend from the end of a lexical word to the end of the next lexical word and comprise that lexical word together with any preceding grammatical words (function words). The left boundary of the prosodic word is marked by a fundamental frequency rise and by lengthening of the initial consonant of the first lexical item; its right boundary is marked by lengthening of the last sounded syllable (this excludes the so-called mute /e/), and by a continuation rise. When the prosodic word occurs at the end of an utterance, its fundamental frequency contour terminates in a final fall. The prosodic realization of a sentence depends to a considerable extent on speech tempo. When the sentence is pronounced slowly, each lexical word and associated function words is likely to be pronounced as a prosodic word. In rapid speech, several lexical words may be combined into one prosodic word, and several prosodic words may combine into a rhythmic group. The combining of two or more lexical words into one prosodic word is more likely to happen when the words are more closely associated syntactically, e.g. when they constitute an adjective-noun sequence. The greater the independence of two adjacent words, the greater the preboundary lengthening on the last sounded syllable of the preceding word. For example, a sentence *Nous pouvons vous proposer du café noir, du café au lait...* was realized as ((Nouspouvons vous proposer) (ducafénoir)) ((ducafé) (aulait)), in which the syllable *noir*,

terminating a rhythmic group, was more than 100% longer than the preceding syllable, while the syllables terminating single prosodic words were, on the average, 40% longer than the syllables that preceded them. This sentence also illustrates the fact that the rhythmic structure of a French sentence is not necessarily dictated by its syntactic structure, although the grouping of prosodic words may serve to disambiguate syntactically ambiguous sentences such as *Jean ou Pierre et Simon sont venus*.

Even this brief look at the prosodic structuring of a French utterance shows a number of parallels with the structuring of English utterances discussed in the main body of the paper. There are also important differences, concerning primarily differences in the timing of comparable prosodic attributes. Recall, for example, that the Abercrombian metric foot extends from the beginning of a stressed syllable to the onset of the next; the French prosodic word extends from the end of a lexical word to the end of the next lexical word (which sounds to non-French ears as exactly the opposite stress placement: the Abercrombian metric foot begins with a stressed syllable, the French prosodic word appears to end with one). Vaissière (1980, to appear) has made a beginning in the search for language-independent prosodic features, but not much progress can be realistically expected until descriptions of the prosodic structures of many more languages become available.

The topic of this paper has been the role of prosody in the internal structuring of a sentence. I have concentrated on a level immediately below the sentence—the metric foot, the intonational phrase, the tone group, the prosodic word. Each of these units has some prosodic characteristics. In defining the essential nature of such units, different linguists have attributed primacy to stress, or to intonation, or to timing; some linguists have considered the prosodic structure of a sentence to be basically independent of syntactic structure, others have derived the phonetic realization of a sentence directly from its syntax. There is evidently room for different opinions and much further experimentation. It is my personal opinion that rhythmic structure is basically independent of syntax, but interacts with it in various ways; and that in evaluating the relative contribution of different prosodic features to the internal structuring of a spoken sentence, the time dimension will be found to have primary importance. I base this opinion on at least three facts: That experimental studies reviewed above have shown temporal cues to be much more powerful in signalling the presence of unit boundaries than pitch cues (e.g. de Rooij, 1979); on the fact that listeners are unable to recognize unambiguously the intonational markers on which tone group and intonation contour theories depend (Currie, 1981); and the fact that boundaries can be effectively recognized when the test sentences have been reduced to monotone (Scott, 1980, 1982). Furthermore, I have just completed an experiment (Lehiste 1982, in press) whose results reveal that speakers and listeners are just as successful in disambiguating syntactically ambiguous sentences in whispered speech as they are in phonated speech; in my materials, the speakers did not compensate for the

absence of intonational cues by exaggerating temporal cues. I hope that this paper will stimulate many other experiments.

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The Phonological End Justifies Any Means

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6/14/82

For well over a century linguists have declared that theirs is a largely autonomous discipline which, with the exceptions of a few excursions into such areas as history, physiology, or acoustics, owes little to other scholarly fields (cf. Whitney 1867:53). The subject matter of autonomous linguistics was said to be the patterns and the structure that exists in language and the method—there really has been only *one*—used to find this structure was the *comparative method* which is basically just a rigorous way of demonstrating relations between units of language. So defined, linguistics probably *was* an autonomous discipline and it got a lot of mileage out of its single method. Bloomfield's remark in 1922 still holds:

It was Grimm's merit . . . that by the strength of a method [the comparative method], he conquered for science a body of facts so vast that the generations since have worked well within the bounds he reached and scarcely ever gone beyond.

The comparative method when applied to a single language yielded the method of *internal reconstruction* and this technique is the basis for the structuralist phonologists' criterion of complementary distribution used in finding phonemes and the generative phonologists' positing of common "underlying" phonological forms for related morphemes.

The autonomy of linguistics and the use of the comparative method were suited to the task of describing and cataloguing the units and patterns in language. But in the 20th century linguists have started to ask *why* these units and patterns exist, i.e., they seek explanations for, not just descriptions of linguistic structure. We have reached the point where the continued re-working of the patterns in language no longer gives us answers to our questions.

Accordingly, one of the most promising trends in modern linguistics has been the bridges built between linguistics and other disciplines: sociolinguistics, psycholinguistics, neurolinguistics, etc. The data and methods of these other disciplines have already greatly enriched our field and promise to continue to do so. In this paper I will discuss four problems in phonology and attempt to show, briefly, how their solution requires us to throw open the gates of our science to the data and methods from any source that proves useful.

Spontaneous Nasalization

In most cases, distinctively nasal vowels derive from sequences of *vowel+*

nasal consonant (or sometimes NC+V), e.g., French [vɑ] "wind" < Latin *ventus*, Hindi [dāt] "tooth" < Sanskrit *dant-*. On occasion, however, nasal vowels appear in words which never had a nasal consonant at any point in history, e.g., Hindi [sāp] "snake" < Sanskrit *sarpa*, [pəhāč] "attain" < Prakrit *pahuccai*. These are cases of so-called "spontaneous nasalization" which seems to have been systematically investigated first by Grierson (1922).

As it happens, in the majority of cases the spontaneously nasalized vowel appears adjacent to consonants characterized by heavy airflow: the glottal fricative [h], voiceless fricatives and affricates, and aspirated stops. In the case of [h] it is reasonable to suppose that since there is no aerodynamic requirement that the velum be raised during its production, it could be produced with a lowered velum and this state could be assimilated by adjacent vowels. (See Ohala 1975 for additional speculations.) This would not account for the involvement of oral obstruents, however, since they would definitely require an elevated velum.

Ohala and Amador (1981) (henceforth O & A) attempted to test a hypothesis (Ohala 1975, 1980) that vowels produced with a slightly *open glottis* might have acoustic characteristics which would mimic the effects of nasalization. A slightly open glottis allows some coupling of the subglottal cavity to the oral cavity (comparable to the coupling of the nasal cavity to the oral cavity during nasal sounds) and results in anti-resonances which, when they interact with the resonances of the oral cavity, increase the bandwidth and lower the amplitude of the first resonance (Fant 1973:8, Fujimura & Lindqvist 1971). Such effects coincide with some of the acoustic cues for nasalization on vowels. That vowel margins immediately adjacent to high airflow consonants would have a slightly open glottis has been shown by several glottographic, fibrescopic, and airflow studies (e.g., Sawashima 1969).

To see whether physiologically oral vowels might sound more nasalized on those parts abutting voiceless fricatives, vis-a-vis other oral environments, O & A used digital methods to create a series of steady-state vowels each 500 msec long by iterating single periods from the relevant parts of CVC(V) speech samples spoken by 4 adult males (2 American English and 2 Mexican Spanish speakers). Test vowels were made from the last or second-to-last period of vowels before the voiceless fricatives [s], [f], and, for Spanish, [x]; control vowels came from the last period before [n] (to make sure that the cues for true nasalization would survive the iteration process) and from periods before the oral consonants [d], [l], and, for Spanish, the trill [r], as well as from periods equidistant between the 2 C's in the CVC(V) utterances, that is, where the effect of the consonants was expected to be minimal (to make sure that the iteration process itself didn't introduce distortions that would mimic nasalization).

These vowels, with normalized amplitude (but not normalized pitch), were randomized and presented to 14 phonetically trained American English listeners who judged the degree of nasalization of each vowel on a 7-point scale, where "1" meant "completely oral" and "7" meant "heavily nasalized." In separate

recording sessions, velar elevation and oral airflow were sampled (using the nasograph and a pneumotachograph, respectively) as the same 4 speakers spoke the same words from which the iterated vowels were made.

As shown in Fig. 1, which presents representative data from one of the Spanish speakers, the physiological recordings revealed: (1) the expected lowering of the velum on vowels near nasals (see [bana]) but no detectable lowering during vowels next to oral consonants (see [bala] and [bafa]), (2) greater airflow—and by implication, greater glottal opening—during the latter part of vowels next to voiceless fricatives (see [bafa]). Some results of the perceptual study are represented at the top of the figure.

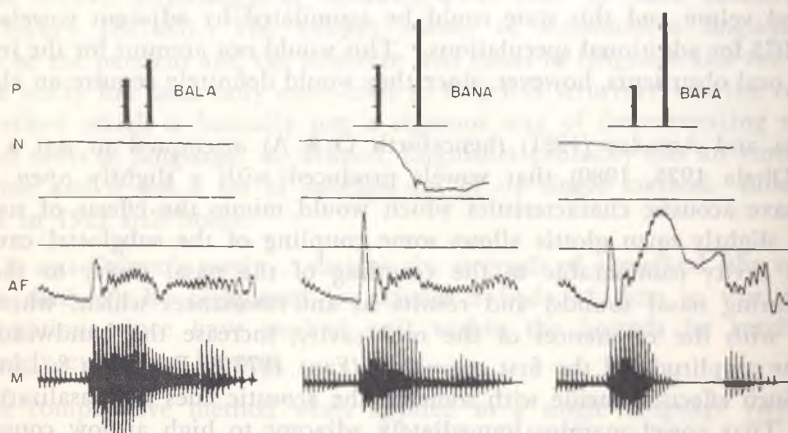


Fig. 1. P: Listeners' judgements of degree of nasality, on 7-pt. scale, of iterated vowel (left bar of each pair taken from period in middle of vowel; right bar from period at the end); N: Nasograph signal, where elevation of line is correlated with elevation of velum; AF: Oral airflow measured by pneumotachograph; M: Microphone signal. Utterances from a speaker of Mexican Spanish. Temporal synchronization of parameters is approximate.

The height of the vertical bars indicates the degree of perceived nasalization (tick marks at 1 unit intervals). The left bar of each pair corresponds to the iterated vowel made from the period excised from the middle of the uttered vowel; the right bar, that from the period just before the onset of C_2 . As expected, stimulus vowels made from the period in the middle and those taken from periods immediately before control oral consonants, e.g., [l], were judged to be relatively non-nasal. However, stimulus vowels from periods before nasals and those before voiceless fricatives were heard as being heavily nasalized even though, in the latter case, there was demonstrably no physiological nasalization. Such "spurious" nasalization was strongest on the vowel [a] but very weak on higher vowels. The vowel [a] may have enhanced the spurious nasalization since its pharyngeal constriction leads to acoustic coupling between the vocal tract and the glottal volume velocity waveform in the region of F1 and thus increased

bandwidth of F1 (K. N. Stevens, personal communication, Fant 1980a, b).

O & A concluded that the sound changes manifesting spontaneous nasalization came about when vowels that "sounded" nasalized, even though they weren't, were reinterpreted by listeners as having actual nasalization and were thereafter pronounced with nasalization.

Asymmetries in the Direction of Sound Change

It is a very old and, I think, quite correct notion that certain sound changes occur due to the sounds involved being acoustically and perceptually similar and thus confusable (Sweet 1874:15). Consider, for example, the following very common sound changes: $kw > p$ (e.g., Proto-Indo-European **ekwōs* > Greek *hippos* 'horse'), $pj > tʃ$ (e.g., Roman Italian [pjeno] > Genoese Italian [tʃena] 'full', and $ki > tʃi$ (e.g., *chicken* [tʃikən] < Anglo-Saxon *cocc* 'cock, rooster' + diminutive ending) (Ohala 1979). Various speech perception studies have found parallel confusions (e.g., Winitz, Scheib, & Reeds 1972). However, to say simply that two sounds, A and B, are confusable would imply that A should change into B as often as B changes into A. This is generally not the case, though. In the examples given above, the change is almost invariably in the direction presented, rarely in the reverse direction.

We might be tempted to offer an articulatory explanation for these asymmetries (e.g., "the preferred direction of change results in the physiologically simpler sound") except for the fact that these asymmetries also show up in the laboratory-derived confusion matrices from tasks where listeners just had to identify, not articulate, the sounds they heard. For example, Winitz *et al.*, in one of the conditions of their study, obtained the following confusions (where ">" means "reported as" and the percentage given indicates the percentage of time the sound was confused in the way indicated):

$[k] > [t] / [i]$ (32%) but	$[t] > [k] / [i]$ (6%)
$[p] > [t] / [i]$ (34%) but	$[t] > [p] / [i]$ (6%)
$[k] > [p] / [u]$ (27%) but	$[p] > [k] / [u]$ (16%)

One might explain these asymmetries by "response bias" in the cases where the more frequent confusion yields the more frequently occurring sound [t], but this would not account for the confusion $[k] > [p]$ since [k] is more frequent in English than [p] (Wang & Crawford 1960). Asymmetries in confusion must have something to do with the physical structure of the sounds themselves and how the human perceptual system processes them.

A clue to this problem may be found by examining the kind of confusions that occur in identification tasks involving stimuli presented to other sensory channels. Confusion matrices derived from identification tasks where the stimuli are the 26 capital letters of the Roman alphabet have been reported by Gilmore, Hersh, Caramazza, & Griffin (1979) for a visual presentation and by Craig (1979) for a vibrotactile (touch) display. Both show clear and similar

asymmetries. For example the following confusions were found more frequently than their reverse: $Q > O$, $E > F$, $R > P$, $B > P$, $P > F$, $J > I$, $W > V$. In all these pairs the first letter is structurally identical to the second *plus an extra feature*. If we assume that the major cause of these confusions is the incomplete perception of the ensemble of attributes that make up the letter, it follows that the failure to detect this extra differentiating feature will lead to the misidentification of the target letter as that letter which equals the target letter minus this feature. Adding (or hallucinating) an absent feature is less likely so the reverse confusion should have a lower probability of occurrence. As Garner (1978) has convincingly argued, such asymmetrical confusions should happen only between stimuli that differ by attributes which vary in an all-or-none way ("features" in his terminology), not by attributes which vary in a continuous way ("dimensions"). This principle should hold no matter what sensory channel is involved. Thus we should have a good chance to understand the asymmetry of the above-mentioned sound changes if we look for the "extra" feature which differentiates, say, [kw] from [p], etc.

Careful research is needed to identify these features but some preliminary speculations about them can probably be made. The sequence [kw] differs from [p] by the presence of a moderately sharp spectral peak in the low frequency region of the noise burst; otherwise they are largely identical, e.g., in F2 transition. The palatalized labials [bj, pj] have a brief rise in F2 following release which is lacking in plain apicals (Ohala 1979). Systematic manipulation of the acoustic waveforms of these sounds could confirm whether removal of these features leads to the confusions predicted.

Of course, asymmetries in the direction of common sound changes may have other causes, too, some of them non-perceptual. Elsewhere, I have discussed aerodynamic reasons why long voiced stops should become voiceless but not vice-versa (Ohala, 1982).

Phonemes as Categories

The "phoneme theory" which was elaborated around the turn of the century consists of many semi-independent claims, e.g., that speech consists of a string of concatenated units (of "phoneme" size) and that physically distinct sounds such as the aspirated and unaspirated stops in the words *cool* [k^hu:l] and *school* [sku:l] are the "same" sound. With Sapir and a few other adventurous phonologists, these became psychological claims. But except for anecdotal evidence such as that reported by Sapir (1933) from his attempts to teach his informants to write their language, or Chao (1934) from word games, these claims have not been experimentally verified, i.e., where some pains are taken to control factors which might distort the results and therefore render their interpretation ambiguous (Twaddell 1935).

In the Phonology Laboratory at Berkeley we have tried to use some standard techniques from experimental psychology to confirm (or disconfirm) claims such as those made about the grouping of allophones into phonemes. One of these

techniques, so-called "concept formation" (CF) (Deese & Hulse 1967, chap. 12) had been previously used in a linguistics experiment addressing a syntactic issue (Baker, Prideaux, & Derwing 1973). Jaeger (1980a) reported our first experiences with this technique where the question addressed was "do native speakers of English regard the unaspirated stop in words such as *school* to be in the same category as the aspirated stop in *cool*?"

Briefly, a CF experiment would proceed as follows. The subject (S), seated in a quiet room, dons earphones and hears instructions of the sort: "You'll hear a series of words over the earphones, some of which belong to a certain category due to the way they sound; the rest do not belong to the category. If the word is in the category, respond 'yes', if not, respond 'no'. We'll let you know after you respond what the right answer was. You'll have to guess on the first few words but eventually you should figure how to anticipate the right answer. When this happens we'll give you a test. In this part we won't tell you what the right answer was after you respond."

Table 1. Sample CF session; target category: words with [k^h],

	Trial No.	Stimulus (over earphones)	Sample response from S	Correct answer (over earphones)
TRAINING SESSION	1	keep	no	yes
	2	fan	yes	no
	3	cash	yes	yes
	4	ghost	yes	no
	5	lip	no	no
	6	knife	no	no
	7	choir	yes	yes
	8	chip	no	no
	9	ceiling	no	no
	10	kerosene	yes	yes
	11	occur	no	yes
	12	cool	yes	yes
TEST	...			
	75	gnat	no	no
	76	science	no	—
	77	kitchen	yes	—
	78	square	yes	—
	79	step	no	—
	80	school	yes	—
	...			

Then the experimental session might proceed as in Table 1, which should be read from left to right, top to bottom.

As can be seen, except for focussing Ss' attention on the pronunciation of the stimulus words, no other hints are given about the defining attributes of the target category. Ss must figure this out on their own by pure induction.

It is possible in this way to teach linguistic concepts or categories to linguistically naive Ss without using any verbal mediation.

The first part of the experiment, where only clear, uncontroversial exemplars of category and non-category items are presented, with feedback, is the *training session*. As shown in Table 1, in order to make sure Ss don't inadvertently form some unwanted category using orthographic criteria (based on their own mental image of the spelled word), the category words have spellings which represent [k^h] in diverse ways: *ch*, *c*, *k*, *qu*. Moreover, some of the non-category words may be spelled with those same letters representing different sounds as in, e.g., *knife*, *chip*, *ceiling*. The criterion of having learned the category was set at 15 correct trials with 2 or fewer errors. When this was achieved the S began the *test session* where, without warning, the items whose category membership is at question are introduced (along with items like those in the training session) and where feedback is withheld. Of interest is how Ss categorize these new items in comparison to the clear cases. For example, in Table 1, if Ss respond 'yes' to the words *square* and *school* as often as they do to *kitchen* and the other words with [k^h] (corrected for the number of times they respond 'yes' inappropriately to non-category items), then we may conclude that they regard [k] and [k^h] as belonging to the same category.

In fact, as Jaeger reported, this is exactly what American English Ss (all linguistically naive) do, in conformity with the traditional phonemic analyses of English. This result is of some interest because there is phonetic evidence that the [k] in *school* is perceptually indistinguishable—at least to American English listeners—from the [g] in *ghoul* (Lotz, Abramson, Gerstman, Ingemann, & Nemser 1960), which is in a different phoneme.

One might still ask whether there could be some small phonetic difference between [g] and [k] which influenced Ss to put them in different categories. Also, is it possible that Ss had some sort of response bias? That is, that they automatically put the [k] or any new sound remotely like the [k^h] into the target category and that they would do the same if the target category was /g/ (i.e., included [g] and [g]).

These questions were addressed using the CF technique (Ohala, forthcoming). Twenty linguistically naive American English speaking Ss were assigned randomly to one of two groups. The overall format of the experiment was the same as that described by Jaeger with the following exceptions: although the target category for Group I was 'words containing [k^h]', included in the non-category exemplars during the training session were the words *ghoul*, *gate*, *gold*, and *grape*. These words were created by splicing the [s] from the beginning of the words *school*, *skate*, *scold*, and *scrape*, i.e., they contained [k], not [g] (if one believes there is a difference). These latter four words appeared intact (i.e., with the [s]) in the test session presented to Group I. Would Ss assign these words to the target category even though they—or the crucial part of them—had been presented as non-category items in the training session? The target category for Group II was words containing [g] or [g] which was

exemplified not only by "genuine" /g/ʰ such as those in *glitter* and *together* but also the four words given above, *ghoul*, etc., which had been formed by splicing the [s] from *school* and so on. Would Ss reject these words from the category even though a fragment of them had been given in the training session as category items?

As it turned out, Group I included the test words *school*, etc. in the category with [kʰ] just as decisively as Group II excluded them from the category of /g/, in spite of what might be considered conflicting evidence during the training session. More likely, the evidence in the training session was *not* conflicting because [g] and [k] really are identical but the criterion for assigning allophones to phonemes is distributional, not purely phonetic. These results also show that the categorization of new items in the test session is not influenced by any obvious response bias.

To my knowledge, these results and those reported by Jaeger represent the first experimental verification of the traditional phonemic claims. In other studies using the CF technique English Ss demonstrated that they regard the affricates [tʃ] and [dʒ] in words such as *chip* and *gyp* to be single sounds not clusters, again, as has traditionally been claimed. Jaeger (1980b) also used this technique to show that English speakers' "knowledge" of the sound patterns subsumed under the labels "vowel shift" and "vowel laxing" (e.g., the pattern that relates *insane* and *insanity*) is mediated to a significant extent by their knowledge of English orthography.

Size-sound Symbolism

From Jespersen (1922) to Jakobson and Waugh (1979) there has been extensive documentation and experimental verification (Sapir 1929) of a widespread, cross-language tendency to use certain specific speech sounds in words related to the semantic dimension of size and correlated notions, e.g., distance, age, etc. Specifically, high front vowels, such as [iːyeø], are used in words associated with SMALL and lower, backer vowels, especially [ɑɔo] in words associated with LARGE. Examples from a variety of languages of SMALL words are: English, *teeny*, *wee*, *little*, *doggie*; Spanish, *chico*; French *petit*; Japanese [tʃi:sai]; Greek *mikros*. Examples of LARGE vocabulary from the same languages: English, *large*, *huge*; Spanish, *gordo*; French, *gros*, *grand*; Japanese [o:ki:]; Greek, *makros*. Westermann (1927) showed that in some African languages tone was also used systematically to convey size, high tone for SMALL and low tone for LARGE, e.g., Twi [kàkrá] "small", [kàkrá] "large". Nichols (1971) documented cases in North American languages of systematic association of certain consonant types with opposite ends of the size continuum, e.g., Tillamook [waqɑq] "frog" but [wu-wekek] "(small) frog"; Wiyot, [ditatk] "two round things", [ditsafsk] (ditto, diminutive), [diʃaʃk] (ditto, augmentative).

There are, to be sure, exceptions to these tendencies. The English words *big* and *small* are prime examples of this. Nevertheless, Sapir (1929) and many

others after him demonstrated in psychological experiments that although native speakers may have these conflicting patterns in the existing vocabulary of size, when asked to assign nonsense names such as [gil] and [gɔl] to large and small objects, they almost invariably pick the word with the [i] for the small object and [ɔ] for the large object. This result has cross-language validity (Chastaing 1965). Also, a few quantitative studies of the relevant vocabulary have been conducted (Thorndike 1945, Chastaing 1965, Ultan 1978) and they demonstrate that the tendency noted is not significantly weakened by the exceptions.

There have been many attempts to find some articulatory dimension characteristic of these speech sounds which is iconic with the dimension of size, but none of them can account for the full range of the size-sound symbolism data, including those of tone and consonants. There is, however, one physical characteristic of speech sounds, whether vowel, consonant, or tone, which predicts fairly successfully how they will be used in size-sound symbolism, viz., their *acoustic frequency*. The vowels characterizing SMALL have high F2, those characterizing LARGE, a low F2 (or, more precisely, the difference between F2 and F1). The consonants used with SMALL have, in general, predominantly higher frequencies (either in F2 transition or in frication or noise burst) than those used with LARGE. With tone, it is quite simply the higher F0 which is used with SMALL and low F0 with LARGE.

But why should a correlation exist between frequency and size? It has been suggested that speakers would naturally associate high frequency sounds with small objects and low frequency with large ones because for physical reasons the natural frequency of the sound emitted by an animal or an object (e.g., bells, hollow logs) is inversely related to their physical dimensions (Jespersen, Chastaing). I believe this is essentially correct except that the association is much older than the individual speaker who recognizes it and even much older than human language or the human species. This, at least, is the lesson I derive from reading the ethological literature. For example, Morton (1977) documented the existence of an amazing cross-species (birds and mammals) similarity in the acoustic characteristics of those vocalizations which animals use in face-to-face competitive encounters. Confident aggressors emit harsh or staccato cries with a low F0; submissive or non-threatening individuals, tone-like cries with a high F0. The dog's aggressive growl and submissive whine or yelp are familiar examples. Morton suggested that these vocalizations, like many visual displays given in competitive encounters (e.g., erection of the hair), serve indirectly to convey an impression of the apparent size of the animal. As mentioned above, a large individual would naturally have large and more massive vocal cords (or, in birds, syringeal membranes) and these would, for physical reasons, tend to vibrate irregularly and at a low frequency; the smaller vocal cords of small individuals would tend to vibrate in a more regular way at a high frequency. The aggressor could exploit this and enhance its fearsomeness by emitting a cry with acoustic characteristics of a larger in-

dividual; a submissive individual benefits by giving the impression of being small, and therefore non-threatening, and so would produce a "small" cry. So consistent across species, so stereotyped in use and apparently unlearned is the communication of size by frequency, that this code, call it the *frequency code*, must be genetically specified—that is, it is maintained by a genetic not a cultural template.

I propose that the frequency code, which must be innate in humans as well as non-humans, is the basis for the phonetic patterns observed in size-sound symbolism. (I also believe the frequency code underlies universals of intonation—both linguistic and paralinguistic—and of certain facial expressions such as the smile which, when produced with a vocalization, could systematically alter the dominant frequencies of the resulting sound. Limitations of space prevent me from providing details on these points.) To be sure, the use of the frequency code in sound symbolism by humans differs in important ways from its use by animals. The speaker uttering the word *teeny* is not attempting to appear small or non-threatening. Rather, the intention is to refer to or denote something small. Nevertheless, in other respects the parallels are considerable: the selection of the frequency parameter to convey size (why not duration, bandwidth, spectral tilt?), the assignment of SMALL to high frequency and LARGE to low frequency (why not the reverse?). These parallels argue for a common origin of the frequency code.

It should not surprise us that the shape of speech, which is influenced by the physical environment, is also influenced by the ethological environment.

Conclusion

In this paper I have tried to demonstrate that phonology can benefit by embracing the data and methods from fields as diverse as acoustics, psychology, and ethology. Phonology does not lose its identity by this; what sets phonology off from other disciplines is its *questions*, its *end*, not its *methods* or *means*. In phonology (if not in ethics), the end justifies the means.

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Syntactic Reconstruction! A Case Study

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By contrast with the sixties, which can be described as the decade of phonology, since the mid seventies it is the study of syntax which has largely dominated the historical linguistics scene. There can be no doubt that this renewed interest in syntactic change had its origins in advances that were made on the purely descriptive side which resulted in more detailed and more sophisticated synchronic analyses of syntactic structure. The present paper may perhaps be taken as an example of this same process, illustrating how greater precision in synchronic analysis may be exploited for the purposes of diachrony. In it we will attempt to reconstruct certain aspects of the development of verb complements in English not through 'linear' reconstruction, that is based on an examination of successive texts, but rather through 'comparative' reconstruction, that is based entirely on an analysis of the situation in present-day English and present-day German. This exercise in comparative reconstruction without reference to the historical evidence in a context where subsequent verification may be carried out on the basis of the written documents, would seem worthwhile in view of the fact that most of the generalisations that are made about syntactic change are based on comparative rather than on linear reconstruction, and this in spite of the fact that the method of comparative reconstruction is normally resorted to by historical linguists only when documentary evidence is lacking and empirical verification is therefore impossible. Quite apart from the substantive issues involved, a control case such as this is of interest since it provides both an opportunity to test the accuracy of the method of comparative reconstruction and to see how syntactic changes postulated by this method are in fact reflected in documented language history.

If we leave aside complements formed with a derived noun (*They announced the queen's arrival/Sie kündigten die Ankunft der Königin an*) which are best dealt with in the lexicon, English may be said to have three and German two types of verb complement construction. Two of these are shared, namely infinitive constructions (*to go home* in *I advise you to go home; heim zu gehen* in *Ich rate dir heim zu gehen*) and *that/dass*-clauses (*that you will come* in *I hope that you will come; dass du kommst* in *Ich hoffe, dass du kommst*). The third type, the gerundive construction (*washing dishes* in *I hate washing dishes*) is specific to English. I will exclude the gerundive type of complement from the

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*=reconstructed; **=ungrammatical ??=of doubtful acceptability

present discussion because the German cognates of English gerundives, namely verbal nouns in *-ung* and verbal adjectives in *-end*, behave syntactically in German just like any other nouns and adjectives and are as much outside the sphere of verbal syntax as they are.

Limiting ourselves to the first two types of complement then, we find that the verbs which co-occur with (or 'govern') these may, quite independently within each language, be grouped into a number of fairly discrete classes established on the basis of common syntactic and semantic behaviour. Those classes in the two languages which share at least one pair of cognate verbs may then be equated as diachronic sames. I have listed these starred classes in Appendix 1, the cognate verbs in the two languages being *underlined*. I have then within each starred class set up syntactic correspondences based on a comparison of the syntactic patterning of its English and German members. Appendix 2 gives sample sentences to illustrate these correspondences. It will be seen that agreement in the form and distribution¹⁾ of the two complement types, infinitive constructions and *that/dass*-clauses, in the two languages although not total is such that it cannot be considered accidental. For, only in class VIII and a subsection of class IX is there in fact any absence of congruence. In these, German has a *dass*-clause in the first, a verbless complement in the second, corresponding to an accusative- and-infinitive construction in both cases in English. Everywhere else clause corresponds to clause and infinitival complement to infinitival complement in the two languages. In some classes only one of these types of construction is found, in others both types occur side by side, either in free variation or distributed on the basis of syntactic environment. Assuming that where the two languages agree in having parallel structures after the verbs of starred classes we are dealing with retention and where the structures are not congruent in identical environments we have innovation, after the verbs of wishing (class VIII) and certain verbs of saying and thinking (class IX) one or other language must have innovated.

In attempting to establish which language has innovated in the case of the verbs of wishing—and we will here confine our discussion to these—we may take as our point of departure the fact that in English the infinitival complements of these verbs are on the surface identical with those of the verbs of commanding (class VII) where both languages have the infinitival complement. We can tentatively postulate on this basis analogical extension of the infinitive construction from the verbs of class VII to those of class VIII, despite differences in their respective syntactic configuration, the members of class VII being potentially three-place, and those of class VIII strictly two-place, verbs. For it will be seen that the clue to the development lies with the verbs of class VII if these are subjected to closer scrutiny.

1) The forms of the verb in the infinitive in the two languages are cognate, that is in English to *sing* -Ø corresponds to *-en* in German *zu singen*, as also of course are the markers *to/zu* which precede it. These latter are in addition distributed in like fashion in the two languages, being absent after the verbs of classes I to III and present elsewhere.

In both languages independently the verbs of class VII may be divided into two subsets. In German the subdivision is based on the case-form of the object noun phrase following the verb, subset (a) demanding a dative and subset (b) an accusative. Less immediately obvious is a concomitant difference, which is however crucial, in that with the verbs of subset (a) the infinitival complement functions as the direct object whereas with those of subset (b) this is not so. There are two arguments to show this. First, with an (a)-verb such as *verbieten* 'forbid', the infinitive construction may be replaced by a noun phrase in the accusative such as the accusative pronoun *alles* 'everything' (*Sie verbot ihm alles* 'She forbade him everything'). Secondly, the complement can form the subject of a passive sentence (*Das Haus zu betreten wurde ihm verboten*, literally 'To enter the house was forbidden to/for him', or more naturally, with extraposition, *Es wurde ihm verboten, das Haus zu betreten*). Neither of these possibilities is open to the verbs of subset (b); here the nominal replacement of the infinitival complement is a prepositional phrase (*Sie überredete ihn zu einer Reise* literally 'She persuaded him to/for a journey', *Ich warnte ihn davor* 'I warned him against it') and the verb forms a personal passive with the object noun phrase as its subject (*Er wurde überredet, das Haus zu betreten* 'He was persuaded to enter the house').

If we turn to the situation regarding the verbs of this class VII in English, although they all pattern alike on the surface, it will be seen on closer examination that a subdivision is in fact present which closely parallels that of German. The division that I have adopted here is based on Quirk *et al.* (1972: 837), who use as their main criterion whether or not the verb permits the simple pseudocleft construction. Thus in subclass (a) sentences of the type *What I told him was to see a doctor* are acceptable whereas subclass (b) absolutely requires *to do* (*What I persuaded him to do was to get married*, but **What I persuaded him was to get married* is not possible).²⁾ The fact that not only in the syntactic correspondences of class VII but in general the object case in English regularly corresponds in German to either an accusative (*I saw him: Ich sah ihn*) or a dative (*I gave him a book: Ich gab ihm ein Buch*) allows us to derive the object case in English in the context of subclass VII(a) from a dative and in the context of subclass VII (b) from an accusative. This assumption that the subdivision is old, and with it the case difference, would seem to be indirectly confirmed from the way in which loan-words into English from French and from Latin are distributed between the subclasses. Thus while in Modern French *permettre* takes an indirect object with *à* and *aviser, encourager, inciter, persuader* all take a direct object, in exactly parallel fashion English *permit* belongs to subclass (a) and *advise, encourage, incite, persuade* belong to subclass (b).

Having established the subdivision of class VII as old, we can now argue that two syntactic developments of English which lack a formal parallel in

2) Cf. Fichtner 1979:279, with only minor discrepancies, although he places *advise, ask, order* and *tell* in subset (b).

German may be considered to derive directly from the neutralisation of dative and accusative after a class VII(a) verb. The first of these is the ability of English class VII(a) verbs to govern an infinitival complement in the passive (*She ordered the children to be sent home*), the second is the occurrence of infinitival complements after the verbs of class VIII (*She wanted the children to go home; She wanted the children to be sent home*). What unites these two postulated innovations is the fact that in both cases the complement is a single constituent in which the initial noun phrase is simply the subject of the complement, that is of the lower verb, and no longer also a constituent of the main clause, that is the object of the higher verb (as in *She ordered the children to go home*). This fact is demonstrated by the absence of any selectional restriction between higher verb and noun phrase. German, not surprisingly, has a *dass*-clause in both instances (*Sie befahl, dass die Kinder heim gesandt werden; Sie wünschte, dass die Kinder heim gehen; Sie wünschte, dass die Kinder heim gesandt werden*). Thus in terms of syntactic behaviour a class VII(a) verb is in this particular environment identical with a class VIII verb in that in both cases it can only be followed by a single constituent. Precisely why an infinitive construction should have become available in this environment in English but not in German can be accounted for by the loss of dative marking. For if we now return to the case of a class VII(a) verb, such as *forbid/verbieten*, followed by an infinitival complement in the active, we have established that such a complement would initially have consisted of a noun phrase in the dative followed by the infinitive construction in direct object function (*Ich verbot ihm, die Kinder mitzubringen* 'I forbade (to/for) him to bring the children'). After the loss in English of dative marking in the noun or pronoun, however, both it and the following infinitive construction would have become identical in syntactic status and thus open to reanalysis as a single sentence-like constituent (the so-called 'accusative-and-infinitive' construction) of which the noun phrase is simply the subject. The higher verb would thus have had its valency reduced by one argument (a three-place verb becoming a two-place one), though this would only have become overt at a later stage when the new analysis manifested itself positively in the two innovations already described, namely the ability of a VII(a) verb to accept infinitival complements in the passive and the spread of the infinitival complement to the verbs of wishing. Thus, from an inherited structure such as *She allowed the children to go home* we obtain on the one hand *She allowed the children to be sent home* and on the other *She wanted the children to go/be sent home*. Neither of these developments has, as we have seen, a formal counterpart in German where the subject of the infinitive continues to maintain its double role as a constituent of both the higher clause and the complement. Danish on the other hand, which has like English merged dative and accusative, has also extended the infinitive construction to the verbs of wishing.

Having postulated the above reanalysis, together with its syntactic consequences, on the grounds of simplicity and explanatory power, we will now see

to what extent it receives confirmation from the documentary evidence. That the ancestor of *forbid*, together with other verbs now obsolete, was in Old English as in Old High German constructed with the dative is easily ascertainable, as also is the fact that verbs of wishing demanded a *that*-clause as complement in Old English. Our reconstruction was, however, unable to predict that the verbs of commanding, which initiated the whole process, originally occurred far more frequently with a *that*-clause than with the noun-phrase-plus-infinitive construction,³ and that in general the infinitival complement spread rather slowly at the expense of the clausal type. Thus, according to Ard (1975:24), after verbs of commanding *that*-clause complements were decreasing during the Old English period and had in fact disappeared by Middle English while noun-phrase-infinitive complements have steadily increased in frequency, from 'rare' in Old English to 'frequent' in Modern English. Ard's synthesis is based on Visser (1969/73:227Off., *passim*), who notes that the noun-phrase-plus-infinitive type came into its own only in Middle English or even later and gives the following first dates: *entice* c. 1389–1400, *compel* c. 1398, *force* c. 1400, *require* c. 1420, *entreat* c. 1439, *permit* 1514, *challenge* 1559, *persuade* 1579, *press* 1590, *allow* 1743, *order* 1749. With class VIII verbs the first dates are in fact often extremely recent: *desire* 1412, *wish* 1581, *want* 1670, *hate* 1847, *intend* 1874–6, *prefer* 1887, *love* (once c. 1380, then) 1921. 'Derived subjects', such as subject of passive complement (*the children to be sent home*) and *there* (*I want there to be peace*), are also extremely recent, so that we must assume a gradual diffusion of the construction over the centuries.

To summarise this first section, it would appear that our comparative reconstruction is not only compatible with the historically attested facts but is also capable of accounting for them in a systematic way. It is, however, unable to attach to the postulated spread of the accusative- and-infinitive construction any kind of time-scale other than that it must have postdated the merger of dative and accusative. It is encouraging to find that a recent statistical survey has established a direct link between an increase in the proportion of non-finite to finite clauses from Old English onwards and the development of the accusative-and-infinitive construction, so that already by Middle English infinitives outnumbered *that*-clauses as objects of finite verbs (Manabe 1979:44).

We will now turn to another mechanism which would also appear to have played a role in the demise of clausal complements, this time in conjunction with the verbs of classes I, IV and V. What distinguishes these verbs from most of the others is the fact that they invariably take an infinitive complement (*He began to study music*) whereas the performatives (class VI), those verbs of thinking which behave like *hope/hoffen* (class IXc), and the verbs of wishing in German (class VIII), take both an infinitive construction (*I hope to study music*) and a clausal one (*I hope that John will study music*). In the case of

3) It is necessary to specify the presence of the noun phrase since in Old English constructions such as *He bebeod faran* 'He ordered to leave' (cf. Modern German *Er befahl aufzubrechen*) without a noun phrase were well attested, although they have not survived.

these latter the selection of the infinitival complement implies coreferentiality between higher and lower subject (*John hoped to study music*, but not ***I hoped John to study music*) whereas in the former such coreferentiality would seem to be part of the structural analysis and the verbs of classes I, IV and V may for this reason be termed equi-subject verbs (*John began to study music*, and not ***I began John to study music*). In fact it looks as if these equi-subject verbs have lost the ability to co-occur with the wider range of complements. We will argue that there is indeed some concrete evidence for this and that the equi-subject constraint is indeed an innovation.

Our initial method will be internal reconstruction in each language based on a closer examination of all the relevant verb classes. We will argue that these in fact form a cline, which ranges from full lexical verbs (such as those of classes VI, VIII and IX mentioned above) to mere auxiliaries (such as the modals of class I), with classes IV and V occupying an intermediate position. We will then attempt to show that this synchronic cline is the direct reflex of a diachronic process of grammaticalisation demoting full lexical verbs to the status of auxiliaries, from open to closed classes. It will be seen from the sample sentences of Appendix 2 that a performative verb such as English *promise*, German *versprechen* (class VI), is a full lexical verb since it imposes its own constraints on the choice of subject, which must as a rule be human or at least animate, and since its complement has clausal or infinitival form under the stated conditions (*He promised to protect her*/*Er versprach sie zu beschützen*; *He swore that the man would come*/*Er schwur, dass der Mann kommen würde*). A class V verb such as English *try*, German *versuchen* on the other hand, admits only an infinitival complement and requires the subjects of higher and lower verb to match in the sense that both must be coreferential and as a rule animate, so that the surface subject may be said to be derived by equi-subject deletion (*He tried to help her*/*Er versuchte, ihr zu helfen*; ***He tried Mary to help her*/***Er versuchte Maria ihr zu helfen*; ***The glass tried to crack*/***Das Glas versuchte zu zerspringen*). At the other end of the cline, with a modal verb of class I such as *can/können*, the surface subject is determined entirely by the lower verb and the modal itself imposes no constraints on it whatsoever (*He can come*/*Er kann kommen*; *It could rain*/*Es könnte regnen*). Any noun phrase which is a suitable agent for the lower verb is also a suitable surface subject of the entire sentence, so that the surface subject may be said to be derived by subject-to-subject raising. In the middle region of the cline, an aspectual verb (class IV) such as *begin/beginnen* also imposes no restrictions on the choice of subject and in this respect resembles the modals (*He began to sing*/*Er begann zu singen*; *It began to rain*/*Es begann zu regnen*). The equi-subject verbs are thus by no means uniform, and the difference between them and the full verbs is clinal rather than absolute.

While the selectional restrictions are thus parallel in both languages, there are certain differences in the case of passives. These occur freely in both languages after a modal—which behaves in this respect rather like a tense

or modal affix (*John must demolish the houses* and *The houses must be demolished*; *Hans muss die Häuser abbrechen* and *Die Häuser müssen abgebrochen werden*). Although the precise conditions would not appear to have been formulated so far, their use after an aspectual verb is restricted in English (*The trees began to be felled* but ??*The chicken began to be eaten*) and in German a passive in this context is impossible (***Die Bäume begannen gefällt zu werden*; ***Das Hühnchen begann gegessen zu werden*). After a class V verb the restrictions are perhaps even more severe in English (***The houses tried to be demolished*, but *He managed to be cheated*) and in German a passive is again excluded (***Die Häuser versuchten abgebrochen zu werden*; ***Er schaffte es, betrogen zu werden*). Whether or not, and if so to what extent, a passive is possible after any given verb may thus, like relaxation of selectional restrictions, be an indicator of degree of grammaticalisation.

In order to substantiate this postulated process of grammaticalisation we must return once more to the performatives (class VI) which, as we have seen, normally require an animate agent and can take either kind of complement. Certain class VI verbs, however, such as *threaten* and *promise* together with their German counterparts *drohen* and *versprechen*, have uses in which the animateness condition is not met (*The summer promises to be warm*/*Der Sommer verspricht schön zu werden*; *The floor threatened to collapse*/*Der Boden drohte einzubrechen*). This relaxation of the animateness constraint correlates, furthermore, with the inability of the verb to co-occur with a clausal complement (***The floor threatened that it would collapse*). And lastly, the verb suffers in this context of a non-animate subject a weakening of its lexical meaning which has the effect of turning it from a performative of class VI to an aspectual of class IV. For, in this specific environment, the verb merely signals the imminence of the event together with attitudinal information as to whether or not it is welcome and, of course, an addressee noun phrase is no longer possible (***The weather promised me to improve*/***Das Wetter versprach mir besser zu werden*, although this latter is acceptable in German when *mir* is taken as an ethical dative). We witness here, then, a direct correlation between subject selection, lexical meaning of the verb and choice of complement type, the relaxation of constraints on the subject implying loss of semantic content in the verb (with 'demotion' to class IV) and curtailment of possible complements.

In German there is in addition a further syntactic correlation in that in their aspectual uses these verbs behave in certain respects like auxiliaries whereas in their performative uses this is normally not so (Höhle 1978). Thus when the higher clause is itself subordinate we find that in the aspectual uses higher clause and complement are fused so that their verb forms occur jointly in an unbroken sequence at the end whereas in the performative uses of the same verbs higher clause and complement remain normally discrete.⁴⁾

4) In a main clause the finite verb is in second position (*Hans gab das Buch Maria* 'John gave the book to Mary') whereas in a dependent clause it is in final position (...*weil Hans das Buch Maria gab* '...because John gave the book to Mary'). If an auxiliary verb is present the

A parallel range of uses occurs in German with another set of verbs, notably *wollen* 'will, want' and *möchten* 'want, like', which are also listed twice, namely both in class VIII and class I. We find here the same syntactic and semantic situation as was observed above regarding classes VI and IV. The verb has, again, its full semantic content and syntactic range in combination with an animate subject whereas with other kinds of subject the modal connotations prevail and the syntax is that of an auxiliary. As may be seen from Appendix 2, the syntactic correspondences reflect this latter situation, indicating that in its volitional use English *will* has been displaced by *want* and that the modal uses have been generalised (*The tomatoes will not ripen/Die Tomaten wollen nicht reifen*; *It will rain/Es will regnen*; *John will study music/Hans will Musik studieren*; ***She will that he resign/Sie will, dass er zurücktritt*).⁵⁾ The movement from lexical towards grammatical meaning of the verb correlates furthermore with a parallel formal weakening in that the modals have reduced forms capable of being derived from the corresponding full ones but not vice versa (*ll* from *shall, will*, etc.).

We may deduce from all this that there must be at least two stages to the grammaticalisation process, first introduction of the equi-subject constraint and then subject-to-subject raising. We have argued that this process is unidirectional, the modal and aspectual verb being derivable from the full lexical verb and not vice versa. Our main argument must plainly be semantic, it being impossible to 'reconstitute' the lexical meaning from the modal or aspectual one. The hypothesis is strongly supported also from the composition of the verb classes. For, significantly, class I comprises in both languages verbs which are extremely archaic in that they are followed by a bare infinitive without the usual infinitive marker (*to/zu*), the membership is closed, the proportion of cognates is very high, and the morphological structure and etymologies of its members leave no doubt as to their Indo-European origin. These verbs must

rule is transferred to it (*Hans hat das Buch Maria gegeben* 'John has given the book to Mary'; ...*weil Hans das Buch Maria gegeben hat* '...because John has given the book to Mary'). In complex sentences the complement normally follows, although occasionally it precedes, the higher clause (*Hans sagt, dass er das Buch Maria gegeben hat* 'John says that he has given the book to Mary'; *Hans vergass, das Buch Maria zu geben* 'John forgot to give the book to Mary'). When the complex sentence is however itself dependent, higher clause and complement remain discrete in the case of a clausal complement (...*weil Hans sagt, dass er das Buch Maria gegeben hat* '...because John says that he has given the book to Mary'). But when the complement is infinitival there is potentially a choice between biclausal and monoclausal order, which is partly lexically determined and partly free (...*weil Hans vergessen hat, das Buch Maria zu geben*: biclausal, and ...*weil Hans das Buch Maria zu geben beabsichtigt*: monoclausal). The performative uses of *versprechen* are as a rule biclausal (...*weil Hans versprochen hat, das Buch Maria zu geben*; ?*weil Hans das Buch Maria zu geben versprochen hat*). The aspectual uses must be monoclausal (...*weil das Wetter jeden Augenblick besser zu werden verspricht* '...because the weather promised to improve any moment'; ***weil das Wetter versprach, jeden Augenblick besser zu werden*).

5) Semantically these sentences are not equivalent in the two languages; in German the semantic range extends from an assessment such as 'looks as if' to volition/intention.

have been undergoing the grammaticalisation process longest. Classes IV and V are by comparison less archaic, have fewer cognates and have been open to new membership including loan-words from Romance. But in their cases too we have seen evidence of encroaching demotion, for instance in the context of passives.

This unidirectional character of the grammaticalisation process is crucial to reconstruction in those cases where the two languages fail to show parallel constructions, for instance where German has sequences of modal and modal, of modal and auxiliary, and of 'modal' and complement clause (see Appendix 2). Since German has retained more of the full lexical status of the verbs in question, we must assume that the innovation has taken place in English although exactly what should be reconstructed for the common ancestor is far from certain. The protolanguage will clearly resemble German more than English, with certain verbs at least retaining their status of full verbs alongside modal and aspectual uses. For it would be uneconomical to suppose that parallel processes of grammaticalisation would have operated independently on cognate verbs in closely related languages, although such processes may possibly have continued after separation. Verification from historical sources is hampered here by the fact that our dictionaries do not systematically record either syntactic behaviour or selectional restrictions (although Greule 1982 shows considerable progress in this respect for the earlier stages of German). There can be no doubt though that the modal uses of the 'premodals' were already well established in Old English (Lightfoot 1979 ch. 2; Visser 1969/73 *passim*) so that in their case the equi-subject constraint must have developed in a prehistoric period antedating the common ancestor. With some of the other verbs, however, its development should in principle still be traceable.

Finally, in order to complete the picture, let us turn to the reconstruction of complement clauses. These latter appear to have been already well established in the protolanguage as, on the one hand, the sole complement type permitted after such verbs as *say/sagen* etc. (class IXa), and, on the other, as alternants of an infinitival complement (dependent upon whether or not higher and lower subject are coreferential) after *hope/hoffen* etc. (class IXc), after the verbs of class VI and, originally, after those of class VIII. The fact that comparable complementary distributions are also found in other Indo-European languages, although associated with different verb sets in individual languages,⁶⁾ would certainly support reconstruction of the alternating pattern. Both sorts of verb must thus be presumed to be inherited.

The reconstructed conjunction */θat/ is, with the exception of stress, formally identical with the neuter singular */θat/ of the demonstrative pronoun

6) The following survey I owe to the co-operation of a number of colleagues and students, who were kind enough to translate a small sample of English sentences into the respective languages. It will be seen that, outside the Balkan linguistic area, there is a certain preference for infinitive constructions when higher and lower subject are coreferential. (*=infinitive construction, o=clause, x=other; EQ=equi-subject, -EQ=different subjects):

which underlies both English *that* and German *das*. But this fact must, for the shallow time-depth we are here concerned with, be considered accidental for there would seem to be no evidence to suggest that either in the protolanguage or subsequently there has existed a syntactic relationship between the two forms which could account for the reanalysis.⁷⁾ The situation presumably resembles that of Modern German where the same homophony is found (the orthographic distinction between the conjunction *dass* and the pronoun/article *das* is a mere spelling convention dating from the sixteenth century). Documentary evidence of a formal difference between conjunction and stressed pronoun is obscured by the fact that in the earlier stages of both languages the two words were spelled alike. Positive arguments in favour of a phonological distinction are thus hard

	'will'	'hear'	'let'	'begin'	'order'	'went'	EQ/-EQ	'say'	EQ/-EQ	'hope'	EQ/-EQ
Danish	*	*	*	*	*	*	*	○		○	
Dutch	*	*	*	*	*	*	○	○		○	
German	*	*	*	*	*	*	○	○	○	*	○
French	*	*	*	*	*	*	○	○	○	*	○
Portuguese	*	*	*/○	*	*/○	*	○	○	○	*	○
Latin	×	*	*	*	*/○	*	*	*		*	
Anc. Greek	×	×	*	*	*	*	*	*	○	*	*
Mod. Greek	×	○	○	○	○		○		○		○
Serbian	○	×	○	○	○		○		○		○
Slovene	×	×	○	*	○	*	○		○		○
Croatian	*	×	○	*	*	*/○	○		○		○
Russian	×	○	*	×	*	*	○		○	*	○
Sogdian	×	×	○	*	*/○	*/○	○		○		○

7) It is customary to assume that the conjunction arose originally from the reanalysis of a demonstrative pronoun in what would appear to have been a sequence of two main clauses (such as *I heard that* and *He is ill* (Lehmann 1980, with references), especially since formally and functionally parallel conjunctions alongside homophonous neuter pronouns are found in other Indo-European languages (for example Sanskrit *yad*, Latin *quod*). Whether the homophony should be considered to be 'old' or to have resulted from syntactic calquing (cf. Shimomiya 1974 for a parallel) is uncertain. An alternative syntactic pattern in which the complement is extraposed and 'supported' in the main clause by an unstressed neuter pronoun is, however, also found in other Indo-European languages (Rix 1979). In German this pronoun (*es* or *das*) is optional with many verbs but appears to be obligatory with some (especially those of class V). When the complement is the focus the pronoun is stressed and only *das* occurs (*Das weiss ich, dass er Geld gestohlen hat* 'That I know that he has stolen money'); when it is 'given', unstressed pronoun and conjunction follow each other (*Ich weiss es/das, dass er das Geld gestohlen hat* 'I know (it/that) that he has stolen the money'). Simple /θat/ might thus alternatively represent a telescoped /θat/-plus-/θat/, the extraposed construction perhaps constituting an initial device for turning a clause into a complement. Both patterns, the simple and the extraposed one, are found in Old English and in Old High German. In fact both the simple reanalysis from pronoun to conjunction and extraposition have been independently associated with change to verb-object order, the former by Lehmann (1976, 1980) and the latter by Kuno (1974).

to find; Müller and Frings (1959), however, show that in Old High German verse only the pronoun was capable of carrying the *ictus* and Mitchell (forthcoming) assumes a similar phonological differentiation for Old English.

If we now look back over the issues discussed in an attempt to evaluate our experiment, I do not think that we have in fact found any serious discrepancy between the results of comparative reconstruction and the documentary evidence other than the inevitable telescoping of discrete successive steps into a single unitary change which is one of the characteristics of reconstruction. The complementary nature of the two methods is well illustrated in their treatment of the development of the accusative-and-infinitive complement. While this appears in the documents as a gradual diffusion through the respective lexical classes and sentence types, it appears in reconstruction as a single process with a unitary motivation. One is here reminded of the early stages of Indo-European studies, when the fundamental patterns of phonological change were first perceived not in linear language history but rather as a *fait accompli* through the long-distance comparison of European languages with Sanskrit.

Our attempted verification of the postulated process of grammaticalisation has proved less satisfactory. There would appear to be two reasons for this. One is the fact that the time-depth involved greatly exceeds the period for which we possess documentary evidence. The other is the present lack of readily accessible information on valency structure at various points in time. When dictionaries become available which systematically record syntactic configurations and semantic constraints, the historical linguist's task will be very much easier. While awaiting this, however, it has, I think, proved useful to work from living languages where the volume of synchronic material and the possibility of consulting speakers makes the relevant facts more readily accessible. A rather striking result of our analysis of the grammaticalisation process is the close interdependence of syntactic and semantic change which has brought about a complete reversal in the most advanced cases of dominance relations, the 'higher clause' having lost its independence so as to become a mere modifier of the former complement, while this latter has lost its syntactic 'governedness' (its *Kasuscharakter*, as Delbrück once put it). While in this particular case the higher verbs have retained at least some of their free form status, it seems probable that the process will continue and ultimately create new morphology (Givón 1979: 239ff.).

We have discussed in some detail two basic mechanisms of syntactic change, replacement of one structure by another (*that*-clause by infinitive) and internal reanalysis. Of the latter we have seen two kinds, reinterpretation of constituent structure, as in the case of the noun-phrase-and-infinitive construction, and reinterpretation of syntactic status, as in the case of the aspectuals and modals. The first type corresponds to what Langacker (1977:64) termed *resegmentation* and Paul *Gliederungsverschiebung*, the second to Langacker's *reformulation*, which includes grammaticalisation, a concept familiar at least since Meillet (Vincent 1980). As may be seen from the reference to Paul and Meillet, both

concepts are in fact traditional, but we have recently gained a better understanding of them as mechanisms of syntactic change. A basic characteristic of internal reanalysis is its 'invisibility' in that it can only be identified indirectly, either through synchronic ambiguity or through subsequent developments sparked off by it (what Timberlake 1977:141 terms its 'actualisation': the gradual mapping out of the consequences). Thus although sentences are not like lexical items, which are handed down wholesale, we have come to realise that the transition from an old to a new analysis takes place through the surface structure, the output of the older members of the speech community formed on the basis of their rules constituting the raw material from which the new generation draw their rules. The problem is to discover how and why a reanalysis becomes possible.

This new awareness of the important role of surface structure is, I think, a unifying factor in much current work, irrespective of theoretical outlook.

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Appendix 1: Shared verb classes (cognates in italics)

I. Modals:

can, dare, may (might), must, need, shall, will
 dürfen, können, mögen, möchten, müssen, sollen, wollen, brauchen

II. Verbs of perception:

feel, hear, see, watch
 finden, fühlen, hören, sehen

III. Causatives:

bid, have, help (to), let, make, teach
 bitten (zu), heissen, helfen (zu), lassen, lehren, machen, spüren

IV. Aspectuals:

begin, cease, continue, fail, start, used to; promise, threaten
 anfangen, aufhören, beginnen, pflegen; drohen, versprechen

V. Verbs of achievement:

attempt, hasten, manage, refuse, seek, try
 (es) probieren, (es) riskieren, (es) schaffen, (es) vermeiden, (es) verstehen, (es) versuchen

VI. Performatives:

promise, swear, threaten
 drohen, geloben, schwören, versprechen, zusagen

VII. Verbs of commanding:

- (a) *advise, allow, ask, forbid, order, permit, tell, teach*
 befehlen, empfehlen, erlauben, genehmigen, raten, untersagen, *verbieten*, vorschlagen
 (with dative)
 (b) *compel, dare, encourage, force, incite, induce, persuade, press, tempt, urge; challenge*
command, entreat, oblige, request, warn
 auffordern, beauftragen, drängen, ermutigen, ersuchen, herausfordern, nötigen, über-
 reden, verhindern, verpflichten, warnen, zwingen (with accusative)

VIII. Verbs of wishing:

desire, hate, like, love, prefer, want, wish
 bevorzugen, (es) hassen, (es) lieben, möchten, wollen, wünschen

IX. Verbs of saying and thinking:

- (a) *accept, acknowledge, admit, announce, answer, assume, complain, contest, deny, dream*
ignore, imply, notice, regret, remark, report, say (with clause)
 antworten, bemerken, beweisen, entgegenen, erwidern, erwarten, finden, fühlen, leugnen,
 sagen, träumen, zustimmen, zweifeln (with clause)

- (b) assume, *believe*, consider, declare, doubt, expect, *feel*, *find*, hold, know, prove, *reckon*, report, show, suppose, *think*, *understand* (with accusative-and-infinitive and verbless complement)
finden, *fühlen*, *glauben*, *halten* (für) (with verbless complement)
- (c) *forget*, *hope*, insist, *mean*, propose, *reckon* (with infinitive and clause)
denken, *glauben*, *hoffen*, *meinen*, *rechnen*, *vergessen*, *verstehen*, wissen (with infinitive and clause)

Appendix 2: Syntactic correspondences

- I. He must go. He cannot come.
 Er muss gehen. Er kann nicht kommen.
- The glass can break. It could rain.
 Das Glas kann zerbrechen. Es könnte regnen.
- The houses must be demolished.
 Die Häuser müssen abgebrochen werden.
- The tomatoes will not ripen. It will rain.
 Die Tomaten wollen nicht reifen. Es will regnen.
- Sie will, dass er zurücktritt 'She wants him to resign'
 **She will that he resign.
- Er hat zurücktreten wollen.
 **He has willed resign 'He (has) wanted to resign'
- Er soll zurücktreten wollen.
 **He shall will resign 'He is said to want to resign'
- Er soll zurücktreten wollen haben.
 **He shall have willed resign 'He is said to have wanted to resign'
- II. She heard him leave the house.
 Sie hörte ihn das Haus verlassen.
- She heard that he had left the town.
 Sie hörte, dass er die Stadt verlassen hatte.
- Er hörte ein Lied singen 'He heard a song being sung'
 **He heard a song sing.
- III. She let him come.
 Sie liess ihn kommen.
- IV. He began to fell the trees.
 Er begann die Bäume zu fällen.
- The grass began to burn. It began to rain.
 Das Gras fing an zu brennen. Es begann zu regnen.
- The trees began to be felled.
 **Die Bäume begannen gefällt zu werden.
- V. He tried to escape.
 Er versuchte zu entkommen.
- **The glass tried to break.
 **Das Glas versuchte zu zerspringen.
- He managed to be cheated.
 **Er schaffte es, betrogen zu werden.

VI. He promised to protect her.

Er versprach, sie zu beschützen.

He promised that he would protect her.

Er versprach, dass er sie beschützen werde.

He swore that the story was true.

Er schwur, dass die Geschichte wahr sei.

The weather promises to improve.

Das Wetter verspricht sich zu bessern.

The ship threatened to go down any minute.

Das Schiff drohte jeden Augenblick unterzugehen.

VII. (a) She advised him to go home.

Sie riet ihm, heim zu gehen.

She ordered him to be sent home at once.

****Sie befahl ihm/ihn sofort heim gesandt zu werden.**

(b) She persuaded him not to go home.

Sie überredete ihn, nicht heim zu gehen.

VIII. I want to go.

Ich möchte gehen.

I want him to go.

Ich möchte, dass er geht.

IX. (a) I said that I will come.

Ich sagte, dass ich komme.

I said that he will come.

Ich sagte, dass er kommt.

(b) I knew him (to be) dead.

Ich wusste ihn tot.

(c) I hoped to arrive in time.

Ich hoffte beizeiten anzukommen.

I hoped that he would come.

Ich hoffte, dass er kommen würde.

Linguistic Similarity and Its Significance: Comparative Procedures

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1. Comparing languages is almost a normal human activity. Human beings feel that their native language is distinctive and characteristic, a bond which gives its identity to their group, and at the same time what marks its main differences to others.

According to my experience with South American Indians, I would say that people in primitive conditions, still without agriculture, have already a feeling for linguistic links. In working with the most primitive Chaco stock, the Matacos, I found that they know that the neighbouring speakers of two languages, Choroti and Ashushlay, belong to their own family, although they are far from mutual intelligibility, nearly as distant as English is from Greek. They feel also that another neighbour, the Tobas, with a completely unrelated language, is somehow close to them, and in fact they share, along with many cultural elements, loan words as well. But a greater difference separates them from Guaraní or Quechua-speaking Indians, whom they also know, but who have completely different languages and cultures.

Men have a sense of linguistic similarity, and on this basis they construct an ethnic consciousness. The famous chapter 10 of Genesis represents in a more developed degree of culture an effort to understand the ethnic significance of one's own language, just as the stories of Ismael and Jacob reflect the linguistic similarity between Arabic and Hebrew. In reading the list of Noah's children and grand-children we observe that the descendants of Sem and Cham are more closely akin than those of Japhet, who in the list are the ancestors of the Ionians, Iranians, Caucasians, and so on.

The Greek and Romans felt, it seems, less curiosity about foreign peoples. They did not possess clear traditional accounts of the common origin of mankind, or a myth like that of the tower of Babel relating to the primitive unity of human languages. As is well known, only Plato in his speculations in the *Kratylos* about the origin of language, made a reference to the similarity of Phrygian to Greek, but neither he nor widely enquiring historians like Herodotus, detected the similarity of Old Persian, which was more or less known to them, to Greek. The opposition Greek-Barbarian prevented them from discovering any relationship. This explains why the first Roman historians to write about Celts and Teutons did not discover a linguistic similarity which is obvious in such words as the numerals *ains*, *twai*, *threis* in Gothic and *unus*,

duo, tres in Latin, and in the same way between *fadar, brothar* and *pater, frater*.

The translators of the Bible and its commentators brought first to Western culture a consciousness of linguistic similarities among nations. Tubal and Magog and Gomer were supposed to be the ancestors of the Iberians, Scythians, and Cimbrians, whether they be Cimmeric or Germanic nations. For centuries the need to explain the diversity of languages and nations was satisfied by interpretations of the afore-mentioned chapter 10 of Genesis.

The more rational classification of European languages achieved by Joseph Justus Scaliger in establishing the four big families distinguished by the word *Deus, Gott, Theós* and *Bog*, i.e. Romance, Germanic, Greek and Slavic languages, separated from them the so called *matrices minores*, the smaller languages, like Albanian, Hungarian, Irish and Britonic, Basque, etc., fundamentally still keeping the old distinction between the most noble and important languages, descending directly from the languages which stemmed from the tower of Babel and those resulting from mixtures and corruptions.

2. The ideal method of classifying languages for a linguist with a historical background is the genealogical one, allowing him conveniently to define a language as Romanic or Germanic or, taking taxonomy a step further, to say that it is an Indo-European language.

But this genealogical type of classification confines the linguist within a metaphor. Only figuratively can we call languages daughters or sisters and speak of their genealogical relationship.

Languages are among the stablest and most open to analysis of human institutions and usages, although they live only on the lips and in the brains of speakers, and are secondarily recorded in their scripts. To say, then, that a language is Romanic or Germanic or Indo-European means simply that such a language keeps in its elements traces of a more or less recognizable past.

Nevertheless, within the mutability of human things, language is stable. As a system of signs it has to accomplish its function for as long as possible. In the languages of culture it is usual for an educated person to be able to read a "classic" several centuries old with the help only of dictionaries or commentaries. The relationship among languages of common descent is—because of this stability—frequently obvious. Because the relationships within a more extensive and older stock such as Indo-European are no longer obvious, a more refined way of comparing languages had to be formulated. As the supposed common origin lies five or six thousand years back, an intuitive approach was not possible in this taxonomic second degree, and correspondences and similarities had to be discovered by the comparative method.

Our problem is to examine the limits of knowledge as to the history of languages, and at the same time to note that every apparent similarity at all levels could be the result of chance. The longer time periods are, the more can the mutability of language be deceptive about similarities. To give some examples of modern IE forms: could one—without older forms and the com-

parative method—recognize the identity of E. *come* and Port. *vir*? Or would one correctly separate E. *much* from Span. *mucho*? On the other hand, practical limitations in what seem to be unlimited possibilities of structure can produce visible typological similarities among languages without any traceable contact. So Zapotec is a VSO language like Hebrew and Quechua an SOV language like Dravidian.

But in fact linguists have not paid attention to time to the extent that would appear desirable. Perhaps the first occasion attention was drawn to the importance at the linguistic evolution of a longer or shorter time span was by lexicostatistic work, and specifically by that of A. L. Kroeber (1960, 19).

3. The ideas of M. Swadesh on lexicostatistics and glottochronology for the first time raised the question of language in time, or rather, that of the evolution of language, and languages, in time periods longer than those considered up to then. Mary Haas (1966, 149) recalls that when Kroeber referred to the genetic taxonomy which results from the historical-comparative method, he established its temporal limits between five and seven millenia,

but there comes a point in the past—perhaps 10,000 years ago, perhaps less—at which the method no longer yields reliable results (Kroeber 1960, 21; cf. Kroeber & Chrétien 1937).

At first Swadesh did not see new horizons opened, because he was occupied mainly with American Indian languages; the time perspective for the human population of the New Continent was then believed to be not much longer than fifteen millenia.

But the problem of time depth for languages can be more far reaching. His initial position prevented Swadesh from seeing limits and possibilities clearly. He placed an excessive emphasis on his distinguishing between “cultural” and “non-cultural” words, forgetting that “cultural” depends on what culture is in every case. Although he made very wise corrections in his lists, and his definitive list of 100 words (Swadesh 1955, 132) seems by far the best one, the assumption of “non-cultural” words being obviously exempt from borrowing is difficult to accept, and above all it cannot be supported that

the similarities found cannot be due to other than genetic factors (Swadesh 1954, 313).

On the other hand, as for the application of lexicostatistics, the greater the time depth, the more its value diminishes, especially when there are no written documents.

Swadesh at first established (1954, 321) a limit of five percent, under which similarities can be due to chance. Unfortunately he later (Swadesh 1972, 285–292) forgot this limitation and fell a victim to the temptation of uncontrolled comparisons. Lexicostatistics, deprived from their mathematical apparatus, can not offer results comparable to those of carbon 14 or dendrochronology, which

inspired Swadesh. Similarities observed in the list of one hundred "non-cultural" words are not necessarily genealogical. Nevertheless it can be granted that the counting of similarities has some significance.

Swadesh tried to determine (1954, 326f.) taxonomic correspondences for the percentages of similarities observed in his list. Even if their significance is not translatable into mathematical terms, they have an orientation value which is undoubtedly extremely useful for languages with an unknown history. It would be unwise to discard them.

Table 1

TERM	DIVERGENCE CENTURIES	COGNATE PERCENT	GENERAL CHARACTERISTICS	EUROPEAN EXAMPLE
language	0-5	100-81	mutual intelligibility of local forms (dialects)	English
family	5-25	81-36	relationship apparent even to layman	Germanic
stock	25-50	36-12	relation obvious to comparativist	Indo-European
microphylum	50-75	12- 4	rare but striking basic agreements	?
mesophylum	75-100	4- 1	very rare agreements	?
macrophylum	over 100	less than 1	discoverable thru reconstruction	?

(from Swadesh 1954, 326)

A species in natural science would in linguistic taxonomy be comparable to a dialect, a genus to a language, and a family to a language family. If we follow the conventions of natural science, we could also compare the language stock with order. Swadesh gives as still larger units, microphylum, mesophylum and macrophylum (cf. Table 1). Work remains to be done to refine the limits of linguistic stock and the possibilities of going beyond it. If similarities under five percent are of doubtful or no value, the highest taxon in linguistics with the available methods would be the (micro)phylum.

4. The need for a linguistic taxonomy was clearly felt as a greater time depth was to be accounted for. An age of 6000 to 5000 years is assumed for the separation and evolution of big groups, documented since the beginning of writing, such as Indo-European and Semitic. If we call these groups stocks, families are to be considered the result of separation some 2000 to 1000 years ago, such as Romanic, Germanic, Slavic, and so on.

It is not customary in linguistic classification to look on time depths for the separation of languages, but a systematic account of time would improve its taxonomic classification. Problems which up to now remained on the frontiers of scientific knowledge could be considered within a taxonomic setting. Only if there were a method for establishing a taxonomy beyond the stock, could the

connection between Indo-European and Semitic (Möller 1911, Cuny 1946, Silvestri 1981) or between Indo-European and Uralic (Collinder 1965), or the whole problem of Hamito-Semitic, including Egyptian (Cohen 1955, 179-217, Vergote 1970, 531f.) be scientifically examined. This is also the problem, I presume,—though this is outside my area of knowledge—with the connections between Uralic and Altaic, or with the whole subject of the origin of the Japanese language.

Linguistic taxonomy, as it now appears, reflects the formation and differentiation of languages. It is a classification into families, stocks, and possibly phyla, according to the time depth of their separation.

5. Linguistic typology can be used as a classificatory instrument, both in genealogical taxonomy and in a purely descriptive taxonomy of external features. Halliday (1966) pointed out how arbitrary is the selection of classificatory features in languages among so many variables.

But linguistic typology can be used primarily to describe languages and it is not necessarily connected with taxonomy. Cf. for the different ways of understanding typology Ramat 1976, 8ff.

Partial typologies are successfully employed (s. further No 6): thus there exist typologies of the number and disposition of phonemes, of the pronominals, of certain semantic fields like kinship terms, etc. But what we are considering now is a holistic typology, which intends to present the fundamental features of the language as a whole. This typology, as W. P. Lehmann 1978, 5 says,

is based on the analysis of patterns and principles which have been identified as central in language, such as the structure of simple sentence and its constituents, and processes like government, modification and subordination.

To these, mainly syntactic features, can be added features (like those proposed by Greenberg 1960) which belong to other levels: structure of the word, rate of words and morphemes, classes of bound morphemes, etc., and also some additional syntactic features.

A complete typology is still a desideratum. We should recall the first attempts to establish linguistic types by Adam Smith, the brothers Schlegel and W. von Humboldt, who distinguished the theoretically pure types of analytic and synthetic, isolating, agglutinative, inflective, and incorporating languages. After a period of oblivion, as a consequence of the big successes of comparative linguistics, and especially of the rigorous method of the Neogrammarians (cf. Greenberg 1973, 171), the need for a typology was again formulated by Sapir (1921, chapter VI, on whose connections to the Humboldtian line, s. Christmann 1966). The typological current has been and is now, both in Europe (F. N. Finck, E. Lewy, V. Skalička, B. A. Uspensky, and more recently Hagège 1982) and in America (Greenberg 1960, 1966, 1970, 1973, Lehmann, ed., 1978) recognized as a necessary method. In the universalistic work on languages which is to be expected in the coming years it will be a most convenient tool.

Greenberg has developed practical typological criteria in two directions: on one side quantifying ten indices based mainly on Sapir (Greenberg 1960), and on the other (Greenberg 1966) discovering the long duration and therefore the historical significance of certain word orders in the normal sentence.

Of these two methods proposed by Greenberg, the first has not been much employed, even by himself. This fact leaves it in the category of proposals. We can mention here a few adherents to it: Cowgill 1966, H. Contreras as cited by Cowgill 1966, 140, J. E. Pierce 1966, 1977, Tovar 1966, 1977, 1979a, 1979b, 1981, etc. The second proposal, concerning the basic word order, has been commented on and developed by several authors: Vennemann 1973, 1974, Lehmann 1974, 1978, Li, ed., 1975, Tovar 1978, 1981, etc.

The relative lack of practical application of the first of the two proposals by Greenberg is due perhaps to the fact that the concept "word" (which is employed in nearly every one of his indices) is somehow imprecise. For instance, a postposed article and postposition ordinarily form a unit with the main word in most of the current spellings in languages, whereas a preposition and preposed article are put as separate words in front of the noun, without considering in this case the accentual unit. The same occurs with conjunctions put before and behind. This different treatment of grammatical elements in quite equivalent functions, which in one case are counted as separate words and in the other not, sometimes makes Greenberg's indices unwieldy.

A combination of his quantified typology with that of word order has never been attempted, so far as we know. And certainly a typology can be more useful as more features are considered in it. A total of about fifteen features is just a portion of those which could be used. We are thinking more of the practical application for description and classification than of theoretical typology. The use of computers in a practical typology could develop a larger number of measures (s. a proposal by Pierce 1962).

Lexicostatistic similarities can serve—for languages of less known history—in a first exploration of genealogical connections; such similarities could eventually be confirmed by means of typological similarities (s. for a first attempt on South American languages, Tovar 1966).

6. Typology thus has a double use for the study of the history and relationships among languages.

A holistic typology can reveal tendencies in the development of languages which are observable through a long stretch of time. Such is the case with the drift pointed out by Cowgill (1966, 133) in Indo-European languages, which show

a general rise in agglutination, an early rise in synthesis followed by a decline, and a general decline in the prefix-(and infix-)to-suffix ratio.

Or with the tendency of the same IE languages, as seen by Lehmann (1974), to turn to the use of prepositions and to the order SVO from a previous situa-

tion with postpositions and SOV. Cf. for a contrary development in Chinese, Li & Thompson 1974.

If we consider the insular Celtic languages and their VSO disposition, we could ask whether this is the natural evolution of the Indo-European tendency (SOV→SVO→?) or the result of contact in the British Isles with unknown languages with a VSO order like Hamito-Semitic (cf. Tovar 1979b, 891f.).

Table 2

	Type	Postpositions	AN	GN	DN
Basque	III	+	—	+	—
Irish, Welsh	I	—	—	—	—
English	II	—	+	±	+
Spanish	II	—	—	—	+
Island Carib	I	+ ¹	—	—	—
Guajiro	I	+ ²	—	—	+
Arawak	I	+	—	—	+
Yucuna	II/III	+	+	+	+
Baure ³		+	— ⁴	—	+
Piro	III ⁵	+	—	—	—

1) The postposition is in Island Carib added to a pronominal element which precedes the following substantive: *t-úma t-áufari* 'with her aunt (literally her-with she-aunt)'.

2) Much the same as Island Carib; only pronouns take directly postpositions.

3) Keenan 1978, 284f. includes this language amongst those of VOS order. He has used the same materials as myself, which are scarce and of dubious interpretation.

4) Cf. Keenan 1978, 291.

5) For the features of Piro s. Ultan 1978, 236.

Let us take as an example in this connection the Basque language, which, as is known, is isolated, and can be presented as the only Pre-Indo-European survival in Western Europe. Basque is a (S)OV language with some non consistent features as Table 2 shows: features marked with plus correspond to type III of word order; the Celtic languages of the British Isles have a very consistent type I; in observing that Romance languages are different from the other languages of Europe (all with the order SVO) precisely having a minus in the feature AN (and French has also something of a Celtic feature in *cet homme-ci, cette femme-là*, being nearly a minus in DN), the typologist feels a temptation to believe in the areal influence of West-Europe with a strong "Hamitic" element (s. Tovar 1979b). Let us recall, for this areal factor in Western Europe, that lexicostatistics gives a shared vocabulary of 9.67 and 10.86% for Basque and respectively Rif and Sous dialects of Berber, and 6.59 for Basque and Coptic (Tovar & al. 1961, 259f.). Anyhow Basque is a testimony of the relationship to Northern Africa in primitive Europe, but itself belongs, like Proto-IE, to type

III. The features of Irish and Welsh could represent the consequences of areal influence of type I in Western Europe.

A somewhat similar case can be observed in the most extended family in South America, Arawakan, reaching from the Antilles to the southern part of Mato Grosso and from the eastern slopes of the Andes to the mouth of the Amazon. It shows different types of word order, which are evidently to be taken in connection with areal influences in such an extended and discontinuous area. I have selected a few languages of this family (of which more than a hundred dialects are more or less known). Perhaps type I can be considered as primitive in this family, although postpositions are not consistent within it. But a proof for type I would seem to be its presence, just as in the northern languages, in such remote southern areas as those of Ignaciano and Machiguenga (Bolivia and Peru, respectively in the vicinity of Baure and Piro), according to Keenan 1978, 287.

As shown in Table 2, the three northern languages: Island Carib, as represented in Belize and Honduras, Guajiro in Colombia and Venezuela, and Arawak of Surinam, have a greater similarity (just as a higher percentage of common words). In the central Yucuna (southern Colombia) and in the southern Baure (Bolivia) and Piro (Peru) can be observed more contradictory features, as if areal influences might have altered the original situation. In any case type III of Piro points clearly to the vicinity of the Andean type of Quechua.

The consequence to be drawn from such observations is that instead of a supposedly natural drift in the change in word order, the contact of languages must be considered responsible for it, just as in phonology and loans at all levels.

7. It has been observed that, within certain limits, the quantified indices of Greenberg show some similarity in IE languages for a period of more than one millenium (so in the somewhat parallel cases of Sanskrit→Persian, Anglo-Saxon→English, s. Tovar 1981).

The old typologists already discovered that a long evolution can ultimately change the type of a language. So G. von der Gabelentz (1901, 255ff.) described a spiral, graphically represented by Skalička in 1941 (Skalička 1979, 159f.), in which on the one hand inflection results from agglutination, and agglutination in turn is derived from isolation, whereas on the other hand inflected languages develop in the direction of isolation, as has been observed in English.

"The linguistic cycle" is the title of a paper by C. T. Hodge (1970), in which the results of research in Egyptian language with its long history are presented. For Proto-Hamitic-Semitic a predominantly syntactic (isolating) type is assumed, for Old Egyptian a morphological (inflecting) type, for Late Egyptian again a syntactic one, and for Coptic a morphological one—and thus the longest registered evolution of a single language (more than 3000 years) would confirm the universality of the cyclical phenomenon.

Of course it has been observed (Skalička 1967, 1829) that the speed of type-

logical change is variable, and we would add that this is a consequence of extra-linguistic causes. But the tendency of a language to develop according to inherent trends could be established comparatively, by means of the maintenance of typological features, when lexicostatistic similarities really indicate a "genealogical" relationship.

A second use of typology, as in the comparison of partial sets of features at different linguistic levels, can explain similarities across linguistic family boundaries. U. Weinreich (1953) showed interferences in languages in contact at different levels (phonetic, grammatical, lexical) and for the first time examined the problem of the quantification of interference in terms of the "crystallization of new languages from contact".

To give some examples: partial typologies explain the formation of linguistic areas, as they are known in different parts of the world. Basque and Spanish share the same system of five vowels, and English has shifted from rounded front vowels (which it shared with German) to central ones (also found in Welsh) (Tovar 1978). The general development in western European languages of compounded verbal forms like *I have seen*, *je suis venu* or *amare habeo* is a common inheritance of Greek, mainly via Bible translations (cf. Coseriu 1971). It is probably (if not an inner drift) the early development of Greek to an SVO language that decided such a word order for later Latin and the other European languages, as also for Georgian (Tovar 1979a).

Is it utopic to aspire to an ultimate completely genealogical classification for all languages of the world, even those "without history"? Perhaps lexicostatistic similarities can be checked with holistic typological criteria, to go further from stocks to phyla. On the other hand, coincidences in partial typologies may be useful for the analysis of more or less extensive contact areas. A cautious combination of genealogical classification according to the historical approach with typological comparisons and areal considerations (s. K. H. Schmidt 1977) as applied to North America by J. Sherzer (1973, 1976) could help to create a method as the instrument for universal linguistics which will have to be developed in the coming years.

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12-2

New Directions in Indo-European: Historical Comparative Linguistics and Its Contribution to Typological Studies

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Twenty-five years ago, I attended for the first time an International Congress of Linguists, in Oslo in 1957. Linguistics looks very different now, as we all know; though not the *annus mirabilis*, 1957 was a vintage year. But as a representative of a very old branch of the field I wish to plead for continuity as well as change. Hence my subtitle and its reference to my teacher Roman Jakobson's classic contribution to the Oslo Congress: "Typological studies and their contribution to historical comparative linguistics". That subtitle is not meant to imply a reversal of the relation of things, but to be consistent with the notion of bidirectionality, equipollence, of the scholar who gave us grammar of poetry and poetry of grammar. These lines were written before Jakobson's death on July 18 of this year [1982]; I have now the melancholy duty of inscribing them to his memory.

I have often had occasion to quote the Irish scholar Bergin's wry remark that "no language has changed so much in the last 50 years as Indo-European." Indeed, the last time I quoted it in print was nearly a decade ago, and Indo-European, the grammar of the reconstructed proto-language, has changed quite a bit even since then. This is of course as it should be; a reconstructed proto-language is an artifact, a model, a scientific hypothesis which is always subject to alteration, correction, and improvement in the light of new data or superior analysis.

Let me first try to picture what is new—or at any rate different—in Indo-European studies in the last quarter-century; put another way, from the time I was a graduate student till the present. Thereafter we can turn our attention to what, ideally, we can expect of Indo-European studies in the remainder of this decade, in the 1980's.

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To begin with the obvious, a notable development of the past 25 years has been the expansion of lexicographical tools. Greek offers two new etymological dictionaries to replace antiquated works; two have been begun in Hittite; and Sanskrit now has the first completed etymological dictionary of its history—or in any event the first since Yāska. The final fascicule of the Oxford Latin Dictionary should appear in 1982, and the last one of the Royal Irish Academy's dictionary appeared in 1976 (the first appeared in 1913!). The dictionary of the University of Wales continues apace. Middle Iranian lexicography and

etymology are now firmly grounded, and 1980 saw the first fascicle of the Chicago Hittite dictionary; its completion is envisaged in the decade of the 80's. Other thesaurus-like undertakings are underway in Hittite as well. Mention should be made of the Vedic word-concordance, 16 volumes in all; we are profoundly indebted to our Indian colleagues for this great work.

The flourishing of Indo-European studies over the past quarter-century has naturally resulted in the virtual impossibility of keeping abreast even of selected areas. We are fortunate in having in the Viennese journal *Die Sprache* a semi-annual *Indogermanische Chronik*, now in its 16th year. To this global reference is made; I have systematically avoided giving individual credit to the living scholars of so many nations whose accomplishments are here surveyed. That bibliography maintains an awesome standard both of breadth of coverage and up-to-dateness; we are grateful to our Austrian colleagues. At the same time the proliferation of the fruits of scholarship—the *Chronik* now lists more than 1500 titles a year—constitutes a real problem in the dissemination and retrieval of information which Indo-European studies, no less than other humanistic disciplines, will have to confront squarely in the 1980's.

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Probably the most dramatic development in Indo-European studies of the past quarter-century has been the increase in the data-base; both of the "raw" and of the "cooked", i.e. both in new documents unearthed and in earlier known documents whose philological analysis has been markedly improved. It is worthwhile calling attention to this data-base "explosion" in some detail, for its extent and its implications are not widely known among linguists. I confine myself to those branches of the family (five of the ten major ones) which are significantly attested before the Christian Era: Anatolian, Indo-Iranian, Greek, Italic, and Celtic. It is in these that the documentation and its quality have been most substantially increased, and that typically in the oldest period. The consequences for historical linguistic reconstruction of the proto-language are evident, in that these are the most anciently attested languages of the family, and—*ceteris paribus*—those most likely to exhibit the highest concentration of archaic features, of the "exceptions" on which reconstructions typically rest.

The most momentous increase of the last 25 years is surely to be found in Anatolian, the oldest attested branch of the Indo-European family. (Recent attempts to revive the Indo-Hittite hypothesis have not found a following.) The number of fragments of clay tablets unearthed at Bogazköy and other sites in Turkey has nearly doubled over this period, now totalling over 28,000. The quarter-century has seen the publication in cuneiform autograph of 33 new volumes, almost half of our published corpus of the Hittite language. The work of editing these texts continues: note only the now 26 volumes of the series *Stud. zu den Boğ.-Texten*, since 1965. In Hittite philology the most notable development has been in cuneiform paleography, in the relative dating of

the tablets by their scribal hand. The result is the ongoing 'periodization' of the entire corpus, with enormously important consequences for Hittite historical phonology, morphology, syntax, and lexicon: the grammar must be entirely rewritten. We now know, for example, that the older tablets sometimes noted (by the 'scriptio plena' or doubling of vowels) both vocalic quantity and stress: Hittite can be added to the short list—Vedic, Greek, Balto-Slavic, and indirectly Germanic—of languages to preserve the Indo-European accent. And in the categories of the Hittite verbal system we are on the edge of a major breakthrough, much as with Greek over a hundred years ago.

We are finally in a position to see a real comparative grammar of Anatolian. Languages 25 years ago little more than names now have grammars, dictionaries, edited and printed texts: Palaic, Luvian, Lycian, Lydian. One indeed has these and a new name as well: Hieroglyphic Luvian (formerly Hier. Hittite), due in particular to radically revised readings of certain critical signs (in the form of rewrite rules, e.g., such revisions as $a \rightarrow i$, $i \rightarrow zi$).

All of this, it should be noted, is the work of a handful of scholars, far fewer, I daresay, than have given papers on clitic movement in Romance. There was a time when a good reading knowledge of Sanskrit and considerable experience with the texts was as automatically expected as the competence in Latin and Greek then gained in school. Ferdinand de Saussure could say [unpubl. ms, Harvard Univ. Libr.] "Quoiqu'il m'est arrivé—horresco referens—de lire le Râmâyana d'un bout à l'autre dans le texte original,..." The sheer manpower that could be deployed was in large part responsible for the rapid progress of the linguistic explanation of Sanskrit and, progressing hand in hand, the reconstruction of the proto-language. While today circumstances are different, still some degree of familiarity with Sanskrit is often to be found among linguists who are neither Indo-Europeanists nor Indologists. But how many have done any Hittite? With Hittite and the Anatolian languages, IE studies in the 80's stand on the threshold of a new era. Let us hope there will be the manpower to take advantage of it.

In Indo-Iranian linguistics the keynote in the last quarter-century has been philological exactitude. Always in the forefront in Vedic studies, we can find the same new vigor in Old Iranian studies. This period has seen two new scholarly editions of our oldest text, the Gathas of Zarathustra, with consequences not only for scholarship, but for one of the world religions as well. Reexamination of the Avestan writing system has shown furthermore that the standard transliteration in use for most of the twentieth century is not fully accurate, and obscures linguistically meaningful distinctions; the Avestan of our handbooks must accordingly be corrected. Of far greater potential is the growing understanding of the nature of the manuscript tradition of the various parts of the Avesta, which could lead to a new critical edition of the text to replace Geldner's of 1894. And we may look forward, in this century if not the 80's, to a thoroughgoing revision of Bartholomae's foundation of Old Iranian grammar. Of critical importance will be also the contribution of Middle

Iranian, a burgeoning field of languages and dialects mostly unknown before this century.

It is nearly a century since the publication of William Dwight Whitney's *Roots* in 1885; let us hope that the 80's will see a long overdue revision of that work, as well as the promised volume on the verb of the great Sanskrit grammar begun by Jakob Wackernagel in 1896.

In Greek the period since 1953 has seen the dramatic rise of Mycenaean studies, and their inevitable decline as well, given the nature of a small and closed corpus of texts. If at least linguistically this lode has yielded up most of its nuggets, the history of the Greek language is still incomparably the richer for this accidental glimpse of bronze age literacy. We see the dialectology of the Hellenic world in a new and different light now, and research in this area is very active. The consequences have been far-reaching; it is in recent years primarily from linguists that historians of Ancient Greece have learned that the famous 'Doric Invasion' may be largely a myth. Thanks to intense archeological effort, there has been a steady increase in our documentation of alphabetic Greek epigraphy, particularly in the archaic period. Each new inscription may bring wholly new lexical items, like the *platiwoinos* and his supervisor the *platiwoinarkhos* (cultic officials in 7th century Tiryns), or dialect confirmation of what was previously only reconstructed, like the initial *w-* in the name of Argive Helen in cult (dat. *welenai*). Literary Greek as well has benefited from new papyri and even from new technology, witness the magnificent Cologne Archilochus papyrus, as the editio princeps puts it, 'in Fackelmanns Waschmaschine aus Mumienkartonnage herausgeköcht.'

The conservatism of Greek in phonology has always been remarkable; it is nothing short of astounding, and a rather awesome fact as well, that the exposed and unprotected final short [i] of Modern Greek *périsi* 'last year' has remained unchanged for some 3200 documented years, and in this very word must go back, at least as a morphological variant, to the Proto-Indo-European of perhaps the fifth millenium B.C. It is therefore not surprising that the three distinct 'laryngeal' *h*-like phonemes of Indo-European have left their clearest reflexes in Greek, albeit indirectly. Thanks to the work of a small number of European and American scholars, Greek historical phonology looks very different from what it was 25 years ago.

In Italic, particularly for the archaic period, we have a considerable number of new inscriptions, in a variety of languages, some hitherto unknown. Our documentation of Oscan in the 4th century B.C. is now far better than that of Latin in the same period, and we can now see that the history of the Oscan vowel system was remarkably similar to what happened much later to that of Latin on the way to Proto-Romance. We have now a fragment of "Proto-Umbrian", and tantalizing glimpses of epichoric, quite possibly Indo-European languages of Sicily. These fragmentary stones can provide invaluable grammatical information: to the complex dossier of the 3 pl. perfect ending, the 70's added Venetic *-ers* and Latin *-erai*. Most remarkable is the Ancient Latin

genitive singular *-osio*, with the Indo-European form still intact in a personal name in Latium around 500 B.C. But if this and other inscribed Latin texts have added to our knowledge, one familiar piece must be subtracted: the famous Praenestine fibula, known and loved by every student as our oldest Latin document, has now been shown to be a forgery of the 80's of the last century.

New finds in Spain, France, and Italy have substantially altered our picture of the Continental forms of Celtic. Our documentation is greatly increased: regrettably, our knowledge and understanding of the earliest attested forms of Celtic have not been correspondingly enlarged, for despite many efforts the documents resist translation. It is hoped that further archeological work will reduce the isolation of these challenging texts. Nonetheless I cannot help feeling that if as many translations were attempted of the Old Irish *Bretha Nemed* or the *Privileges and Responsibilities of Poets* as have been attempted of the Gaulish inscription of Chamalières or the Celtiberian of Botorrita, our knowledge of Celtic would be a great deal further advanced. It is the six-volume *Corpus* of manuscript Irish law which stands as the most exacting challenge to Celticists in the 80's, and which doubtless will for many decades to come.

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What then of the reconstructed proto-language, Indo-European itself? Here above all we can observe a very different picture from that to be gathered 25 years ago. If I had to express that difference in one word, I would say 'tighter'. The grammar of Indo-European today is more tightly organized, and more sharply focused, at all levels; impressionistically, there are fewer loose ends, fewer hazy areas, and those that do remain are more clearly identified as such, whether in phonology, morphology, syntax, or semantics.

This is certainly at least in part a function of the climate of linguistic opinion. On the one hand it must be recognized that the "Chomskyan revolution" of generative grammar has had very little direct effect on historical linguistics in general or Indo-European in particular, save in terminology and notational conventions. Though there have been honorable and valiant attempts, I know of no problem in the history of any language that has been solved uniquely by the techniques of generative grammar, as opposed to a more traditional paradigm. The same statement could not, I think, be made about classical structuralism, for example. But we can see today many of the limitations of simple structuralism, and Indo-Europeanists are clearly influenced by the formalism of generative phonology and syntax, and by the quest for rule-governedness, even where that principle scarcely differs from the expressed goals of the neo-grammarians of a century ago.

So now, for example, Proto-Indo-European phonology recognizes a number of purely synchronic rules, whose operation in turn permit a clearer understanding and a cleaner, neater analysis of subsequent developments in the daughter languages. In nominal and verbal morphology we can observe the

same increased attention to 'tightness'; in the domain of morphology, inflexional and derivational, it would appear that generative grammar has far more to learn from Indo-European than it has to impart. But surely an important and direct effect of generative grammar is the renewed interest in syntactic questions in Indo-European studies, in observing and analyzing syntactic change, and in doing syntactic reconstruction. Were an Indo-Europeanist of the 80's tempted to renovate and update Schleicher's famous fable, as many have been (a harmless exercise), he would pay far more attention to the syntax, and find more to change in it, than his predecessors.

It should be stressed, though, that the reconstructed grammar of Proto-Indo-European is in the proverbial state of flux. The new *Indogermanische Grammatik* planned by the late Jerzy Kuryłowicz in the 60's is long in abeyance, and that for the good reason that we are learning too much about that grammar to codify it yet. Both the volumes that appeared in the 60's were really—in the wisdom of hindsight—elaborate working papers. Their effect was to introduce and stimulate discussion in certain areas of the grammar, not to end it. There have been and will be, must be, many more such 'working papers'; only then, perhaps in the 80's but I suspect later, will it be possible to attempt a real, systematic grammar of Proto-Indo-European.

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It is doubtless fair to state that much of the newer, 'tighter' Indo-European is the product of a vigorous international group of what could be described as 'hard-liners'. It has been said that there are two kinds of historian, the 'tough-minded' and the 'tender-minded'; 'hard-liners' would be the former. But this is not the whole story. There have been other important developments and changes in Indo-European studies in the last 25 years, which we may—not entirely facetiously—label the approach of the 'soft-liner', the 'tender-minded'. These involve inter alia a more humanistic view of linguistic phenomena on the one hand—the older ideal of 'philology'—, and on the other an approach more closely linked to the various newer sciences of man.

Generative grammar in recent years has had to be variously tempered by such things as pragmatics and the ethnography of speaking; in this respect general linguistics is merely catching up with Indo-European studies and historical comparative linguistics, which have always kept such things in view. An array of allied fields continue to make their contribution to Indo-European, to shape and define the Indo-European problem: language and society, language and culture; language and law, language and religion, language and mythology; archeology and prehistory of Europe and Eurasia; language, pragmatics, and diachronic behavior; language and literature, poetics and verbal art, oral literature and the formula.

The hard and the soft-liner, the tough and the tender-minded ideally contribute alike; for they share the same concerns, for the best possible documentation and data-base, and for the greatest rigor of methodology. The soft-line approach complements the hard-line in Indo-European studies, it gives flesh

and blood to the bare bones of reconstruction, and above all it provides context which gives meaning to our forms.

Were I to offer a candidate for the single most significant book in Indo-European studies to appear in the last quarter-century, it would be the late Émile Benveniste's *Le vocabulaire des institutions indo-européennes* of 1969. At once provocative and maddeningly imprecise, meditated but put together with uncharacteristic haste, the book is in the words of a perspicuous reviewer a 'synthesis of his views on Indo-European civilization, ideology, and cultural history.' It is also the most complete ethnosemantic description of a culture ever written; and that of a culture which still cannot very accurately be located in either time or space. It is in this book of Benveniste's, in all the areas it touches on and all that it does not, that we can see the real challenge of, and challenge to, Indo-European studies in the 80's.

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In the preceding survey I have tried to give only illustrative details; to indicate the sort of thing that is different from the Indo-European studies of 25 years ago, not to catalogue the differences themselves. But perhaps certain other things may have emerged from the presentation. I have singled out for special mention first the increase in the data-base, in the documentation of the oldest stages of the Indo-European family. I have noted areas of the grammars of specific languages where superior analyses, with or without the aid of new data, have replaced obsolete views, whether of the past century, of the past decade, or of the past year. This has, I hope, given in Indo-European studies a picture of a field alive with excitement, growth, and change; a field where the student of 1982 can be expected to know things that no one in the world knew in 1957. That is of course a commonplace of science, and certainly of most contemporary linguistics.

Yet it is also important—and this is the real point of my subtitle—to look at what has *not* changed in Indo-European studies over the past quarter-century, to observe what is *not* different. The answer is, the method. To put it crudely, the improvements we can observe, and they are numerous and real, have come not from doing something different, but from doing the same thing better. This would not seem to be the claim of most areas of contemporary linguistics. But neither the comparative method nor the other techniques of historical linguistics have changed very much for a long time; nor have the goals. It is in this that we can see the true continuity of historical linguistics. As Indo-Europeanists we admire what we have always admired: breadth and depth of documentation, philological rigor, analytical acumen, and elegance of argument. We can understand and appreciate today the approach of a scholar to a problem a quarter-century ago, or a century or more ago; we can follow the structure of his argument, evaluate it, and accept or refute it. Moreover we can be tolerably certain, in the best of circumstances, that that earlier scholar would understand and appreciate our own arguments of today. In historical comparative linguistics there is a dialogue between the present and the

past, as well as potentially between the present and the future. The language changes, across the generations, but it is the same language.

The importance of typology and the search for universals—and no one doubts its importance—should not blind one to the fact that languages are different; that some are more different than others; that these differences must be examined very carefully, with a method; and that the same rigorous method must be applied to looking at and classifying similarities as classifying differences. Typology and the search for universals is in part a search for an answer to the question, 'how does the human mind work?' Language is the defining characteristic of man; let us not forget that the historical dimension of the study of language is as important as the historical dimension of the study of man. Therein lies the contribution of historical comparative linguistics, to the 1980's and beyond.

The Speaker's Organization of Discourse

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Abstract The report reviews some of our recent work on the speaker's production of discourse. The theoretical framework assumes the existence of relatively autonomous modules in a speaker which interact in producing discourse. Among these modules are those concerning the generation of the message, those responsible for giving linguistic shape to the message, and those involved in articulating the message. Moreover, it is supposed that the speaker disposes of a monitoring device for detecting inappropriate and erroneous materials.

With respect to message generation, a short discussion is presented of our research on linearization, the ways in which a speaker decides on what to say first, what to say next, etc.

The discussion of giving linguistic shape to the message (formulation) is concentrated on a major aspect of human language: its context-dependency. Several studies relating to the production of anaphora, deixis, ellipsis and intonation are reviewed. Also some results are mentioned which suggest the absence of first-order feedback from formulation to message construction.

The discussion of monitoring concentrates on an analysis of a large repair corpus. There is evidence that the well-formedness of repairs is closely related to the well-formedness of coordinations and of question-answer pairs. The three together indicate that through (self-)perception, speakers keep or bring certain procedures in activated state immediately after use.

In a concluding section this notion is expanded as follows: A theory of the speaker requires, apart from the mentioned production modules, the activity of modules for perceptual parsing. The latter's products are then available for "reparametrization", to be (re-)used in creating the next utterance. This may account for both fluency and a variety of context-dependencies in the production of speech.

Introduction

One major characteristic of the past decenium in psycholinguistics is its fancy for textbooks. Introductory texts have been appearing at the rate of about one per year. I am not mentioning this fact in order to suggest that the discipline is apparently coming of age, though many of us might take pleasure in entertaining that thought for a while. Rather, my purpose is to make an observation on the distribution of attention in psycholinguistic research, as it appears from these texts; this on the assumption that, at least collectively, they do reflect the state of affairs in the field. The observation is that Mother

Psycholinguistics has one deprived child: the study of the speaker. The major area's interests are language comprehension and language acquisition; issues of language production in speakers aged over four rather "evoke expressions of skepticism, pessimism and neglect" (Butterworth, 1980).

There is really no good reason why this should be so, and I am optimistic enough to expect major changes for the better in the decade to come. These favorable expectations derive in part from the first experiences with studying language production in normal adults, and in children beyond the initial phases of acquisition at our newly founded Max-Planck-Institute. I am most honoured that I can report on some of this work at the present occasion. Before going into details, however, I should express to you some of the theoretical notions and beliefs that motivate and guide our research.

Theoretical underpinnings

A central theoretical notion is that of *modularity*, i.e. the notion that a speaker's behavior is the resultant of interacting subsystems, called "modules" (cf. Chomsky, 1980). This notion, obviously, applies to any biological or social system, and is rather empty without further specification. It is indeed such specifications which may turn out to be true or false, or to need substantial qualification as research proceeds. Their status is that of working hypotheses, rather than god-given truths.

A first specification is that of *relative autonomy*. We take it that the modules involved in the process of speaking are relatively autonomous. By this we mean that the information exchange between modules is weaker than that within modules (cf. Levelt, 1981a). In other words the partitioning into modules is not arbitrary: the system has natural joints. Any theory of the speaker will have to distinguish between module-specific processes and interactions. A major theoretical task which goes with the distinction of modules is to show the absence or "relative" weakness of interactions between modules. Closely related to relative autonomy is the assumption of *specificity*. This says that modules have their own specific principles of functioning, which cannot be reduced to one another or to general principles of behavior. Though further specifications can be made,¹⁾ these two, relative autonomy and specificity, suffice for the present discussion.

As far as a theory of the speaker is concerned, the existence of relatively autonomous and specific modules would at least take the shape of the following stratification of modules. A first set of modules has to do with *the construction of the message*. These message-level activities involve, among other things, con-

1) One might, for instance, suggest that modules are "hard-wired", or even localizable in the brain, that they are genetically given, etc. We don't want to commit ourselves to any of these here, though a theory of the speaker should eventually have something to say about these issues. We do commit ourselves, however, to a realistic view of modularity as opposed to a merely descriptive one: Description is, of course, never possible without "modularizing" the topic in some way. But such a partitioning needs not be an empirical issue in itself.

ceiving of an intention and a speech act, consulting the perceptual, social or memorial data base, retrieving and ordering information for expression. In short, they deal with what rhetoricians traditionally call invention, memory and arrangement. All of these can be called *conceptual processes*.

A next set of modules is dedicated to *giving linguistic shape to the message* to be expressed. Among the processes involved are the retrieval and the syntactic positioning of lexical items. Garrett (1975) calls this the functional level, which is to be distinguished from the positional level which deals with morphological decisions. Functional and positional levels together we will call *formulating processes*. Arguments of autonomy and specificity have especially been made with respect to formulating—and its equivalent—parsing processes. There is convincing evidence for the existence of dedicated syntactic modules in the organization of human language. Certain properties of syntax seem to be species-specific and they resist reduction to general principles of communicative behavior. Far less recognized and studied is another equally specific property of formulating and parsing, namely *context-dependency*. This is especially observable in linguistic phenomena such as anaphora, ellipsis and deixis, which may or may not be syntactic in character.

Finally, we assume that there are modules which are specifically dedicated to the *articulation* of the generated linguistic entities.

Our working assumptions predict that these levels of processing have their own specific organization and that there is only fairly limited information transmission between levels. More specifically we would expect that there is no first-order, i.e. on-line feedback between levels.

The above mentioned three sets of modules, i.e. those for message construction, formulating, and articulation do not exhaust the processes involved in speaking. There is a rather mysterious but undeniably necessary set of control processes which I will refer to as *monitoring*. Monitoring is involved not only in error detection and repair, but also in the generation of other context-dependent phenomena, such as anaphora and ellipsis. At the present state of knowledge it is impossible to decide whether monitoring should be conceived of as a unified module, or rather as an interaction between modules, and sensitive to context. My inclination is to suppose the latter. The unifying property is that the information flow in the on-going process is compared to and affected by the results of previous processes: monitoring essentially involves remembering, tracking, comparing, and restructuring. The first three of these are also involved in language perception, and perceptual modules may well be involved in monitoring.

In the following I will report on some of our work in the area of message construction, formulating and monitoring. I have no remarks to make on articulation.

Linearization, an aspect of message construction

Of the many facets of message constructions, we have been giving some attention to a particularly neglected one. It was what rhetoricians have called *dispositio* or arrangement. If a speaker wants to express anything more than the most simple assertions, requests, commands, etc, he or she has to solve what I have called the *linearization problem*: the speaker will have to decide what to say first, what to say next, and so on. There is virtually nothing in the psycholinguistic literature relating to this issue, in spite of the fact that it should figure centrally in any theory of the speaker.

It appears from our work (Levelt, 1981b, 1982a, b, Ullmer-Ehrich, 1981) that there are two sets of principles determining the speaker's arrangement decisions. The first set of principles relates to the content of the information expressed. The central notion here is that of *natural order*. Certain domains of discourse have their own natural order, and the arrangement for expression follows this order. The clearest example is the domain of event descriptions: the natural way of describing events is to follow their temporal sequence. It is, for instance, experienced as odd to say (1) (from Kempson, 1975):

- (1) The Lone Ranger rode off into the sunset. He mounted his horse.

This preference for chronological order in the description of events appears very early in children (cf. Clark, 1973). Clark argues that it is only later that children develop awareness of thematicity in discourse which may bring them to produce and accept utterances in which the thematic event is mentioned in first position whatever its chronological position. Karmiloff-Smith (1979) discusses how children proceed through an intermediate stage in order to cope with thematic deviation of natural order in event descriptions, by producing utterances such as (2):

- (2) The boy went upstairs, and before the boy went upstairs, the girl washed the boy.

Natural ordering can also be observed in the domain of spatial discourse, for instance in route direction. Klein's research (1979, 1982) at our institute shows that speakers invariably follow the shortest or easiest route from starting point to goal, though there is no logical necessity to do so. Again, children show the same pattern rather early (Weissenborn, 1980). Similarly natural orderings may exist for other structures of relations, such as in planning discourse (Linde & Goguen, 1976), tutorial discourse (see Collins et al., 1975 for geography), argument structures (Miller & Klein, 1981), etc. It should be noted, however, that natural order is a cultural concept. It relates to mutual knowledge of speakers and listeners in a language community. There is a set of tacit assumptions about the canonical structure of things: in Western culture causes precede effects, intentions precede decisions, etc. But tacit assumptions may vary from culture to culture, and what is a natural order in one place of the

world may be quite unnatural somewhere else.

Given, however, a range of common knowledge and stereotypes in a culture, speakers' arrangement according to natural order has two obvious psychological motivations. The first is that it allows for easy retrieval on the part of the speaker. If consecutive events are more closely associated in the speaker's memory than non-consecutive ones, retrieval is easier if it follows the order of events, and similarly for other domains of discourse. The second is that it makes the listener's job easier if a stereotypical or "canonical" order is used: the listener can then use the same cultural stereotype or canonical form to infer an informational structure from the speaker's discourse. Arranging according to natural order facilitates retrieval on the part of the speaker, and inference on the part of the hearer. So far for natural order as a determinant of linearization.

The other set of linearization principles derives from process-requirements in the expression of complex informational structures. A speaker has to keep track of what he has said and what is still to be expressed. These book-keeping requirements impose restrictions on the linearization process which are quite general, i.e. not domain-specific, and which do not seem to be very culture-dependent. An extensive series of experiments in our institute has established three such principles of linearization (Levelt, 1981b, 1982b, Ullmer-Ehrich & Koster, 1983):

The first principle is the *principle of connectivity*: a speaker introduces new items as much as possible on the basis of an existing connection to the last mentioned item. This could easily be shown in experiments where subjects had to describe spatial patterns such as those in Figure 1. If a subject is asked to describe Figure 1(a) starting at the arrow, he will proceed in a connected fashion, following the arcs of the figure. But also in describing from memory subjects follow the informational structure as much as possible in a connected fashion using a just mentioned item as retrieval cue for a closely connected following item. This appears from the classical experiments on apartment descriptions by Linde and Labov (1975), as well as from our own experiments on how people describe living-rooms (Ullmer-Ehrich, 1982, Ullmer-Ehrich & Koster, 1983).

The second principle concerns the order in which speakers deal with multiply connected nodes in the informational structure. This is called the *first-in-last-out principle*. An example suffices to demonstrate it. If a subject has started the description of Figure 1(b) by going in a connected way from pink to green to black to yellow to brown, he will then have two choice nodes, the green and the black one, waiting for completion. The principle predicts that the last unfinished choice node, i.e. the black one, will be returned to first: from brown the speaker will return to black and describe the arc to red before returning to green and completing the description of the branch white-orange-purple-gray. In the theoretical model we constructed for the speaker's linearization, there is a push-down storage for keeping track of return addresses.

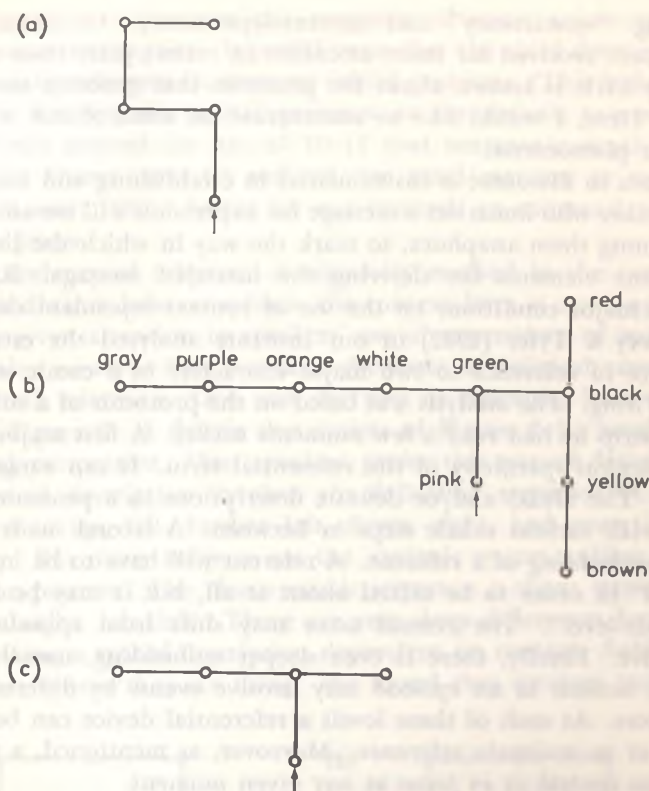


Figure 1 Patterns to be described by subjects in linearization experiments. Nodes in the patterns are differently colored.

Such a store implements in a natural way the first-in-last-out principle.

The third principle is called the *principle of minimal effort*. It predicts that the speaker's linearization will be scheduled in such a way as to minimize the load on the push-down store—i.e. the number of return addresses. Several predictions can be derived from this principle. One is that a subject will prefer to describe the left-branch of Figure 1(b) before the right branch: in that way he will never have more than one return address on store. Also, shorter branches will be described before longer branches, i.e. the right one before the left one in Figure 1(c): this will minimize the duration of storage for a return address.

Together these process-principles of linearization predict that discourse structures whether spatial or otherwise will tend to be right-branching.

Context-dependency, an aspect of formulating

Earlier I mentioned two claims of module specificity for the level of

formulating: "syntacticity" and context-dependency. Of those, the syntactic processes have received far more attention in recent years than context-dependency. Very little is known about the processes that generate anaphora, ellipsis, or deixis. Here, I would like to concentrate on some of our work relating to these latter phenomena.

Anaphora in discourse is instrumental in establishing and maintaining reference. A speaker who linearizes a message for expression will use context-dependent devices, among them anaphora, to mark the way in which the listener will have to recombine elements for deriving the intended message. Recoverability is one of the major conditions on the use of context-dependent devices. Marslen-Wilson, Levy & Tyler (1982) in our institute analyzed the establishment and maintenance of reference to two major characters in a comic strip, *The Hulk* and *The Thing*. The analysis was based on the protocols of a subject describing the comic strip he had read a few moments earlier. A first major variable is the degree of *lexical specificity* of the referential term. It can range from the full noun (e.g. The Hulk) and/or definite descriptions to a pronoun (he) to a zero anaphor, with various subtle steps in between. A second main variable is the *degree of embedding* of a referent. A referent will have to be introduced at the "story-level" in order to be talked about at all, but it may be reintroduced at the "episode-level". The central actor may shift from episode to episode in the narrative. Finally, there is even deeper embedding, namely at the event-level. The actions in an episode may involve events by different more or less central actors. At each of these levels a referential device can be used to either *introduce* or to *maintain* reference. Moreover, as mentioned, a referent can be more or less central or *in focus* at any given moment.

In their analysis Marslen-Wilson et al. related the lexical specificity to degree of embedding, as well as to introducing, maintaining and focussing a referent. It turns out that the type of referential device used is very closely tied to the narrative and informational contexts in which it occurs. At the one extreme, introducing a referent at the story level involves giving a name and a definite description. At the other extreme, maintaining a referent at the event-level is either done by means of a pronoun or a zero anaphor. The gradations in between are very finely turned in ways for which I must refer you to the published paper.

The acquisition of these context-dependent devices is surprisingly slow, but follows a regular path. Both Hickmann (1981) and Karmiloff-Smith (1981) of our institute argue that deictic use precedes anaphoric use of articles and pronouns in children. Karmiloff-Smith (op cit) in her study of story telling in English and French speaking children aged 4-9, found that four-year olds held narratives together by spatial deixis (in front of the picture book), and by frequent paralinguistic gestures. There was no trace of anaphora in the use of pronouns. Subsequently, around the age of 6, the first type of anaphora appeared. This was invariably using a pronoun in the initial slot of utterances to indicate the thematic subject. This often required the child to adapt the verb

in case the thematic subject was the recipient of some action (*give*→*take*), leading to characteristic repairs. It is still later that the child develops the skill to put non-thematic referents in sentence initial positions. This is first done by using full NP's, keeping the initial pronoun as the default case of thematic subject. It is only around the age of 10-11 that sentence-internal constraints on anaphora are always correctly realized, for good measure so to say. (But see Deutsch & Koster, 1982). So far for our research on context-dependency in the uses of anaphora.

Context-dependency of reference is especially marked in the use of deictic expressions. As distinct from anaphora, the context here is non-linguistic. We have especially given attention to speakers' use of expressions of spatial deixis. One of our major concerns here has been their use of deictical versus intrinsic systems of spatial reference. One can, by way of example, look at patterns (a) and (b) in Figure 2. A deictic description of Figure 2 (a) would take the speaker's spatial perspective. Most speakers prefer this way of describing. The canonical deictical description involves the following sequence of directional terms: up, further up, left, further left, down, right, and *mutatis mutandis* for Figure 2(b). But many subjects use the intrinsic system, taking perspective from within the pattern. The characteristic sequence is then: straight, further straight, left, straight, left, left. There are very deep differences between these two ways of making a spatial description dependent on context. Subjects hardly ever mark which system they are using. We found that speakers tacitly assume

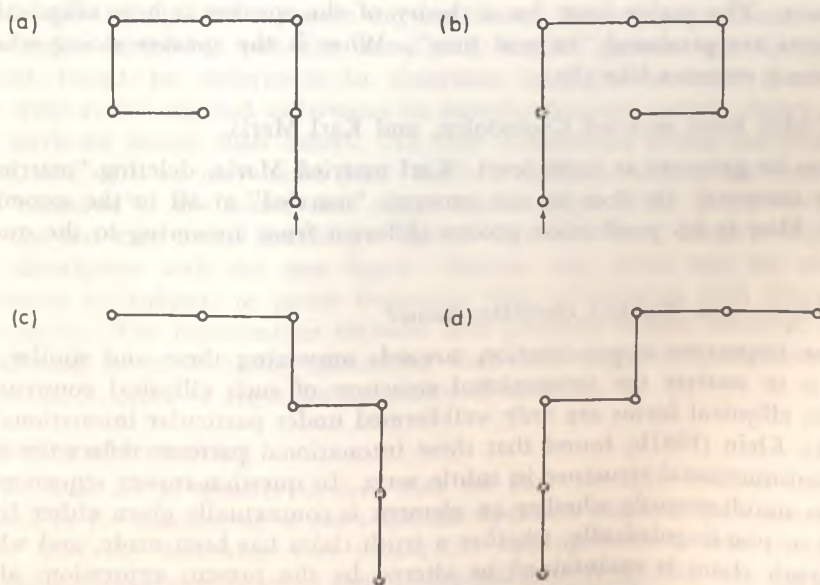


Figure 2 Intrinsic descriptions of patterns (a) and (b) are interpreted deictically in the drawings of listeners (c) and (d).

the listener to use the deictic system. If the speaker uses the intrinsic system, the listener takes it to be deictic: Figure 2(c) and (d) give the drawings subjects make on the basis of intrinsic descriptions of the patterns (a) and (b). The disparity makes one doubt whether language was designed for communication.

A major psychological difference between the deictic and the intrinsic systems is that the deictic one can *always* be used: it is the default system which does not require a deep understanding or interpretation of the spatial relations and of the character of the objects involved. The intrinsic system, however, is based on functional and spatial relations between the objects themselves, such as desks and chairs having fronts and backs. Research in our institute by Ullmer-Ehrich and Koster (1983) shows that speakers' shift between the systems is dependent on the strength of spatio-functional relations between the objects involved. These authors asked subjects to describe arrangements of furniture in a doll-house. Strong spatio-functional relations, such as between a table and a juxtaposed chair lead easier to intrinsic descriptions than when the same objects are put apart in different places of the room or when unrelated objects are juxtaposed.

Context-dependency is not only clearly observed in the cases mentioned above, but also in a large variety of elliptical constructions. Klein (1981b) made extensive analyses of the rules governing ellipsis in German. The range of possibilities for ellipsis is of a different order of magnitude than one might expect on the basis of the attention given to gapping alone in the linguistic literature. The major issue for a theory of the speaker is how elliptical constructions are produced "in real time". What is the speaker doing when he produces a sentence like (3):

(3) Today Peter married Gwendolen, and Karl Maria.

Does he generate at some level "Karl married Maria, deleting "married" at a later moment? Or does he not generate "married" at all in the second conjunct? How is his production process different from answering to the question (4):

(4) What's von Weber's christian name?

One important approximation towards answering these and similar questions is to analyze the intonational structure of such elliptical constructions. Certain elliptical forms are only well-formed under particular intonational constraints. Klein (1981b) found that these intonational patterns reflect the underlying informational structure in subtle ways. In question-answer sequences they express simultaneously whether an element is contextually given either linguistically or non-linguistically, whether a truth claim has been made, and whether that truth claim is maintained or altered by the present expression; all this works out in interaction with where the slot in which the finite element is going to appear in the sentence.

Experimental work by Kelter and myself (Levelt & Kelter, 1982) has made it clear that certain normally arising correspondences in wording between questions and answers are not realized any more, if the speaker's working memory is filled with additional information. Conversational analysts have observed that words or turns of phrase, once used, easily reappear in a subsequent phrase or turn (see the present sentence). The experimental work suggests that formulating or parsing procedures stay in an active state for some time after their use. The speaker can employ this available information both for regenerating particular surface forms, or for deciding on what *not* to regenerate (in case of ellipsis).

Context-dependency, especially if the context is linguistic, logically requires storage of earlier information. It is of paramount importance for a theory of the speaker to find out which aspects of self-produced speech, and which aspects of the interlocutor's speech are stored at all, and which conditions of delay hold for the different types of information. Very similar issues will come up in the discussion of monitoring. Before turning to that I would like to make one remark on interaction between modules.

As noted earlier, our working assumption is that feedback between modules is minimal. In one series of experiments (Levelt & Maassen, 1981) we have tried to establish first-order feedback from the formulating level to the message level. More specifically we studied whether variation in the case of lexical retrieval would affect linearization, i.e. the order in which units of information are expressed. We asked subjects to describe two simultaneous events, for instance a circle moving up and a square moving down. In one experiment we had performed a detection task for these and similar geometrical figures, and we had found no differences in detection latencies between them. But there were rather marked differences in *naming* latency: certain figure names were retrieved slower than others. We thus constructed events involving two figures, one with an easy name (like "circle") and one with a less easy name (like "diamond"). We found that subjects who began their event description with the less easy name had longer speech latencies than subjects beginning their description with the easy figure. Despite this, there was no tendency whatsoever for subjects to prefer beginning their description with the easy-to-name figure. The linearization decision thus precedes lexical retrieval, and is not affected by it. It is my contention that such negative cases have to be established in order to argue for autonomy of modules.

Monitoring and repairing

The final set of speaker devices that we have recently studied are those involved in the monitoring of one's speech. Speakers monitor their own speech for at least the following two properties: appropriateness and correctness. Appropriateness always involves context-dependency. A speaker may use a word which on one reading is correct, but the context also allows for another reading. Also the referent of a particular anaphoric element may be underspecified in

the available context, or a term used may not be exactly of the right level (too informal, too imprecise, etc.) in the current communicative context. The speaker will also monitor for correctness: if he intended to say "blue", but says "green" instead, the word is incorrect, not inappropriate. In addition syntactic or phonological errors may be made which need correction.

We have analyzed a corpus in which over 900 taped repairs appeared. They all concerned spatial descriptions of patterns like those in Figures 1 and 2. The two types of monitoring are reflected in rather different patterns of repair. We found evidence that the speaker stops speaking as soon as trouble has been detected (cf Levelt 1983 for an extensive technical report on this work). Normally, he or she will not try to complete a linguistic unit of one sort or another. This often leads to interruption so rapidly that it occurs *within* the incorrect word ("verti- eh horizontal"). However, this hardly ever happens within trouble words that are merely inappropriate, but correct: interruption occurs at the earliest right after such an inappropriate word. Still, interruption often does not occur till several words after the trouble item. We could attribute this to a delayed *detection* of trouble, and we found that the detection chance sharply increases towards the ends of constituents. This suggests the existence of a "trading of attention" between processes of production and of self-perception.

The two types of monitoring, i.e. for correctness and for appropriateness, are also reflected in the type of editing term used. Appropriateness trouble often leads to the Dutch equivalent of *or* (*of*) and *rather* (*dus*), whereas incorrectness leads to *no* (*nee*), or to excuses such as *sorry* (in Dutch!).

Though interruption can take place at almost any moment during speech, the repair itself can only start at specific places, either at the trouble spot itself, or earlier. These places are often constituent boundaries, but this is not necessarily so. There is another, and deeper rule that governs the speaker's retracing. Repairs can in general be easily classified as well-formed or non-well-formed. A repair like (5) is well-formed:

5) Are you coming, eh going tomorrow?

Non-well-formed, however, would be (6):

6) *Are you coming, eh you going tomorrow?

We have found that, with some explainable exceptions, a repair is well-formed in just the case that a corresponding coordinate construction is well-formed. The correspondence rule is given in Levelt (1983). The corresponding coordinate constructions to the just mentioned repairs are (7) and (8), respectively.

7) Are you coming today and going tomorrow?

8) *Are you coming today and you going tomorrow?

The existence of such a well-formedness connection between repairs and coordinations suggests that they share certain processes of generation. In both

cases the speaker has committed himself to expressing the completion of certain linguistic constructions. The study of ellipsis and the study of repair are now converging on the same issue that was mentioned above: the speaker keeps certain procedures in a prolonged state of activation; and this state of activation determines not only what is or is not a possible coordinate to an earlier clause or phrase, but also what is a possible repair or point of continuation after interruption of speech. A major issue, then, is where this state of activation comes from, or in other words: which modules are involved?

The role of perceptual processes in speaking

In this report I have concentrated on our efforts to develop a modular theory of the speaker. A main objective for such a theory is to explain the different forms of context-dependency in the speaker's discourse without taking recourse to postulating a wide variety of interactions between modules.

Especially complicated is the precise place of monitoring in such a theory. Traditionally, monitoring is taken to be a special purpose mechanism, designed for dealing with "trouble" in the generation of speech (Laver, 1973, Schegloff et al., 1977). One should, however, question whether it is correct to carve up the system in such a way as to postulate a relatively autonomous module for "de-bugging". The structural similarities between repairing, coordinating and question-answering are so apparent that a different approach seems warranted. In order to explain the highly similar forms of context-dependency arising in these cases, it may be helpful to attribute an essential role to *perceptual* procedures in the formulating process. Earlier we reported evidence that formulating and parsing procedures, once used, may stay in an active state for some time following that use. This may be, in part, due to (self-) perception. Further operations might then apply to these perceptually activated or re-activated procedures by changing their parameters in appropriate ways. The thus readjusted procedures can then be re-applied, producing output which is both similar and dissimilar to what was said earlier, dependent on the re-parametrization applied. They may turn a full clause into an elliptical coordinate clause, a full NP into a pronoun, an erroneous clause into a repair, a question into an answer, etc. By re-using the results of parsing in this way, the speaker can simultaneously realize fluency, and cohesion with previous discourse. Another argument for the postulation of such a level of "re-parametrization" is that these forms of cohesion and context-dependency develop only slowly in childhood: their acquisition is not complete till long after acquisition of basic syntax and morphology.

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Psycholinguistics: Historical Aspects, Methodological Problems and Selected Topics in the Field of Language Acquisition and Multilingualism

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My report consists of three parts. The first deals with some historical and methodological problems of psycholinguistics, the second and third discuss selected topics in the field of language acquisition and multilingualism.

1. On History and Methodology

Though there has been collaboration between linguistics and psychology since the pioneer of experimental psychology Wilhelm Wundt, it was not before the 1950s that psycholinguistics as an interdisciplinary term emerged. It has been dated from 1953, the Indiana University Summer Seminar (Osgood/Sebeok 1965).

What does *psycholinguistics* (PL) refer to? For the founders in the 1950s it referred to one of the disciplines studying human communication which "deals directly with the processes of decoding and encoding as they relate states of messages to states of communicators." (Osgood/Sebeok 1965:4). In the sixties and seventies the American definitions can be seen as paraphrases of this with slight modifications. PL was supposed to deal "with psychological processes which contribute to the acquisition, production and understanding of language" (Fodor/Jenkins/Saporta 1967:161). It was seen as "the investigation of the nature of language performance in distinction to the formal study of language as an abstract system" (Blumenthal 1974:1105), or "the discipline studying the processes involved in the production and comprehension of utterances and with the acquisition and deterioration of these processes" (1974:1071). In the European field where psychology of language (in German *Sprachpsychologie*, *Psychologie der Sprache*) was established much earlier (cf. also Bühler 1934), *psycholinguistics* has been used also to encompass sectors from this field (cf. Leontiev 1971). In the first edition of his "Psychologie der Sprache" Hörmann equates *Sprachpsychologie* with *Psycholinguistik*, in the second (1977:8) he notices that there is a "Unbehagen am Stand der Psycholinguistik" and a development towards the broader *Sprachpsychologie*.

These definitions may be sufficient to illustrate the problem of delineating a scientific area by definitions. More important than definitions, however, is the content of and activity within the area outlined. They have changed during the round three decades not only according to the development and changed perspectives of the two basic disciplines psychology and linguistics, but also

through the development of new trends out of this field, resulting in new disciplines such as *neurolinguistics*, *patholinguistics*, *paedolinguistics*, to mention only a few. That there seems to have been a need for intrafield differentiations, too, can be seen in the categories such as *theoretical PL*, *applied PL*, or *developmental PL*. Surveys and discussions on major trends in research can be found in Osgood/Sebeok (1965), Slobin (1970), Fillenbaum (1971), Leuninger/Miller/Müller (1972), Rubenstein (1974), Engelkamp (1974), Prucha (1974), Clark/Clark (1977), Oksaar (1977), Hörmann (1977), List (1978) and (1981), Gauger (1980), to mention only a few which have been chosen on the basis of their variety of references and approaches, though not a few overlappings can be also noticed. The readers of Rosenberg (1965), Rosenberg/Koplin (1968), Flores D'Arcais/Levelt (1970), Slama-Cazacu (1972) and Halbe (1976) also provide orientations on contradictory opinions and approaches.

On the level of interdisciplinary work the basic problems which psycholinguistics shares with other inter- and transdisciplinary disciplines is the danger that the other one is always considered as a more homogeneous one than it is. The influence of the generative transformational grammar is only one well known example.

According to Brown (1970:VII) the original aim of psycholinguistics—the importation of linguistic science into psychology—has not been fulfilled, because, as he states, linguistics has “solved very few practical problems.” Psycholinguistics “has penetrated to the hard truth that a dilettante interest in another field is not enough to support interdisciplinary work” (1970:IX). These statements make a basic terminological problem obvious: what is meant by “linguistics”? One has to take into account when, by whom and where a statement is made. In the writings of the transformationalists, for example, “‘syntactic’ becomes ‘linguistic’” (Robinson 1973:28). There is no doubt that those psycholinguists, and their opportunistic followers, who identified PL with generative transformational linguistics and, as we know, have been eager to claim to be the only representatives of the field, have hindered the real interdisciplinary development of the discipline, planned with so much enthusiasm and pluralistic possibilities in the 1950's. Another problem which has influenced the development of the field is implicated in the question of Blumenthal (1974:1105) of why most contemporary psycholinguists are unaware of the earlier periods of collaboration between psychologists and linguists. One could, of course, add a synchronic view, too: why are so many psycholinguists unaware or ignorant of what those of other psycholinguistic schools are doing? Rubenstein's (1974) overview—to give only one example—refers almost exclusively to the American scene, and—in two other examples—in the empirist/nativist controversy of the sixties and seventies, hardly anyone responded to William Stern's convergence theory. Stern had already presented this theory as uniting the standpoints of the two schools in 1914 in his “Psychologie der frühen Kindheit” (Psychology of early childhood). In the discussion of the development of language and thought, Piaget and Vygotski are always presented as opposites in survey works.

It is overlooked that Vygotski himself follows Stern/Stern (1928) in his opinion that the child's language is social from the very beginning and credits Stern with having proved how the development curves of thought and language coincide to Stern (more exhaustive discussion of this can be found in Oksaar 1977, Chapter 3.3).

Concerning the methodology of PL throughout its history—from the activities during the period of the influence of Bloomfieldian behaviourism via Chomskyan antibehaviourism with its shift to a cognitive view to the current broader field, which is oriented more on the functions of language—the following points are conspicuous:

- 1) Up to the seventies, necessary sociocultural aspects have generally been ignored in the methodological discussion of the field, even though psycholinguists must also have known that language exists and develops in a biological and sociocultural context and is the chief means of human communication. This ignorance is partly due to the Chomskyan concept of an ideal speaker/hearer in a homogeneous society. The real speaker/hearer uses language in a heterogeneous society in social contexts. The exploration of the use of language and the question of language change, the emergence and choice of alternative forms, for example, in cases of up and down grading, demands that PL take into account the interaction of language and social structure. (Oksaar 1975b, 1976)

- 2) PL approaches language from the point of view of monolingualism (so does linguistic theory), supposing this to be the normal state; bilingualism has long been seen as a problem by many researchers (cf. the discussion in Oksaar 1972). There are, however, far more bilingual people in the world than monolingual, and the majority of those who may be classified as monolinguals, in the interlingual perspective, usually have command of more than one dialect and/or sociolect.

- 3) PL approaches spoken language mainly from the verbal elements, without evaluating the possible simultaneous information from the paralinguistic and kinesic elements.

- 4) The question of universals has not seldom been discussed on the basis of only a few languages.

- 5) When dealing with semantics, PL is concerned chiefly with denotations, neglecting connotations, even though techniques such as Osgood's semantic differential have been available and critically modified, cf. Oksaar 1975b.

- 6) The consequences of the "intimate relationship between the observer and the observed" (Osgood/Sebeok 1965:IX) have, inspite of renewed awareness of the role of the observer, hardly been included in methodological considerations.

- 7) The use of termini lacking in uniformity. What is already *theory* for members of one school, is only a *hypothesis* for others (see Waterhouse 1980, Oksaar 1977).

The current state may generally be described as follows. PL has to orientate itself on the functionalistic-communicative trends of linguistics: its paradigmatic change in the seventies from sentences to texts, from the exclusive research of

the system to the analysis of speech in action. Today, dialogue-linguistics has become more than just a working name. Through the pragmatical orientation of linguistics, growing interest in conversation analysis (Schegloff, Sacks, Jefferson and others) can be noticed, as well as in the sociolinguistic approaches of the sixties: the correlational (Labov, Bernstein) and interactional approach (Hymes and Gumperz) which have both focused attention on linguistic varieties as well as the importance of research on language change in its social context.

2. On Language Acquisition

Child language research has always been guided by the linguistic, psychological and philosophic theories which dominated at the time. That the paradigmatic change in the seventies just mentioned has influenced the theoretical and methodological viewpoints of child language researchers and their change of focus was already evident at the Third International Child Language Symposium 1975 in London. Waterson/Snow (1978:XXI), in their volume of selected papers of this Symposium, mention the following trends: "1. Growing interest in the semantic and pragmatic components of children's linguistic competence, replacing the earlier concentration on syntactic competence. 2. Increasing recognition that language is intrinsically communicative and that the acquisition is dependent upon a social-communicative context. 3. Growing awareness that language acquisition can not be understood without relating it to the concurrent cognitive development of the child. 4. A recognition of the importance of perceptual processing in language acquisition. 5. A reinterpretation of the nature of the structure for language acquisition."

This important shift of interest does not mean that all previous research areas are unimportant. However, it changes the perspective of the approach and in stressing that the communicative function of language is primary, reflects the need to place the research of language structure into the broader context of social action. The tendency was also prevalent on the Congresses in Tokyo, see Ingram/Peng/Dale (1980), Ingram/Dale (1981) and on the Congress in Vancouver 1981. The present time has, under changed methodological and theoretical conditions, found its way back, not only to the important questions of semantics, cognitive development and language change of the early child language research, but also to questions based on the social aspects of language acquisition, which were current in the first decades of the century. (Clara and William Stern, Charlotte Bühler, Luria, Vygotski).

Concerning the cognitive aspects of language acquisition (LA), it is necessary to observe the child's cognitive behaviour already from the beginning with regard to his social contacts and communicative possibilities. In the post war period, Piaget's well-known thesis that the child's speech and thought are egocentric has, beyond any doubt, slowed the systematic investigation of these aspects. Another reason is the "identification of cognitive activity almost exclusively with the development of schemes about material objects in space rather than schemes about persons acting in time" (for a discussion of this,

see Oksaar 1977, Chapter 3.3). It is also necessary to answer the question about which aspects of language can be seen as an index of cognitive development (Dale/Cook/Goldstein 1981:152). In order to know more about the relations between the linguistic and cognitive development, we need systematic cross-linguistic research of mono- and multilingual children's language behaviour in the frame of their total behaviour. The possible contribution of this research to the Sapir-Whorf Hypothesis of linguistic relativity must also be mentioned. The discussion of the hypothesis has in general excluded sociolinguistic aspects, such as questions of socially significant linguistic variation and stability; nor has it sufficiently considered the influence of social structure and social change on the language and the various behaviour norms of its speakers. Also systematic observations of the linguistic behaviour of multilingual people—adults and children—must be undertaken to test this hypothesis. The LA process offers good examples for the manner in which language can affect the reflection of reality and the behaviour of children and how language and culture interpenetrate (analysis of various empirical data in Oksaar 1977, 1981).

Linguistic and pragmatic awareness in children is a further area, which is almost neglected in this field, though the question of when a child develops a "feeling" for grammatic and semantic structures belongs to one of the central problems of LA-research. What connection is there between the development of consciousness of language, the metalinguistic abilities and the development of other cognitive abilities? This question belongs to those waiting for an answer. Language awareness has been taken as fundamental in much recent linguistic work, but it has become clear that it "is an ability that must be developed in its own right, and whose parameters of use and development can be studied psychologically" (Dale/Ingram 1981:257). In Oksaar (1977, 1981) developmental aspects in this field are discussed in the light of earlier and current research, see also Ochs/Schiefelin in (1979). The Hamburg longitudinal LA-project (mono-, bi-, tri-, and quadrilingual preschoolers, N-30, languages: Estonian, Swedish, German, English) shows that children seem first of all to be aware of lexico-semantic regularities—at the age of 3-4 years they responded to deviations from the rules of semantic congruence and choice of lexical items. Multilingual children that age had a certain awareness of differences between languages. They seem to develop early a comparative awareness of the functions of language as a tool, which presupposes analytical abilities, and manifests itself in functional code switching. Recent studies (Weeks, Berko Gleason, Sachs/Devin, Shields) have also shown that preschoolers very early have an awareness for partner related language variation and can acquire the ability to judge a situation in which variation could occur; children under 3 are able to carry on continuous dialogue. Even very young children are able to "derive inferences from social contexts about the attitudes, abilities, intentions... of conversational participants" (Reeder 1981:149). This happens in a period that Piaget classified as the preoperational stage (2 to 6 or 7). According to him, thinking is, at this stage, egocentric, static and irreversible. They can-

not act according to various roles. It seems that such a characterization of children between the ages of 2 and 6 or 7 does not cover what a child really achieves in linguistic and pragmatic awareness.

Why do we use the term *paedolinguistics* for language acquisition research at the preschool age? An increasing amount of research has shown that the child communicates with its surrounding (mother, father, nurse) before it utters the first word (Bullock 1979). This calls attention to the inclusion of paralinguistic and kinesic elements in the research paradigm of spoken language. The fact that from the very beginning the child has to interact with and react to his surroundings shows that he must develop a competence to use his speech according to the communicative functions of his utterances and the norms of the situation. "Developmental psycholinguistics", as "native language acquisition" has been called (Dato 1975:IX), has not been able to cover the necessary broad field of this research. The necessity of a broader approach is also supported by the fact the investigation of language development is a necessary prerequisite if we want to understand man as a biological, social, cultural and intellectual being. Language acquisition and use by the preschool child should, therefore, not be treated from isolated psychological and/or sociological points of view but from the *paedolinguistic* one. *Paedolinguistics* (Oksa 1975a, 1977) studies LA from prenatal to school age from an interdisciplinary approach which views it as a complex of biological, psychological and sociocultural phenomena. The upper limit is placed at elementary school age (6 to 7 years) based first of all on the fact that, until then one language type, the *spoken* language is the child's only linguistic means.

Methodological questions. Among not a few methodological problems in this field we discuss the following:

- 1) The *tertium comparationis* of the developing language of the child and data collection. For an explanation of the development of child language one needs an analysis which shows how one stage develops from another. This presupposes a LA theory which has not yet been conceived, although attempts have been made, see Schlesinger (1972). However, for just such a theory it is important to differentiate clearly between adult language, which always represents a whole group, and the individual language of a small child, which cannot be traced back to a language system as can data from adults. Since the goal of language acquisition is adult language, the corpora from children must be designed in such a way that they can help determine the dynamics of the development and allow the foundation of hypotheses about the process of change. It is not possible to follow development without continuous observation, however, there has been no basic discussion of verbal data collection among LA researchers, much less of the problem of which observations of nonverbal behaviour must be procured. Though researchers use different techniques for their data collection, their results are not seldom compared. Experimental tasks and spontaneous language samples, however, are both not free of the danger of confusing the conclusions with the conditions leading up

to them.

2) Also the metalevel of description has been neglected, as child language has been described by methods based upon categories and units of adult language. It is methodologically not correct to analyse the child's *spoken* language with the apparatus of the adult grammar based on *written* language. This runs the risk of disguising the proper relations rather than elucidating them. This does not mean, however, that one could simply use adult grammars of the spoken language (if there were any) as datum for child language. One should rather ask how the system of the child's language at a certain point is formed and how it changes.

3) In looking for the means and strategies used by the child in order to use language as an effective means of communication there is the same methodological danger in applying *functional* categories. Halliday's (1975:21) system of six functions (Instrumental, Regulatory, Interactional, Personal, Heuristic, and Imaginative), which have much in common with Jacobson's well-known functions, have been questioned whether they "really give as much insight into how language functions for the child, as they do into the categorising principles of the linguist". (Shields 1980:275). The presentation of the adult classification of the child's "repertory in network form", obscures according to Shields "the actual options exercised by the child by the intervening insertion of a web of adult classification material". One can also question if the early utterances of the child serve only one function as Halliday states and if one should not, instead of this, proceed hypothetically from a functional hierarchy in the sense of Jacobson (1960).

4) Speculations. In a review of 220 papers on child language published between 1975 and 1978, Waterhouse (1980:643) found "the following examples of speculative conclusions: 1) Generalizing universal principles from data from one child, 2) discussing the universal range of individual variation of a particular behaviour on the basis of a sample of 3 children, 3) establishing a model of cognitive performance upon the administration of one part of a single IQ test, 4) arguing the existence of cross-cultural universals on the basis of samples from two countries, and 5) claiming on external category system (developed by the researcher) to be "mental operations (i.e. ... "real" operations)".

5) The deficiency of methodological awareness in many LA works is manifested in the procedures in which hypotheses, which are beyond empirical control, are set up and then considered not as hypotheses, but as positive information, as Chomsky's LAD (Language Acquisition Device). It is with good reason that Chomsky's starting point (1965:32), which ascribes to the child "an innate theory of potential structural descriptions" is criticized by various sides (see Oksaar 1977:205). It has hindered a productive development of LA theory, it diverts from the problem of getting under control how the child learns the basic grammatical relations, since they are explained as innate.

6) Methodological objections can be made to the use of the term *universals*

in LA research. We know very little about language acquisition and language development in non-Indoeuropean languages. It is not correct to compare general tendencies in a very small number of languages with universals, particularly using data based on extremely small samples. (Ferguson/Slobin 1973:197 f.) Already on the metalevel of description there are difficulties: what belongs to morphology in one language, may belong to syntax in another.

7) It is also important to observe the differences in LA and the reasons for them and to find methods of establishing them. Methodological interest in the problem of the systematic differences in language development has, however, hardly been shown till now; an approach in the field of syntax can be found in Ramer (1968).

8) The future techniques of description must consider the development of the children's abilities in social interaction and in the dependency on sociocultural norms of the group, according to their age—an area which has hardly been studied up to now. In order to grasp the important dimensions of social and linguistic variations, a combination of the correlational and interactional approach mentioned in part 1, could on the basis of communicative acts form a far-reaching methodological framework.

Integrative approach. In order to interpret the child's linguistic behaviour it is advisable to proceed from an integrative approach (Oksaar 1975a, 1977). This approach is based on the fact that the child must develop a communicative system and must, therefore, also learn rules of action and interaction. This includes the abilities to judge the situations in which his verbal and nonverbal expressions are appropriate. The point of departure of the integrative approach is neither the word nor the sentence, but a unit of interaction: The *communicative act* (CA). The CA includes the total frame of action in which the activity of speech takes place and is imbedded in the situation. The elements of the CA are: 1) partner/audience, 2) subject, 3) verbal elements, 4) paralinguistic elements, 5) kinesic elements, 6) the whole affective behavioural characteristics. Contrary to the too one sidedly sender-related speech acts of Austin and Searle, it has the advantage of retaining both the sender and receiver relatedness in communications. The speech act is less appropriate for the analysis of language acquisition also due to the fact that it has not taken nonverbal and paralinguistic units of communication into consideration.

In the CA a mutual process of comprehension takes place and also mutual adaptation, accommodation and assimilation of speech and other behaviour patterns, which may lead, depending on the interlocutor, to the formation of variants even for adults. The relationship of gestures to vocal speech, an area regarded with growing interest in LA research, can also be effectively observed in CAs.

Language spoken to children is now considered as one of the most fruitful research areas, having in the last years broadened its attention "from input to interaction" (Snow/Ferguson 1977). The papers in Snow/Ferguson (1977), Ingram/Peng/Dale (1980) and Dale/Ingram (1981), offer good orientation on

current research in the child's development of dialogue skills and the nature of adult-child and child-child interaction. Already in the twenties psychologists have considered the dialogue as the most important and natural form for the child's development (Katz/Katz 1928). The question: what sort of language does a child hear?, however, has gained increasing interest first in the sixties, though the existence of the very specific register called "baby talk" or "mother's speech" was known already to Hugo Schuchardt, Hermann Paul ("Ammensprache"), Otto Jespersen ("nursery language"). Why is this simplified register so important? It shows that the basis of LA is not just any adult language—with "false starts, deviations from the rules, changes of plan in mid-course" (Chomsky) but rather a very specific variation of it, whose structure leads to the conclusion that it can ease the LA process. According to Brown (in Snow/Ferguson 1977) the old opinion about the order of this process "AS>CS (adult speech>child speech)" must be replaced by a new one, that is "AS>BT>CS (adult speech>baby talk>child speech)". The existence of this register thus invalidates one of the basic arguments of the nativist approach: that a child could not learn language on the basis of the defective data of natural speech.

This register also has other implications for linguistic theory. Already von der Gabelenz (1901) directed attention to its constant influence of adult speech. Variants of it do appear in adult-adult interaction. Von der Gabelenz (1901) has directed attention to the occurrence of it in the speech of lovers; nowadays, we can find characteristic features of it, such as specific grammatical and phonological modifications, and some of its paralinguistic patterns in the speech of native speakers with guest workers. The psycholinguistic reasons which seem to dominate in the first two cases are economy and vividness for emotional grounds, in the last one economy for better understanding. In this broader context we may come to look from another perspective at questions of language than is usually done. Because language use ranks above language change, the conditions of language use must be examined first. One can hypothesize that the conditions and motives controlling language use also could influence the process of language change.

3. On Multilingualism

After World War II research in multilingualism has been activated all over the world, resulting at present in a wide range of issues from educational and sociocultural aspects to psychological questions, to mention only the perspective close to our topic. However, there are not yet suitable theories that could offer a solid basis for treating cognitive, attitudinal and social problems arising out of the fact that individual and groups are using more than one language in their sociocultural environment, though various attempts have been made (for a review see Oksaar 1972). Much research has looked at multilingualism as if it was a static matter; this can be seen in the methodology, as well as in the circumstance that there are so few longitudinal projects. On the basis of my longitudinal project (1967–1979) on Estonian-English bilingual behaviour

in Australia, USA and Canada, and Estonian-Swedish in Sweden, I would like to make a few points to some topical issues in current research.

1) Is there a "critical period" for the acquisition of a second language (L_2)? According to Lenneberg (1967) it is not possible to learn a language, especially the pronunciation, with a native like competence after puberty, because it is during puberty that the lateralization takes place. His claim supporting lateralization theory has been criticized (Krashen, Dennis/Whitaker, Gardener and others), there is also empirical evidence, that L_1 can be acquired during and after puberty as in the case of Genie, who had no language until the age of 13 years and 7 months, as reported by Curtiss (1977). It is not even unusual, that people who emigrate at a very early age do not learn to speak the new language without interferences from L_1 , whereas adults have been able to do so. The fact, however, remains that a foreign accent is far more frequent in the speech of adults than children.

As neurophysiological theories alone do not seem to have a secure base, I should like to suggest a psychological orientation for building hypotheses. Instead of saying that an individual *cannot* learn a new language with native-like competence, one could equally well assume that the individual *does not want to*, even if this is unconscious. A lot could speak for this hypothesis, for the personality and the personal identity is being strengthened during this period. Many bilingual informants with Estonian and German as L_1 , in my project, explained that they would feel like they were play-acting if they spoke the L_2 with a "perfect" pronunciation, especially when the listener has the same L_1 or acts as auditory. This places the question of the critical period of L_2 acquisition and use in a new perspective—identity and ethnicity.

2) The following facts should be emphasized. Language use in face to face interaction takes place in the ecological near-milieu of people in which not only linguistic, but also culturally conditioned behaviour rules are valid, which call forth and regulate situationally conditioned behaviour patterns. Traditional linguistic models and analysis instruments are not sufficient to describe a multilingual person's code switching, situational interferences, his violations of pragmatic and semiotic congruence, and to understand their functions and signal values. Research in bilingualism has usually dealt with L_1 and L_2 first of all from the point of view of language as a system and the individual, neglecting the group and language use. However, including also these factors, Oksaar (1972, 1979) has shown that there are three main systems in the repertoire of the bilinguals: L_1 , L_2 and L_x . L_x is constituted of items of L_1 and L_2 from all data of language, but it has its own norms of usage linguistically and socially. These norms are directly connected with code switching. There is *external* and *internal* switching, due to constituents and conditions of discourse (Oksaar 1974), L_x develops through the knowledge of two languages and the social relations between the speaker and the hearer.

The degree of familiarity between the interlocutors determines the type of interference in the frame of two models of interactional behaviour—the

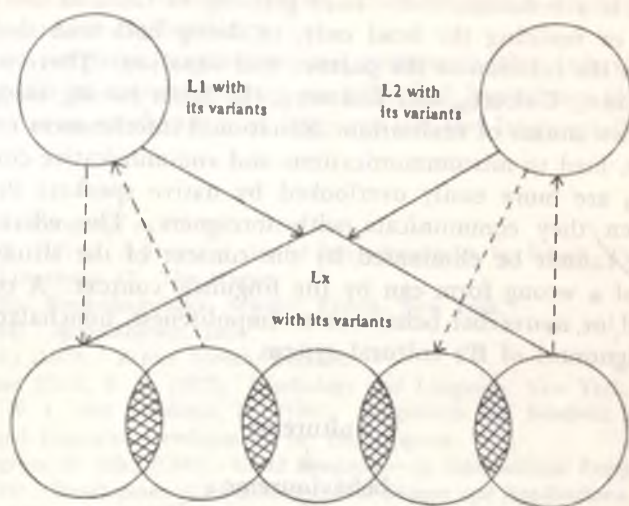


Figure 1: L1, L2 and Lx

Table 1: Average usage of types of interference in social interaction

Type of interference	Type of interference			
	integrated ms transfers	unintegrated ms transfers	loan translations	loan creations
Social relationship				
close friends	×	(×)		
acquaintances	×	(×)	(×)	
less acquainted		×	×	×
not acquainted		×	×	×

From: Oksaar (1980)

normative model, with attention on L_1 norms, and the *rational (spontaneous)* model, with focus on effective utterances, according to the demands of the situation. The traditional concept of interference has to be differentiated: There are *linguistic* and *situational interferences*, the latter are deviations from the pragmatic norms of situation in which the communicative act takes place and that demand a certain kind of verbal and/or nonverbal behaviour. They occur in connection with the culturemes of greetings, thanking, addressing a person, etc. It is necessary to operate with the concepts of *cultureme*, behavioural patterns in contact with other people, and *behavioureme* in this model, as routine and ritual does not cover the whole field. The mere fact that people greet each other, is a *cultureme*, how they express it—verbally and/or nonverbally and/or

extra-verbally, is a *behavioureme*. Thus greeting in German culture is: saying "Guten Tag" or nodding the head only, or doing both and shaking hands—all depends on the relation to the partner and situation. There is then, also, a model of Culture₁, Culture₂, and Culture_x, the latter having elements of both, but with its own norms of realization. Situational interferences can, more than linguistic ones, lead to miscommunications and communicative conflicts. Deviations from L₂ are more easily overlooked by native speakers than deviations from C₂, when they communicate with foreigners. The effect of a wrong behavioureme cannot be eliminated by the context of the situation, as easily as the effect of a wrong form can by the linguistic context. A could interpret B's verbal and/or nonverbal behaviour as impoliteness, nonchalance or insensitivity, being ignorant of B's cultural system.

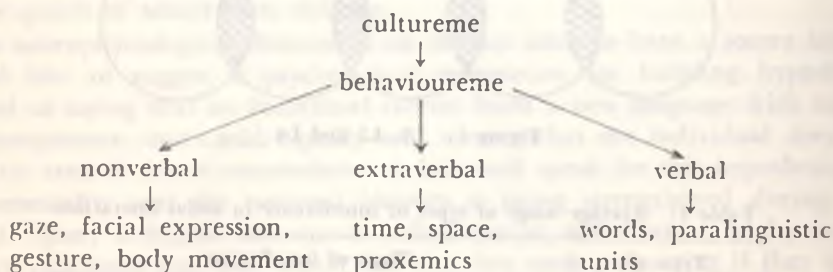


Figure 2: The cultureme model

The models are important for the analysis of language change and change of communicative behaviour. Through the normative model L₁ receives loan creations; the rational model is the source of loan words. This process is dependent on the attitudes of the speakers. During the process of borrowing, many speakers react negatively to the items borrowed, but feel neutral or positive towards loans that have been integrated into L₁ some time ago. The process of acceptance of new linguistic items needs more investigations; concerning new items via C_x, there is hardly any research yet. The models connect psycho- and sociolinguistic concepts, on the basis of the CA.

The interaction of the elements of the CA can be represented by a *congruence model*. Understanding and misunderstanding depend on at least four congruences. In addition to the usual congruences a linguist has to deal with—the grammatical and semantic congruence—we suggest two more congruences—the pragmatic and the semiotic one. I consider *pragmatic congruence* to be agreement between the contents of the verbal, paralinguistic and non-verbal information carriers. *Semiotic congruence* points to agreement of behaviour patterns in time, space and action. The entire communicative act must correspond to the norms of situation.

What is needed for the eighties in psycholinguistics? More longitudinal studies in language in action, more cross-cultural research; more attempts to

overcome the difficulty of communicating across discipline boundaries and more interest in qualitative methodology.

In studying language acquisition and communicative behaviour of mono- and multilinguals in natural settings we may find hypotheses for the explanation of at least some mechanisms of language use and language change.

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Psycholinguistics and Linguistics: Old Relationships and Promising Prospects

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0. I will but briefly speak about some aspects of the relationships between linguistics and psychology or psycholinguistics (PL) in the past.¹⁾ More extension will be given here to my own psycholinguistic approach and to a tentative prognosis of the aforementioned relationships. This solution seemed to me more appropriate, as I cannot speak exhaustively about all the other present-day PL approaches. The reports to be presented in the same section will undoubtedly bring up contributions regarding various other trends.

1.0. I am not an adept of complaining on the "ruins of the past". The more so when we have to deal with some of its most "critical" aspects. But nor do I think that we could discuss about present PL and especially about the PL we are dreaming of for the future times, without at least mentioning some of the mistakes that were made. We may only hope (a vainless hope perhaps) that via such an "exorcism" they will be avoided from now on. It is all the more necessary to discuss them at the first international congress of linguistics of this new decade, as some of the "faults of the youth" of PL are linked to a great extent to the relatively recent history of linguistics (and less to the other *mother-science*, i.e. psychology). Mainly in the sixties, the connection between linguistics and PL was mostly patterned from cause to effect (the latter being PL—see the influence of generative-transformational linguistics). There was also, to a certain extent, an inversion of the relation, that could be noticed especially in the seventies (see the proliferation of such trends as sociolinguistics, "pragmalinguistics", "text linguistics", "discourse analysis", etc.). For the future—in view of a fruitful development of linguistics—I personally should certainly hope that this determinism of PL on linguistics may increase, with one condition, however: that PL be well directed itself, so that it may be of real help to linguistics and not an obstacle or an additional burden for it.

1.1. The results of the old analyses of "psychological linguistics" often seemed

1) With a few changes, the text of this report is the same as the one sent in July 1981 and printed in the volume of plenary reports published in advance by the XIIIth International Congress of Linguists (distributed before the Congress). Notes are added, that include some remarks I made in the oral presentation of my report (at the Congress plenary session), mostly concerning E. Oksaar's and W. Levelt's reports (here referred to as PR, i.e. "(volume of) printed reports"), which I only had the opportunity to read after having sent since long my own report. Notes also include some of my comments or answers to questions, which I made during the discussions that followed after the oral presentation of the reports.

to rely on an impressionistic method: hence the difficulty of acknowledging lecut and, especially, objective psychological principles to be used in linguistics. This was the main reason, in the past, of the mistrust shown by some linguists for the psychological approaches of linguistics, that were therefore deprecatingly labelled "psychologism in linguistics" (see also De Felice, 1966, p. 16).

It is well known that it was only after the almost programmatic development of psychology as a (relatively) exact science, in the late forties, while trying to process data that were objectively gathered, that it became possible to establish other relationships between linguistics and psychology. This fact brought to the development of the new discipline, "*psycholinguistics*". Mention should be made here of the contribution of the concept of *interdisciplinarity*, that meant, in all sciences, something else than a *mere collaboration among various fields*. As to the relationship between linguistics and psychology, that meant, on the one hand, the development of a real interdisciplinarity, an "integrated" one, as we named it (Slama-Cazacu, 1981). This calls for the replacement of a simple "exterior collaboration" with a fusion of common aspects of the two sciences, and, even more, their *integration* within a new *sui generis* ensemble. Hence our emphasizing (Slama-Cazacu, 1973 [1968]) that PL should become a new, autonomous (independent) science, as it is neither a branch of psychology nor a branch of linguistics. On the other hand, the new relationships between the two basic sciences also implied the existence of two sciences that had been evolved in the meantime: scientific psychology that we have already mentioned, and linguistics that, in its turn, had a different pattern—the "structural one"—and was able to understand and accept the concept of a "language in function" (although it was unable yet to *apply* this concept to the practice of language study).

1.2. As soon as it was created, this new field, "PL", met with criticism—for new reasons this time. There were either some linguists who considered it a redundant label—that could have been synonymous with "a well done linguistics". Or there were some others who identified it with one trend or another (such as behaviorism or, later on, as generative-transformational linguistics [GT]): as they did not accept scientifically that trend, they consequently also rejected the field which was confused with it. On the other hand, I might say it was conversely damaged by "blind love", by a non-critical adherence to theoretical conceptions fundamentally unfit for a field that should not ignore the *reality* of "language in function". I am mostly referring to the influence of GT linguistics—which I cannot consider as an aspect of the past, already gone and that should be forgotten.

1.3. The attempt, in the sixties, at making PL depend on GT linguistics overlooked some contradictions (that were not noticed or not taken into account although they were fundamental) between PL as it ought to be and a conception operating with "data", not with "facts" that were supposed to "go well beyond these observations". (Chomsky, 1961, p. 219).

N. Chomsky proved a special attachment towards psychology (see, *inter alia*, the very *psychological* concepts of "competence/performance") and went as far as dissolving linguistics in psychology (while considering linguistics as a "particular branch of cognitive psychology": Chomsky, 1968, p. 1). Modern, "scientific psychology",²⁾ however, would hardly recognize itself in Chomskyan abstractions, as "the ideal speaker/listener", "unaffected by such grammatical-

2) Dr. W. Levelt asked me what I intend by "scientific psychology" and why would it be an "incompatibility" between it and the Chomskyan conception. (I had in mind, of course, the well known concept of N. Chomsky's "ideal speaker/listener"). My answer (but summarized here) was that modern scientific psychology starts from (i.e. has both as its "major subject matter"—as Dr. Levelt admitted—and as a postulate) the *concrete* individual, with all the variables this concept involves. No scientific researcher in modern psychology can ignore—whatsoever variables he aims to principally study—subjacent variables such as the *Versuchsperson's* personality, age, context of life, momentary disposition, etc. (all requirements that are studied by any student in psychology!); this means that we struggle for keeping into account the *concrete* being "used" as a *Versuchsperson* in an experiment. (Advocating for experiments as well in psychology and in PL, I have even—the same as do many experts in present psychology—doubts if sometimes laboratory does not bias our results as far as the very reality is concerned, because the individual may behave in a different way in a laboratory than in "real" life, will have a different motivation, is surrounded by a "distillated" atmosphere, etc.). *Only* starting from such a study (at least, *aiming* to knowing and taking into account the concrete individual), we can arrive to the *general* knowledge, principles, laws—aimed to by any science—that would *mirror* what happens in the very concrete reality and would not be derived from speculations or from the imagination of a researcher. Hence there is no contradiction between the study of "concrete individual" by modern scientific psychology and the *general* laws of psyche that are its final aim, or its *postulate* (starting from or arriving to the general laws of the concrete human individual). Therefore, there is an unavoidable incompatibility between modern scientific psychology and theories that operate with concepts such as the "ideal speaker". That is why some Chomskyan hypotheses were not validated by psychologists (such as, in the 60s, by G. Miller), and also why N. Chomsky enhanced such artificial separations as the one between syntax and semantics, or competence/performance, thus also ignoring the very *reality* of *concrete* communication (which can not be ignored by modern scientific psychology). One major counter-argument is often formulated (and it was expressed here by Dr. S. Grünwald too): "GT changed" and "situation is no more so bad as it was before", hence one criticism or another is no more up to date. However, with such a trend, that changed so frequently, it is impossible to find his way and a reference point that be "sure" or "stable"—at least for what in science can be considered "a while"—within such a very fast dynamics. It is true that any scientist changes—or even *has* to change—something in his views (Dr. Levelt mentioned the name of W. Wundt in this concern); with some conditions, and first of all: that such changes be not so frequent, and not so radically or essentially different (unless a trend would be differently defined or the scientist lose his personality). No such radical change was operated within GT, and the "abstract" individual, or language as an abstraction (separated from the act of communication), or splits within the human being—see below, note 6—are still *the* basis of this theoretical conception. We have witnessed even during this session that essential changes did not occur: for example, a GT faithful disciple manifested his non-agreement with the thesis of the "concrete individual"; Dr. M. Bierwisch put even as an aim for PL to adopt its methods from "physical sciences" (i.e. ignoring the human specificity of the subject matter of linguistics and of PL).

ly irrelevant conditions, as memory limitations",³⁾ etc.: Chomsky, 1965, p. 3. PL based on the GT linguistics of those years operated with hypotheses, concepts, even with Chomskyan methods, heedless of the contradictions and utter incompatibility between the fundamental postulate of present-day scientific psychology, the *real concrete man*, and the main theme of Chomskyan linguistics: the study of "competence" separated from "performance" and derived from "the ideal speaker/listener's" knowledge about language.

Within this framework, the relationships between linguistics and psychology—or PL derived from GT—came to speculations on the concept of "innate language" (the "inneistic" thesis) or on some other Chomskyan hypotheses, or were reduced to experiments (the majority of them "built up", not in the least gathered from a corpus) isolated from any situational context or even from a linguistic macrocontext and beyond the real act of communication.

Such substantiations of the relationships between psychology and linguistics led either to deadlocks or mistrust shown by linguists who were "traditionalists", or just "outsiders," or by those defending the objective study of the language facts gathering) and the principle of taking into account language "as it functions".

2.0. The conception that I applied for more than 30 years (see Slama-Cazacu, 1956 [1954], 1961 [1959], 1973 [1968], etc.) was verified through personal researches, as well as through researches carried out by other authors, and, in fact, I realize more and more that it represents a direction widely spread today as a "fashionable" trend. I will further mention some of the linguistic consequences of *this psycholinguistic conception*.

2.1. While briefly summing up this conception, may I emphasize, on the one hand, the idea of integrating every message "utterance", "sentence", "text", etc.) *in the act of communication* (that implies emitters and receivers, interrelated in a *social relation*⁴⁾). On the other hand, the idea of integrating the messages in *contexts* of various levels (*explicit*: linguistic or verbal, and non-verbal—gestures, facial expression, etc.—; *implicit*: situational, social and historic, as well as the context formed by the internal and external coordinates of the emitter etc.: schemes 1 and 2). In oral communication (predominant in human life), but also in writing—in forms we cannot possibly discuss here—the dialogues prevails, with its specific "dialogued syntax", together with a continuous "mixture" of verbal and non-verbal elements,⁵⁾ in what I have called "mixed syntax" (Slama-Cazacu, 1961 [1959], 1964, 1976, etc.). This conception takes into account emitters and receivers as human beings whose psyche

³⁾ This is another example of a restriction that could not be admitted by modern scientific psychology: "memory limitations" can never be ignored and one could even say that they are part of the "general" psychological features.

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is socially determined. At the same time, it takes into consideration the influence of the *entire* human psyche on the message and conceives the organism as a *whole* (of course, including aspects or parts that have their specificity)⁶). This implies the close relationships between thought and language, the stress laid on the importance of consciousness in the emitter's activity of organizing the message and in the activity of interpretative reception of the message, etc.).

The study of every fact in relation with the various contextual levels and, at the same time, conceptualizing the *context* as an organized but moving (dynamical) structure, all these are essential and specific peculiarities of this conception that I called "the dynamic-contextual theory" (D-C) (with its own adequate methodology). Special stress is being laid, in this conception, on substantiating every assertion with facts (gathered from corpuses derived from real communication, and as far as possible from experiments). I also advocate the necessity of statistical data processing before any attempt at generalization, and of their interpretation (the necessity of explaining the facts): PL is not a descriptive discipline, neither is it a discipline of data systematization, it is an explanatory field. As an example of this approach we may mention the propensity to the study of real communication—the oral one for instance—with everything this implies: hence the integration of any utterance in the contexts of various levels, the inclusion of non-verbal elements together with the verbal ones—whence the study of "mixed syntax"—, the opportunity of realizing the dynamics of message concatenation in dialogues—with the characteristic peculiarity of completing "reply" via the previous and following ones, whence the study of "dialogued syntax"⁷)—, within the framework of a situational and social context, actually of afferent activity.

I consider that "dynamic-contextual PL" can reveal indeed the aspects of *human communication* as it really is, and even some phenomena of *language as it functions* (that appear when language is studied within communication and in connection with human psyche, the latter being considered within the social context, beginning with the relationship between emitter[s] and receiver[s]). This approach has implied since long (see, e.g., *Langage et contexte*, 1961

6) This *integrative* approach prevents me from subscribing to such splits, as the "classical" ones in Chomskian theory (between competence and performance, for example), or the splits operated in his report by Dr. W. Levelt between the "modules" (a concept now in fashion in biology and for which he strangely feels it necessary to make reference to a linguist, N. Chomsky): the "modules" are considered to be "autonomous", interaction being "absent" between modules (cf. PR, p. 244). I find here, again, the obsessional path followed by Chomskian tradition, namely that of dichotomies where the two members are separated.

7) See also very recent studies that I published using large corpuses of *dialogue* (Slama-Cazacu, 1982a). The study of language functioning in a dialogue may offer other results than studies on narration: anaphora and maintaining anaphorically the same referent is not "specific to narration" (cf. W. Levelt, PR, p. 249), and it very probably was not so much observed in children by Dr. Levelt's coworkers not only because in children's narration parataxis dominates—and anaphora mostly appears in hypotaxis—, but also because apparently only narration was studied in Nijmegen from this point of view, while anaphora occurs very much in children's dialogue.

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[1959]) an-in-depth study of "the speaker" or of "message production"⁸⁾.

2.2. There follow, I think, some important consequences for linguistics. The conclusions will show that I do not imagine as being possible yet to give up the schemes of traditional linguistics, and hence a complete change of linguistics as well, that may enable it to break its traditional deadlocks, and—the much important—especially the present one. Nevertheless some modifications are possible for the time being, and some of the facts brought to light by PL researches resulting from the above mentioned conception can *be useful* to present-day linguistics.

2.2.1. The first consequence of D-C psycholinguistic conception is the necessity of studying each utterance in a *context* (linguistic, situational, etc.). Irrespective of the "unit" that might be taken as "fundamental" in linguistics ("word", "transphrastic text"), this should be considered as a (micro)-context, but has to be overcome too and integrated in other contexts, of various levels, in view of its correct interpretation (by the linguist or by the receiver of the communication act). I shall not dwell on examples that are already commonplace, such as solving syntactic polysemies of the type *la vengeance des oiseaux* ("the revenge of the birds" or "the vengeance of the birds"), or *amor patris*. Neither shall I insist upon the establishing of the exact reference of pronominal adjectives, adverbs of place, of anaphorics, or of the acceptability of the utterance *only* with reference to a context.

I shall mention the fact that apparently the "principle of adapting to the context" (formulated more than 30 years ago: Slama-Cazacu, 1956 [1954]) is acknowledged now and applied, without having in view, however, all its logical consequences. For instance, whereas anaphorics can be interpreted only in a context, one has to admit not only the fact that "intraphrastic" (intra-sentence) relationships have to be very often surpassed, but also that linguistics cannot ignore extra-linguistic contexts (data related to the coordinates of the emitter, of the situational context, etc.). Or: an apparently non-sense, though grammatically correct utterance, such as *Flowers pick up humans* (cf. also the "notorious" *Colourless green ideas sleep furiously*), is semantically acceptable as well in the context of a poem written by a Romanian modern poet. Acceptability/non-acceptability are concepts which, I think, have no sense unless they are com-

8) An aspect also largely studied in our works since long (see Slama-Cazacu, 1961 [1959], Chapter *Technique du langage*, or 1973 [1968], Part I, Chapter 3, etc.). That is why I was astonished by Dr. W. Levelt's repeated assertion that *this* study was neglected in PL—or, for some details, that they are completely new as an issue of Dr. Levelt's Institute. I have a great respect for the activity of this Center—so as I have for any scientific work—but much of what is considered in Dr. Levelt's report as being *new* was already suggested, investigated, discussed, and sometimes advocated for with what was called as a "courage" in the debates of the 60s—and I have done it in some books also translated in more fortunate languages than Romanian, hence easy to be read, the much more as they were published in Dr. Levelt's own country. There were also, these last few years, several studies published on this topic, research done in centers in various countries, and international meetings organized (as the one in Kassel, 1980, on "Speech production").

pleted by the phrase "within a certain context" (the Romanian utterance *Cai verzi pe pereți*—"Green horses on the walls"—seems not only semantically, but also grammatically incorrect because the verb *umblă* "run" is missing; it is however perfectly acceptable to Romanian speakers, with the meaning "to speak to no purpose", "to talk nonsense").⁹⁾

While applying the principle of context reference especially in oral communication (where the situational context is greatly implied) "ellipsis" is inexistent in fact; therefore it appears to be a false problem of traditional grammar (Meillet, 1921, p. 60 correctly noticed that some sentences should not be considered elliptical only because they have one single component). In fact, any utterance, in real communication, is completed by the context, so that ellipsis exists but in a schematic view of grammar (when excluding from grammar both the completion through dialogical syntax or through gestural components, or indirect references to objects in a situation, or to the emitter's coordinates that are known to the receiver, etc.; in the written text the completion of the sentence through the linguistic context). E.g.: "[Get] down [from the tree]!" (quoted from Slama-Cazacu, 1961 [1959], p. 213).

2.2.2. Another consequence: The necessity of studying grammar in *the real act of communication*, that is in dialogue and taking into account non-verbal elements. Hence, the study of *dialogical syntax* (still almost ignored by "grammar"), in which the sentences can have connections "at a distance", as they are completed by the replies of other partners, or "over" their replies. E.g.: "[John is speaking:]—that man...—[Helen:]—was a high thief...—[John:]—no, kindhearted" (Slama-Cazacu, 1982a).

2.2.3. From the methodological viewpoint of studying grammar in real communication there also results the necessity of taking into account the "mixed syntax". The verbal elements are replaced, to a great extent, within the same sequence, with non-verbal elements that are objects, attributes, etc. (so the non-verbal elements do not have only the stylistic role of potentiating the verbal elements: cf. Slama-Cazacu, 1973, a.o. studies). E.g.: "*Take [the bag, shown or given] and buy some bread. Here [giving the child some coins], don't lose them!*" (other examples, in Slama-Cazacu, 1961 [1959], p. 213, etc.).

The role played by gestures and facial expressions, using the objects or the "space" as such in communication can no longer be ignored by grammar. At any rate, for the time being, the study of deictics (connected with everything that is included in a situation) has become a "fashion", so that the studies that have been published lately are likely to call for a re-examination of the view-

9) I did not give here many other categories of examples of context dependency, which were studied by me (among others, on anaphora, deixis, ellipsis—of which Dr. Levelt assents "little is known"—PR. p. 248). However, as Dr. Levelt mentions in his report—speaking about new research done in Nijmegen—the fact that "A speaker may use a word which on one reading is correct, but the context also allows for another reading" (PR. p. 253), may I mention my experiments, done a long time ago (see 1961 [1959]—mostly presented, *in extenso*, in the Romanian version), as well as examples such as: the Romanian verb *umblă* and *umblă* (hence differently read by the speaker in different contexts).

point of the so-called "linguistic grammar".

2.2.4. The necessity of studying grammar in *social contexts* while taking into account the *adaptation of the interlocutors to the partners* and to the *situation*. Hence the selection of means of grammatical expression that can be correct or not according to its being suited or not to the situation or to the partners' social status, to their knowledge about the emitter, etc. This is a desideratum that is achieved to a great extent by present-day sociolinguistics (SL). However, it would be scientifically incorrect and damaging to this study if one considers we are dealing with a "discovery" of SL or that SL alone can solve this study.

The thorough study of *oral communication* (through PL methods for the time being) is likely to call for a re-examination of some normative rules of the "official" grammar (especially that taught in school) and exceptions to the accepted rules might be admitted. This necessity will be more and more imperative when more elaborate studies are made on communication during the process of work and of activity in general (see Slama-Cazacu, 1964).

2.2.5. The necessity of introducing, in the study of grammar and in stylistics, *para-linguistic elements* occurring in real communication. E.g. the parasitical elements of *hesitation* (typical sounds for each language: as, in Romanian, əə: . . . , Ym: . . . ; in English ər: . . .); or clichés marking hesitation ("I mean . . ."); or other para-linguistic elements with special functions in the sentence, as "blank" pauses with deliberately meaningful role (caution, contempt, etc.), or unintentional (showing the receiver the emitter's deliberation in choosing an alternative).

2.2.6. There is another aspect that can be mentioned here: the expression is organized on an *interior* plan, before its becoming an exterior, sequential fact. This organization¹⁰⁾ that begins at the *pre-locutory* level, on a mental plan (where thought and affectivity occur as well), implies, of course, a specific psycholinguistic study or even a strictly psychological study, but the problem itself is also interesting for the linguistic study of grammar. Once the subject of a sentence has been chosen, the entire following pattern is also determined (cf. Warland, in Pottier, 1966, p. 101); or, as Pottier (*loc. cit.*) mentioned, when

10) See above, note 8. In *Langage et contexte* for instance (e.g. p. 165, 167, foll.) the chronological arrangement of discourse components was dealt with in details. It is sure that research done in Nijmegen will give us more details and even new facets of these aspects. But omitting from References the pioneering works, or asserting that nothing was done before, or ignoring what was already studied is at least not efficient for the economy of a research. Concerning "linearization" itself: I do not consider that speech production should be envisaged only or mostly as a simple chain-like or simply "linear" construction: "Language has only apparently a linear dimension, that of time: it cumulates succession and simultaneity" (Slama-Cazacu, 1961 [1959], p. 186); let us think to the super-segmental dimension at least, even if we do not take into consideration—as one should do—also gestures or other nonverbal components of the same message, occurring simultaneously with the "words" chain, within the same time unit. The effect upon computer models of language and the programs will continue to be of an extremely ineffective influence, if this pluri-dimensional complexity of human message (and of "speech production") will be ignored.

you begin a sentence repeating (in French) *les, les, les*, that means you *know* that a noun in the plural will follow. The problem is not as simple as it seems to be: hesitations, repetitions, recurrences, self-correction, all prove these difficulties in expressing a thought grammatically. The slips of the tongue may also prove sometimes the same process of organization on an interior plan, rather complicated too: "the Batt [le of Watt] erloo" Slama-Cazacu, 1961 [1959], p. 171), or: *un callo de poldo* instead of *un caldo de pollo* (Pottier, 1966 a, p. 95).

3.0. Consequently, here are some characteristic features of the D-C conception: the stress laid on the study of the means of communication in real "pragmatical" situations of life; on aspects of *oral communication* that are not fully known yet (hence the study of *the dialogue*); on the connection between messages and the *psyche* of the emitters/receivers, deeply determined *socially*; on the role of the *context* (at various levels); on the surpassing the "phrastic" level when analysing the language facts ("transphrastic analysis"); on the connection "at a distance" of the different elements of a text and on the transformation or even the elimination of the concept of "grammatical ellipsis"; on understanding the message as a *structure intentionally organized* by the emitter (a structure that also includes extra-linguistic cues—with the indicated "objects" as well—and the *non-verbal* signs, which the emitter presumes to be known to the receivers or perceptible for him; on taking into account "the implicit" in reception (which is in fact an "*interpretation*" of the message, going *beyond* what is *explicitly* given *verbally* by the emitter: a concept also discussed since long in Slama-Cazacu 1961 [1959]).

3.1. Today, some of these emphases may give the impression of a "déjà vu" or, for others, of something new, and discovered *ab ovo* by them. Various actual trends emphasize one or the other of the above mentioned concepts: "pragmalinguistics", "text linguistics", "discourse analysis", the "theory of presupposition" and of "expectation", "semiotics", SL. At any rate, it is not the D-C theory and methodology that has "copied" or eclectically gathered its various characteristics from the trends "fashionable" today. The historical reality is that our works that have defended these concepts (or that have even "struggled" to enforce some of them) are by far anterior to these trends. It is more significant however to notice that what is important for PL and in general for the linguistics of the future is not so much the separate one or another of these characteristics, to put the stress on only one of them, but to *integrate* them—what it was done since long as far as I am concerned—in a unitary whole. Emphasizing their co-existence, in an *integrative* view, means also to logically connecting them reciprocally: for instance, relating the concept of "dialogical syntax" to that of "contextual connection", of "mixed syntax", etc.).

It is peculiarly important to follow to its last consequences each of these concepts and principles, as well as their ensemble. Very probably, the drawbacks of some modern trends and the deadlocks they reach come from the *onesidedness* of each of the above mentioned trends, from the fact they are not derived from a unitary theoretical conception that may account for them.

3.2. Therefore it is not indispensable to D-C psycholinguistic conception to mark *its* limits as against SL and the other trends. The necessity of such a delimitation may occur for these unilateral trends, which in fact appeared afterwards. This delimitation might be useful not in order to help psycholinguists to make a "living" and keep their jobs, but in view of delineating the specificity of each approach and their peculiar capacities in the theoretical solution of some problems and in research methodology. It is also necessary not to forget that while asserting only *one* peculiar aspect, one should not neglect the other aspects either. Only thus may these trends and disciplines be really useful for the linguistics of the future and in general for the study of human communication.

3.3. Is such a PL but a "well-done linguistics"? (as Roman Jakobson said in his report to the International Congress of the Linguists in 1967; he did not include this statement in the published text, see Jakobson, 1970)? I personally do not think so. There are essential differences between PL and linguistics (differences that were pertinently pointed out by Jakobson [1970] too).

Just in case—one can hardly hope for—when it would be decided by a consensus to transforming linguistics radically in such a direction, the following possible consequences could derive: a) either linguistics would disappear (see the Chomskyan hypothesis of dissolving it into "cognitive psychology") and it would thus become a "well-done" PL; b) or *language* as such (an abstract concept, the general—therefore abstract—system of signs) would be no longer studied by any field (or a new one would be especially created to this end), because PL (that would be this "well done linguistics") *cannot* accept it as its subject. I do not think that linguistics is "badly done" because it studies the level *langue*. It is important however for linguistics to take into account *the existence* of the level *parole*, as well as the *explanation* of phenomena, that can be specifically given by PL. This explanatory science in the true sense of the word should link the language facts to the human psyche included in the *social*, while considering the consequences of this determinism for the study of human communication.¹¹⁾

11) Dr. W. Levelt asserted, in his synthesis of discussion, that in this Congress plenary session PL was not represented in all its trends, because many other psycholinguists were absent. In my own synthesis—which happened to be the last—I expressed my opinion in this concern, maintaining that the general main trends in PL were represented indeed. My assertion was not based on a lack of bibliographical information. On the contrary, not only I receive (as the editor of the "International journal of psycholinguistics") many papers of PL from all over the world, but I also know and read almost all what was published in this field (and also make references about—which is unhappily not a frequent procedure). On this basis I can maintain that PL of today *was* represented in its main trends. These are, in fact, the two directions of human mind, also symbolized in the well-known Raffael's painting "The School of Athens", where Aristoteles points toward earth (reality such as it is), while Plato points toward "sky" (the "ideal" models, the *a priori* patterns). And these major trends *were* represented here. There is no great loss, I think, if the third category—that of psycholinguists still in a Pirandellian ever "search of a theory" or of those who assert that a theory is not necessary for doing research, thus wandering blindly who knows where—was not represented here.

4. Giving up, at any rate, "science fiction",¹²⁾ we must say that today linguistics makes attempts at "turning into" a different thing from what it must be (it becomes "semiotics", for instance). But proper "linguistics", that respectable science, that *must* continue to exist with dignity, through itself, has not got an adequate methodology—and it has not a theoretical conception either—that may enable it to study the various phenomena we have mentioned. PL is the field that can gather and interpret, with adequate methods, various facts that cannot be grasped by linguistic investigation yet. For instance grammar still proves it cannot give up its traditional schemes, achieved mainly by reference to written texts. Until the frames and details of "a new grammar fully and consistently logical" will be built up, it may not be timely for linguistics to give up the thorough study of "the elements": word, phrase or sentence, syntactic connections *within the sentence*. This is still felt as a "firm ground" (even though there is a void under it). But it is not efficient for linguistics not to take into account the new scientific findings, both methodological and theoretical,

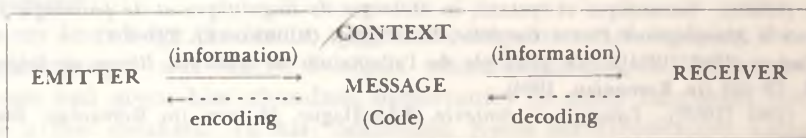


Diagram 1: Components of the communication act

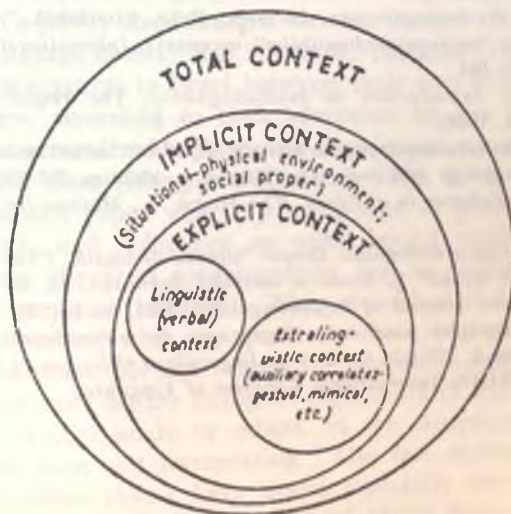


Diagram 2: Contextual levels

¹²⁾ I tried to delineate some of the basic principles of such an *ideal* linguistics, in an article (*Principles for a science-fiction linguistics*: Slama-Cazacu, 1982b) published in the volume that Romanian linguists dedicated to this Congress. I can only hope that this will be a linguistics of the next International Congress of Linguists.

regards the *using* of the facts gathered by PL means, as regards the theoretical study of the various peculiar languages, as well as concerning the principles of practical work, established through psycholinguistic researches, with a view to improving human communication.

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The Rationale of Language Choice

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"... There are, perhaps, two main reasons why one should learn the language of another man: in order to trade with him, or to have power over him, religious or political." W. Whiteley, *Swahili: The Rise of a National Language* (1969). p. 55

The problem I propose to discuss is the question of why speakers who have a choice between more than one dialect or language choose one rather than another. It is closely connected with the problems of "minority" languages and language "death", both of which have been much discussed of late by sociolinguists.

To my knowledge the first to suggest the importance of studying the fate of "dying languages" was Morris Swadesh, whose researches on Native American languages had given him abundant opportunity to study languages with only one or a few speakers. In his "Sociologic Notes on Obsolescent Languages" (1938, published 1948) he notes that "many of the circumstances described are similar to those found in the languages of some immigrant groups..." (p. 226). See also Elmendorf's (1981) amplification on this theme. The first to consider the problem of "language choice" was Simon R. Herman (1961), who described the vacillation of immigrants to Israel between their two languages, in this case English and Hebrew, according to their reception by the host societies. His results could also be generalized to American immigrant groups, as appears from my own writings on these (Haugen 1953, 1956, 1972, 1973).

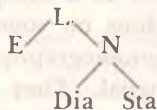
My approach in this paper will be a composite of personal reminiscences, systematic field work, and reflections on some recent studies by others. My reminiscences are not offered as autobiography, but as evidence of some of the problems faced by an active bilingual. They should also document my status as a "participant observer" who can speak of what it means to be bilingual. These anecdotal comments will also suggest the questions and problems that led to my systematic field work on the bilingual experience of my own ethnic group. My reading of the studies made by others, in the Americas as well as other continents, has been long and unremitting. The last section will consider in some detail recent studies that I have found especially rewarding, specifically those of Nancy Dorian in Scotland and of Carol Myers Scotton in East Africa.

1

There was a time when it looked to me as if I would be the last speaker of Norwegian in the United States. In the urban environment of the Middle West

where I grew up, I was the only Norwegian bilingual of my generation. Our closest friends, mostly immigrants from Norway, were not teaching their children Norwegian, as did my parents, even though they preferred to speak Norwegian among themselves, attended Norwegian services at the local Lutheran church and meetings of societies wholly conducted in Norwegian. I was a member of the last confirmation class (1920) in Norwegian in our congregation, and the only one in that or later classes who actively participated in Norwegian-American affairs. When leaders of the congregation proposed to eliminate the word "Norwegian" from the name of the church, I led the opposition; and even though we won the battle, we lost the war. Eventually my interests led to my majoring in Norwegian at St. Olaf College in Northfield, Minnesota, and then going on to become a professor of Scandinavian at the University of Wisconsin. I may note that I also majored in English, but I regarded that merely as a form of job insurance; at least it indicated that my bilingualism was no handicap in my use of English. But why did I choose to devote my life to what was not only a minority language, but in the U.S.A. also a dying language?

When I call myself "bilingual," I am here adopting a restrictive definition, closer to the popular understanding of the term than the one I have used in my writings. I here mean a knowledge and use of the language that is adequate for active participation in the life of the speech community and does not mark the speaker as an outsider by his accent. The languages or dialects in question must be different enough to require a total or near-total shift in language system, from their articulation to their lexicon. I therefore disregard any or all languages acquired in school or in later life: my kind of "feeling at home" in the language is rarely acquired after puberty. I became, before puberty, not merely a bilingual, but a trilingual, or what I would rather call a "double bilingual". By this I mean that I could choose languages according to a double dichotomy: (1) between English and Norwegian; (2) within Norwegian between a rural dialect and a very different urban standard:



To this day I have chosen to maintain and develop each of these norms in as pure and distinct a form as possible. Consequently, I am "at home" in any American English-speaking environment, in any Norwegian-speaking environment in America or Norway, and most especially in Oppdal, the Norwegian mountain valley where my parents were born and where I have been a frequent visitor.

Since the foundation for these skills was laid in childhood, one may say that I did not have much choice. My parents spoke only Norwegian at home and required that I reply in that language. Being an only child, I did not have the leverage of siblings, who typically form a counterforce to parental authority

in matters of language. My parents could not and did not try to prevent me from learning English: that came to me from playmates in the street and agemates in school, which was all American. But the threshold of the home was a language border: English outside, Norwegian inside.

I could still have lost my childhood skills while being absorbed into the American world of my school friends, if my parents had not transported me back to their native valley between the ages of eight and ten. Here I was faced with the necessity of speaking the dialect all the time, with playmates as well as adults; it was a linguistic world all its own. I well remember the initial shock, but also that I was quickly accepted and that these turned into two of the happiest years of my life. "Norway" was no longer, as to most of my generation, a remote spot on the map where my parents had once lived, but a world populated by relatives and friends, real communicating persons of all ages and both sexes. By the end of the two years I had come to feel this scenic valley as a home, a part of my life, and going back to America, where I had to relearn English, was a second shock.

Back in America I discovered that our Norwegian friends were amused at a ten-year-old boy's talking like an upcountry farmer, so I determined to learn to talk standard Norwegian as they did. With their help as role models, my school-teacher mother, and the reading of Norwegian books I was enabled to make the transition. The process was completed after I married a Norwegian girl of urban background and impeccable accent, and on the visits to Norway that we made together on trips that were both professional and personal. But whenever we visited Oppdal, I reverted to the dialect, and even began collecting notes toward a monograph (which has appeared as *Oppdalsmålet: Innsøring i et sørtrøndsk fjellbygdsmål* (Oslo, 1982).

I have since that time asked myself more than once why I made these choices. From a 100-percent American point of view I was deviant, not to say un-American. The home atmosphere of ethnic loyalty was basic, giving me my first, deep emotional experiences of life; on top of this came my two years in Norway. Norwegian became not just a house rule, but a kind of ethical imperative; this was reenforced by a variety of rewards. It was pleasant to be praised for one's language skills by sympathetic adults. On looking back, I see that I was enabled to play the role of a big frog in a little pond. The pats on the back that went with my participation in the immigrant world outweighed the costs that included some degree of segregation from classmates. Another cost factor was the effort of constantly learning and maintaining the languages in their purity. I still smart at the memory of occasional errors that exposed me to ridicule. Once I was explaining to some young immigrant friends the difference between "hard" and "soft" water. For "hard water" I made up a Norwegian word that sounded exactly like the word for "hair tonic" (*hårdvann/hårvann*), a favorite beverage for impecunious alcoholics. But all such mortifying errors were more than recompensed by the warm glow of being accepted, not just among fellow ethnics in America, but eventually also in the urban life of Norway as well as

in the rural life of Oppdal, sharing the joys and problems of friends and relatives, enjoying their hospitality and good will.

In later years I have more than once suffered disappointments with fellow ethnics in America, immigrants who refused to speak their native Scandinavian language with me. These have typically been immigrants who have been successful within the American business or academic community. Rather than maintain a conversation initiated in Norwegian, they have switched to an English that was far from perfect. I have asked myself: why should people who were born and raised in a Scandinavian country become so anglicized that they rejected an opportunity to speak their native tongue? Even some that I knew as young in their first period of immigration found it difficult to maintain their language after a few years in America. Politeness prevented me from asking them why or probing their resistance.

2

In order to find some answers to my questions about the problematic situation of Norwegian in America, I launched a research program in 1936 from the University of Wisconsin, where I not only had access to students who had grown up in bilingual Norwegian communities, but could myself go out and investigate usage in areas where Norwegian was still a common language of communication. What I found was fluent speakers of what I have called American Norwegian, a contact dialect or interlanguage. Nearly all my informants were born in America, but could speak dialects with a rural Norwegian base. They readily submitted to interviews which I had structured around an oral questionnaire modeled on that of the New England Dialect Atlas. The opening questions were biographical and included such language-sensitive topics as "When was or is Norwegian spoken?", "Do you read or write Norwegian?", "Do you subscribe to Norwegian newspapers?", "Can your children speak or read Norwegian?"

The results of my field work have been (in part) presented in my book *The Norwegian Language in America* (1953, reprinted 1964), where the problems of language choice occupy two chapters, respectively entitled "The Struggle over Norwegian" and "The Triumph of English." In these I have added a historical dimension to my field work by tracing the conflict back to the earliest writings of Norwegian immigrants in the 1840's, when the first permanent settlements were founded in Illinois and Wisconsin. The Church, built by the pioneers themselves, became the focus of language maintenance. As a much later writer put it, this was a period of "fencing in" the group, using the language to keep out heretical influences and to provide "something to keep themselves spiritually afloat." (Haugen 1953: 244). As late as 1900 the churches established in mid-nineteenth century were still wholly Norwegian in language. But a restlessness was in the air, and the next two decades saw an almost complete reversal in attitude on the part of the Church. Norwegian was clearly doomed, but its supporters put up a strong fight to delay its demise, and probably succeeded in holding the dike for another generation.

The turning point came with World War I, when the suspicion in which all things German were held colored off on the ultimately German-derived Lutheran churches of the Scandinavians. I have given some indication in my book of how bitter the dispute could be in man-to-man discussion, as well as the difficulties of the Church leaders in finding acceptable compromises. When the language no longer served the purposes of the Church, it was abandoned. In 1905 ninety-five percent of the sermons had been in Norwegian; in 1949 less than three percent were so. In less than a half century Norwegian had virtually been eliminated, and the surprise is only that it took as long as it did. The use of Norwegian was bolstered by the rural settlements, and the overall maintenance of Norwegian was noticeably stronger than those of either Swedish or Danish.

Some of my informants commented on the changeover. A woman in Wisconsin who preferred Norwegian sermons added: "As things are in this country, people marry into other nationalities, and the children don't get taught Norwegian. So I suppose it's best that it's losing out, but I'll be sorry to see it go entirely." There was a general feeling among them of inevitability: "I think it has to go that way," said one man. "We're in America, English is the language of the land..." Several commented on the influence of the American public school; as one man put it, "When my boy was small, he spoke only Norwegian, but after starting school, he changed right over." (Haugen 1953: 273) The picture that emerges is one of a large number of individual choices, not all taken at the same time, but gradually turning whole communities over from one language to the other. In horse-and-buggy days the individual farm had been a more self-contained unit than it later became. Cultural isolation is the obvious explanation in such cases for what is misleadingly called "language loyalty". Even when the parents held out for the language, they found themselves getting English replies from children who were addressed in Norwegian.

The Norwegian example, which I have sketched here as one both personally and professionally involved in it, proved to be typical of most immigrant groups in the United States and Canada, as I demonstrated in my later study called *Bilingualism in the Americas* (1956). To be sure, in Pennsylvania religious and social isolation have partially maintained the American-German dialect known as Pennsylvania Dutch for three centuries, but not in most other German settlements. In Quebec (and some other provinces) French has maintained itself by massive isolation, both social and religious. Spanish is the only non-English language that has grown in numbers, but its strength reflects continued immigration and close contact with homeland bases. To its strength we owe most of the growth of bilingual programs, which may function as reinforcements, but are aimed at ultimate assimilation. I see no probability that our authorities in the long run will support pockets of non-English speaking peoples in our midst. The stakes in terms of national unity are too high.

My research on the step-by-step retreat of Norwegian in the lives of immigrants and on similar developments in other parts of the world have led me

to a theory of language as a commodity on what we may call the *language market*. In a paper given in 1980 at a conference in Glasgow, Scotland, on North Atlantic minorities, I suggested that learning a second language exacts its price from the learner and that he will resist paying this price unless the benefits it brings are commensurate with the cost. (Haugen: 1981). Any language or dialect has its market value, which like those of all other commodities, fluctuates. Happily the market value is not only or even necessarily monetary, although that is certainly an important factor. In an immigrant group that is dependent on the majority society for jobs, knowledge of the majority language is essential for all well-paid positions. Some of the pressure is removed if the speaker can find a job within his ethnic group, say as minister, editor, or teacher, or even as doctor or merchant. It is precisely from these professions that most of the agitation for language maintenance comes, in part because their living depends on it, in part because of strong ideological commitment to the absolute value of the language. For many who are deeply indoctrinated in the mystic value of a language for the health of individual and community, the value of an immigrant language is very high indeed. But for most members of the group it is comparatively low, since it wins them respect neither from the younger members of their group nor from the outside world. As I wrote in my 1981 paper: "A language is a precious treasure so long as it serves as a reservoir of wit and wisdom from the past that provides guidance for the future." But to most people the future lies with the language that offers the greatest economic and social rewards.

3

In his study of immigrants to Israel, Herman (1961) pointed to the value of language choice as an indicator of the "direction of acculturation": "The choice of language is often a significant indication of the group with which they wish to identify." With some qualifications he concluded that language choice could "indicate the trends in the assimilation of members of one group by another." This is another way of saying that language is one of the salient boundary markers between ethnic groups, as shown by Fredrik Barth (1969).

These insights are given concrete formulation in the brilliant work of Nancy C. Dorian in Gaelic Scotland. Her *Language Death* (1981) is the concentrate of sixteen years of field work and participant observation among the Gaelic speakers in eastern Sutherland. Partial results have appeared earlier as articles, with more details about the linguistic nature of the local Gaelic. The area is one that for a long time was isolated, not only from the rest of the world, but even from most of the Gaelic-speaking areas of Scotland.

Like so many speakers of Celtic languages, the younger generations, one after the other, have gradually chosen to speak the language of their political masters and have little by little yielded up their linguistic identity. The Scottish story as Dorian tells it is a sad one, comparable to the evacuation of the Acadians to Louisiana, and resulting from the desire of landowners to introduce sheepgrazing

so that the Highlands of Sutherland might be turned to greater profit. The crofters who had their homes there were evicted and removed to the coast, where they were expected to learn fishing, a trade for which they were quite unprepared. As a result they became a fishing proletariat in the coastal villages, looked down upon and socially segregated, except as purveyors of cheap food. But they also proved to be the most "loyal" to Gaelic. One reason was that they were largely excluded from participating in the general turn-over from Gaelic to (Scots) English on the part of their fellow townsmen. I put the term "loyal" in quotation marks, for the loyalty was more like an exclusion from the "advantages" of their better-situated neighbors. It is questionable to speak of "language choice" in such a case, for the fisher folk had little choice, whether in determining their occupation or in acquiring English. At one point Dorian uses the term "linguistic lag", which is applicable in the same degree to the rural Norwegians who created compact Norwegian-speaking settlements in the Middle West. Sociologists Ruth and John Useem long ago (1945) compared rural and urban Norwegian immigrants in this respect, only to find that while the urban speakers were far more ideologically aware ("mobilized") than the rural ones, it was the latter who continued speaking the language, while (as I said earlier) the former did not even teach it to their own children.

Dorian's treatment goes far beyond the community she has studied into the theory of language shift and acculturation. She notes that in 1500 Gaelic speech was virtually universal in Scotland, but the practice of the anglicized upper classes set a model for language choice that gradually proved irresistible—like the reputed mafioso "offer they could not reject." The language that once was all of Scotland's was gradually stigmatized until anyone who used it was automatically branded, first as being lower class, and as the language retreated into the Highlands, as being wild, barbarous people. One could not find a better account of the deterioration of the position of a language under the social pressure of "upward mobility." Nowadays it is again the "lower class" stigma. Given a long-time political dominance, which is therefore also economic, social, and educational, any language can fall victim to any other. Dorian is clearly sympathetic to the linguistic plight of her informants and hotly denies that the result is inevitable (p. 72, 111). But her careful and lively account brings to light social trends and human weaknesses that come as close as possible to making language death inevitable.

Like so many language choices, the Scotch Gaelic one is largely illusory. While each individual may have some choice as to where he/she wishes to stand on the spectrum of Gaelic>English, the goal has long since been set by the government and the ruling elite. Anyone who did not wish to participate in the bilingual cycle from Gaelic to English was isolated, "queer," or even obstreperous, as we see from the punishments meted out to Gaelic-speaking children, a barbarous form of education that was also known in American schools in immigrant communities. Even where the stick has been hidden behind the schoolroom door, the carrot that replaced it has the same effect. The larger

society has a plethora of inducements for the bright boy or girl, provided they are not orally deviant. Gaelic speech is a remediable "defect." It is one part of group identity that can be shed, and the amount you lose will depend on the tolerance of the receiving society.

For a further study of language choice we now turn to the remarkable work of Carol Myers Scotton in East Africa. It seems a long way off, but as Scotton has outlined the situation after years of research, it has features so remarkably similar that we are tempted to begin to look for universals.

From her several writings on East Africa we select her book *Choosing a Lingua Franca in an African Capital* (1972), which concentrates on Kampala, capital of Uganda. Language choice takes place here under rather different circumstances than in our previous cases. Here there is apparently a three-way choice, between one of a multitude of vernaculars, an East African lingua franca, Swahili, and the language of the former colonial rulers, English. Since one does not usually speak one's vernacular except to members of one's own tribe, the main choice when one meets other speakers is between Swahili and English. While neighboring Tanzania has made Swahili an official language, and Kenya has to some extent followed suit (since 1974), in Uganda the only official language is English.

One reason for this anomaly is that Swahili is not native to Uganda, but was spread from the Kenya coast and the islands of Zanzibar and Pemba (p. 24). It was spread by Arab traders, and for some is associated with the Muslim faith, while Uganda is mostly Christian. Nevertheless it has had phenomenal success in spreading among the people, even in Uganda, as the language one uses to speakers of other vernaculars. Being a Bantu language, it is not hard to learn for Bantu speakers; Kampala even has a dialect of its own. But educated speakers either deny or minimize its role in the daily life of the country, calling it the language of "prostitutes and swindlers." Scotton's informants agreed that it was the language of the uneducated, even though 97 percent of them admitted that they knew it and occasionally used it.

English, on the other hand, is the language of the educated, learned only in school, where it is the medium of secondary instruction as well as taught in all the secondary schools. Use of English is also associated with wealthier and more developed nations, like Britain; and within Uganda it "characterizes the everyday public dealings of the educated and successful African." (p. 26). Scotton foresees two possible developments: either universal education, which could eliminate Swahili, or a nationalistic movement which could make it the national language and restrict the role of English. In view of Uganda's limited resources, she believes that the two will compete as lingua francas "in the foreseeable future." (p. 27).

Scotton's data were collected in exemplary sociolinguistic fashion by a combination of participant observation, interviews, and questionnaires, some oral, administered by native speakers, others filled out in writing. Her findings clearly indicate that as between Swahili and English, the former is neutral,

i. e. unmarked, while the latter is prestigious, i. e. marked. "When one African speaks to another in Kampala, chances are he is speaking some Swahili if the two do not share the same first language. When the situation contains opportunity to improve status, those who are able will use English." (p. 51). So only those who have gone far enough in school to learn English have any real choice.

The vast and rapid industrialization and urban development in places like Kampala has brought together a motley population. On the job most people have to work with speakers of other vernaculars than their own. Here the dominant language is Swahili, often intermingled with English (p. 59). For those who know English it is usual with Asians and invariable with Europeans (p. 64). But the most interesting case is its use with other Africans: always to those of higher position. The use of English is a means of "showing off"; one is trying to impress. To use it with fellow workers of one's own status would be offensive, a "putting down." Hence there is a third situation, in which one switches between the two languages. This satisfies both demands: that one be friendly and that one be competent.

The image of Swahili as a "neutral" language, which implies nothing about one's ethnic or socioeconomic status, is confirmed by Scotton's studies in Kenya (Nairobi), supplemented by her study of Pidgin English in Nigeria (Lagos) (Scotton 1976). These situations show that "neutral ethnicity" is a quality that adheres to these languages by contrast with the local vernaculars. The educated have the further option of using English as well, in some situations as a device of status-raising, in others of varying the social distance.

The trilingual situation in East Africa is remarkably reminiscent of my own, as I have presented it earlier. In speaking to a member of my own dialect community, I will use that dialect; it is the vernacular of my childhood and of my contacts with its speakers. Standard Norwegian, on the other hand, corresponds to Swahili as a neutral language with which I communicate not only with all other Norwegians, but also with Swedes and Danes (as apparently Ugandans do with Tanzanians and Kenyans). There is even an interesting historical parallel in that what I have been calling "Standard Norwegian" is a foreign importation into Norway, much as Swahili is in Uganda. There are good arguments for calling it "creolized Danish," and as is well-known, attempts are being made to replace it by a "New-Norwegian" based on the native dialects. The difference is that in Norway its lingua franca has been established as a standard language through its use over several centuries by the elite. It has thereby been raised to a language of status, with a social meaning much like that of English in Uganda. This explains my own use of it and my efforts to learn it, as well my adoption of it as the language of my textbooks.

Among Norwegian immigrants in America, however, the choice between vernacular and standard existed only as long as sermons and parochial schools were held in Norwegian. Then and later the major choice was between an American-Norwegian vernacular and American English, corresponding to the African choice between Swahili and English and the Scottish one between

Gaelic and Scots English. There is in Scotland, for some of the educated, the further choice between Scots vernacular and "Highland English" or even RP, whatever one considers most "status-raising." (Dorian 1981:84; cf. also Aitken and McArthur 1979).

Finally, we should emphasize that in none of the situations of language choice are we speaking of an absolute "either-or". Wherever possible there is a continuum, within which each speaker places him- or herself in relation to other speakers. Dorian has been particularly explicit about the nature of the Gaelic she has studied in east Sutherland. Not only is there constant switching of codes, but there is extensive transfer from one language to the other, inevitably from the more to the less prestigious. She has made a special study of what she calls the "semi-speakers" of Gaelic, a very valuable afterthought. As she did in the beginning, I rejected such semi-speakers after running across one or two of them; in hindsight one sees that they might have been very interesting. They represent that fringe of passive bilinguals whose memories retain much of the grammar and lexicon, but not enough to qualify as proper speakers. Scotton also mentions the frequency of switching from Swahili to English and of the borrowings in Swahili from English.

In short, the situations described by these scholars correspond very closely with those that are well-known in American immigrant language, as Swadesh foresaw. My dual bilingualism or triglossia appears to have its parallel wherever languages meet under conditions of unequal prestige on the part of the populations who speak them. Minorities are dominated by elites, and languages are means by which elites dominate minorities, making them uncertain of their own values and pressing or encouraging them to reject their old ethnicities, without necessarily granting them admission to or status in the new society.

Scotton and Dorian are in a sense looking at the problem of language choice from opposite ends of the spectrum. While Dorian is studying "language death", Scotton may be said to be studying "language birth." Scotton has recorded the trend toward the adoption of what is essentially a foreign, though easily learned, language. Except where populations have been decimated (cf. the famous case of Ishi), the "last" speaker is also a "first" speaker; what they have in common is language shift. Scotton says little about potential disappearance of the local vernaculars, but she notes a certain resistance by the Ugandans who have lost power in recent developments. Such vernaculars would be threatened if English and Swahili should become, singly or jointly, permanent lingua francas in Uganda. Both scholars are dealing with the dynamic aspects of language choice: why do people learn and use a second language, not their own? Dorian notes the importance of the social isolation of the fisherfolk as a factor in their linguistic maintenance of Gaelic (p. 102). Scotton shows how breakdown of isolation leads to shift. They agree that individuals differ in their acceptance of linguistic change. Both have found that innovations are emotionally charged, and that anyone who deviates from the norms of the

stigmatized group by adopting the dominant language will be "viewed as something of a traitor to his original group" (Dorian, p. 103). In eastern Sutherland such people are described as "proud." The identical term is used by one of Scotton's respondents: people "prefer using Swahili to avoid the suspicion that they use English because they are proud." (Scotton p. 120) Among the Norwegian immigrants terms were invented for the concept of "anglified": *engelsk-sprengt* and *yankeesprengt*. These were pejorative and carried the connotation of "stuck-up", i.e. "proud". (Haugen 1953: 476).

Both Dorian and Scotton account for some of their findings in terms of value, as I have done in notes earlier about the "language market." Dorian writes: "Parents and children agreed on the positive value of English and the negative value of Gaelic for the rising generation." (p. 105). Scotton goes back to a theory launched by George Homans (1958), perhaps derived from M. Mauss' *Essai sur le don* (1925), of social behavior as an exchange. Thibaut and Kelley (1959) saw human relations "in terms of a balancing of costs and rewards of the participants." (p. 102). Extending this to language choice, Scotton sees the choices made by weighing the relative costs and rewards incurred in that choice (Scotton 1972: 109; also Scotton 1976). She admits that it is difficult to test this hypothesis by assigning numerical values to such terms as "costs" and "rewards". In an informal way, however, we can all recognize the gross values assigned to languages, if only by trends toward their greater or lesser use. She has clearly anticipated my idea of the "language market", and it was only when François Grosjean pointed it out to me, that I became aware of the similarity of our ideas.

The major difference is that I give the idea of the language market a wider purport: I include the process of learning and its motivation as well as actual use in discourse. As for the further question of whether the outcome is inevitable, I agree with Dorian that it is not. The market value of a language can change, if the social norms of the speech community are changed. Within recorded history many languages have survived and even been born again, thanks to vigorous social and political movements that have rescued them from extinction. Israel offers perhaps the most spectacular instance, but Scandinavia also has its candidates, e. g. in Faroese and New Norwegian.

Summary. Language choices that lead to the "death" or the birth of languages, as observed by myself as a bilingual and by others who have studied the problem, are significant social decisions based on the speaker's view of the respective values of two languages or dialects on what may be called "the language market." These values, which ultimately involve the speaker's opportunities for living a good life as an accepted member of a community, will shift according to the circumstances of the individual and his relation to the community. In a relatively isolated community, a strong linguistic cohesion can develop, leading to a loyalty that will prevent language shift. The isolation need not be a physical one: as in eastern Sutherland it may be purely

social. But to the extent that the community is economically and politically dependent on the goodwill of a community with a different language, the value of its own language will fall and those who are upwardly mobile will be tempted if not forced to change languages. Learning and maintaining a second language is costly in time and mental energy, but its values far exceed the cost if one can use the languages to maintain contacts with both communities, thereby broadening one's experience and enriching one's life. Without that, language death is a foregone conclusion. But the "death throes" can be lengthy and painful, with individuals taking quite different positions on the problem. There is often a kind of senescence that leads to reduced mastery of its resources by its last "semispeakers", as suggested by Swadesh and confirmed by Dorian.

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Soziolinguistische Probleme in der Bundesrepublik Deutschland

Johann Knobloch
Universität Bonn

Meine sehr verehrten Damen, meine Herren!

Es hiesze, Eulen nach Athen tragen oder als Europäer in Japan Kimonos verkaufen zu wollen, wenn ich hier vor diesem internationalen Publikum die Absicht hätte, über die soziolinguistische Problematik im Sinne von Sir Basil Bernstein, Ulrich Oevermann und William Labov zu sprechen, Namen, die noch heute als Etikett für eine Entwicklung stehen, die in der Bundesrepublik Deutschland sehr lebhaft diskutiert worden ist und auch ihren Niederschlag in pädagogischen Masznahmen gefunden hat (Stichwort: kompensatorischer Sprachunterricht).

Ich sehe meine Aufgabe vielmehr darin, einige Schwerpunkte aufzuzeigen, wo sich das Eingreifen soziolinguistischer Erkenntnisse im Sinne einer Verbesserung menschlicher Lebensverhältnisse und zwischenmenschlicher Beziehungen auswirken könnte.

1) Soziolinguistische Implikationen orthographischer Regelungen

Es ist bekannt, dass Fragen einer Orthographiereform gerade für die deutsche Sprache eine grozse Rolle spielen. Die erste Orthographiekonferenz fand 1876, die zweite 1901 statt. Nach langjährigen Beratungen ist zu erwarten, dass wir in diesem Jahrzehnt zu einer internationalen dritten Konferenz nach Wien einladen können (während der Tagungsort der erwähnten Konferenzen Berlin war). Was steht an Problemen an?

Im deutschen Sprachraum hört man immer wieder: alle anderen schreiben die Substantive klein—warum machen wir das nicht schon längst nach?

Die Reformisten plädieren für die gemäzigte Kleinschreibung nach dem Vorbild der anderen Sprachen des europäischen Schriftenkreises, also der Buchstabenschrift semitischen Ursprungs. Die Groszbuchstaben sind hier zunächst als ein formschönes Alphabet für die Monumentalschrift entstanden und in einer Zeit mittelalterlicher Schreibgewohnheiten in erster Linie für die Hervorhebung religiöser Namen, aber auch der Kapitelfanfänge (Miniaturen) verwendet worden. In der deutschen grammatischen Lehre wird schliesslich der Gebrauch sanktioniert, alle Hauptwörter (sowie die Namen und die Satzanfänge) durch Groszbuchstaben herauszuheben. Eine solche Auszeichnung trifft (im Sinne der modernen Textsemantik) gerade das jeweilige 'Thema' im Satz. Beispiel: Ich komme gern, mein Kommen (meine Ankunft) ist aber noch nicht sicher. Beim Überfliegen der Spalten einer Tageszeitung ebenso wie beim suchenden Blättern in einem Buch wird gerade der Vielleser einen solchen

social. But to the extent that the community is economically and politically dependent on the goodwill of a community with a different language, the value of its own language will fall and those who are upwardly mobile will be tempted if not forced to change languages. Learning and maintaining a second language is costly in time and mental energy, but its values far exceed the cost if one can use the languages to maintain contacts with both communities, thereby broadening one's experience and enriching one's life. Without that, language death is a foregone conclusion. But the "death throes" can be lengthy and painful, with individuals taking quite different positions on the problem. There is often a kind of senescence that leads to reduced mastery of its resources by its last "semispeakers", as suggested by Swadesh and confirmed by Dorian.

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Es ist bekannt, dass Fragen einer Orthographiereform gerade für die deutsche Sprache eine grosse Rolle spielen. Die erste Orthographiekonferenz fand 1876, die zweite 1901 statt. Nach langjährigen Beratungen ist zu erwarten, dass wir in diesem Jahrzehnt zu einer internationalen dritten Konferenz nach Wien einladen können (während der Tagungsort der erwähnten Konferenzen Berlin war). Was steht an Problemen an?

Im deutschen Sprachraum hört man immer wieder: alle anderen schreiben die Substantive klein—warum machen wir das nicht schon längst nach?

Die Reformisten plädieren für die gemässigte Kleinschreibung nach dem Vorbild der anderen Sprachen des europäischen Schriftenkreises, also der Buchstabenschrift semitischen Ursprungs. Die Groszbuchstaben sind hier zunächst als ein formschönes Alphabet für die Monumentalschrift entstanden und in einer Zeit mittelalterlicher Schreibgewohnheiten in erster Linie für die Hervorhebung religiöser Namen, aber auch der Kapitelfanfänge (Miniaturen) verwendet worden. In der deutschen grammatischen Lehre wird schliesslich der Gebrauch sanktioniert, alle Hauptwörter (sowie die Namen und die Satzanfänge) durch Groszbuchstaben herauszuheben. Eine solche Auszeichnung trifft (im Sinne der modernen Textsemantik) gerade das jeweilige 'Thema' im Satz. Beispiel: Ich komme gern, mein Kommen (meine Ankunft) ist aber noch nicht sicher. Beim Überfliegen der Spalten einer Tageszeitung ebenso wie beim suchenden Blättern in einem Buch wird gerade der Vielleser einen solchen

Blickfang zu werten wissen.

These 1: Es wird viel mehr gelesen als geschrieben. Die Orthographie muss jedem Leser die grösztmögliche Verständlichkeit der Texte gewährleisten.

Ich habe, als Mitglied der österreichischen Kommission 1960 durch die freundliche Vermittlung von Prof. Wolfgang Fleischhauer in den Vereinigten Staaten umfragen lassen: die dortigen Deutschlehrer sind mehrheitlich für die Beibehaltung der Groszschreibung. Ich habe dann 15 Jahre später als Vorsitzender der gleichen Kommission durch die freundliche Vermittlung von Prof. Eijiro Iwasaki in Japan umfragen lassen—die Antwort war die gleiche: die Groszbuchstaben sind hilfreich für die Erlernung des Deutschen.

In Wien sind auf Betreiben von Prof. Eugen Wüster in einer Subkommission sprachpsychologische Versuche (Zusammenarbeit von Prof. Maria Hornung und Prof. Vanecsek) angestellt worden. Sie ergaben den signifikanten Wert von 5 Prozent für die Groszbuchstaben beim leisen, sinnerfassenden Lesen. Wenn ich die Möglichkeit hätte, dann liesze ich Caesars *Bellum Gallicum* mit groszgeschriebenen Substantiven drucken. Ich bin sicher, dass die Lateinkenntnisse der Sextaner davon profitieren würden.

Prof. Jean Zemb (Paris), bekannt durch seine kontrastive Dudengrammatik, hat vor einiger Zeit in Wien gesagt: in zwanzig Jahren schreiben wir Franzosen die Hauptwörter grosz. Er weisz um die Vorteile: Erleichterung der Sinnerfassung, Vereinfachung des elektronischen Information Retrieval. Die These des engagierten und kenntnisreichen französischen Linguisten sollte uns zu denken geben.

These 2: Es ist möglich, dass das Zeitalter des Computers auch der französischen und der englischen und allen anderen Sprachen unseres Kulturkreises die Groszschreibung der Hauptwörter (der Substantive) aufzwingt.

In den jahrelangen Diskussionen, die im ganzen deutschen Sprachraum geführt wurden und noch werden, hat man sich auf die Linie der 'gemäßigten Kleinschreibung' zurückgezogen. Freilich werden immer wieder andere Probleme erwähnt, so die laufende Angleichung der Fremdwörter an die deutsche Schreibweise. Hier muss scharf widersprochen werden.

These 3: Fremdwörter sind Schlüsselwörter für den Zugang zu den geläufigen Fremdsprachen. Es ist falsch, ihre Schreibweise verändern zu wollen, weil man damit innereuropäische Sprachbarrieren schafft.

Die deutsche Orthographiereform ist und bleibt ein internationales Problem. Nicht nur, dass—zuletzt in diesem Juni—Vertreter aller deutschsprachigen Staaten zusammenkamen und noch laufend weiter beraten. Deutsch als Verständigungs- und Verhandlungssprache, die Weltgeltung der deutschen Literatur, dies alles verlangt Berücksichtigung.

2) Hochsprache und Dialekt

Aus der vielschichtigen Problematik, die gerade für den Schulunterricht, die Sprachpädagogik einer sekundären Sozialisation, eine bedeutende Rolle spielt,

möchte ich hier einen bisher vernachlässigten Punkt herausgreifen. Vorausgesetzt sei, dass man den Dialekt in der Elementarschule nicht bekämpfen soll, sondern ihm Raum zu seiner Entfaltung geben muss—in der Freizeit des Schülers, als ein wichtiges Mittel, die Bindungen innerhalb einer Gruppe zu festigen. Die Bedeutung der Pflege des Dialekts hat der Innsbrucker Sprachforscher Hermann Ammann schon in einem Aufsatz hervorgehoben, der 1917 geschrieben, wegen des damaligen Papiermangels aber erst aus seinem Nachlass (1958) veröffentlicht wurde.

Das Anliegen, auf das ich hier aufmerksam machen möchte ist dies: es gibt keine orthographischen Regeln, wie Dialektwörter sinnvoll mit den Mitteln der hochdeutschen Orthographie wiederzugeben sind. Ich möchte daher gerade für die Gremien, die sich um Orthographiefragen bemühen, die Empfehlung aussprechen: bleibt auch weiter bei der Arbeit und wendet eure Aufmerksamkeit auch den jeweiligen Dialekten und ihrer Schreibung zu!

3) Die Haussprachen der Roma (Zigeuner) und ihr Verhältnis zu den Sprachen der Gastländer

Die Problemkreise, die hier zu behandeln sind, haben alle einen nationalen und einen internationalen Aspekt. Evident ist dies für das Volk, dessen verstreute Angehörige schon seit 500 Jahren unsere europäischen Mitbürger sind—die Sinti und Roma (Gypsies, Tsiganes). Sie werden als eigenes Volk neuerdings bei den supranationalen Organisationen anerkannt. Es ist verständlich, dass sie ein Volk mit eigener Sprache werden wollen und dass dies das Ziel einer langfristigen Sprachpolitik sein kann und soll. Dies Streben erinnert irgendwie an ein anderes uraltes Volk, dem aber die Einheit in der Sprache der religiösen Schriften erhalten geblieben war und das in unseren Tagen in seine alte Heimat zurückgekehrt ist.

Aber alle diese Vorbedingungen fehlen den Sinti und Roma. Ich kann hier nur das Problem der Schreibung kurz skizzieren. Die Ausarbeitung von orthographischen Systemen der Zigeunerdialekte kann nur auf der Grundlage der Schreibung der Sprachen ihrer Gastländer erfolgen.

Es ist besonders erfreulich, dass sich in den Reihen der Linguisten immer wieder Kollegen finden, die sich mit der Frage der Zigeuner und der Bewahrung ihrer Sprache befassen. Während es früher ausschliesslich Wissenschaftler der Gastvölker waren, die sich um das Erzählgut und um die Sprache bemühten, darf man heute bereits hoffen, dass gleichgerichtete Bemühungen auch von seiten der Zigeuner selbst, die sich zu Interessenverbänden zusammenschliessen, ausgehen. Freilich stehen dort zunächst dringendere Bedürfnisse, der Wunsch nach Beendigung der Diskriminierung und nach besseren Lebensbedingungen, vorrangig auf Tagesordnung. Aber mit der Hebung des Lebensstandards und mit den durch Sesshaftigkeit notwendigerweise vermehrten Kontakten werden sich auch bei den Zigeunern selbst Bestrebungen durchsetzen, Sprache und Kultur zu erhalten und die Verbindung der Stämme untereinander auszubauen. Bei der weltweiten Verbreitung der Roma taucht in diesem Zusammenhang

natürlich die Frage auf, ob es nicht zu einer Vereinheitlichung der Zigeunerdialekte kommen könne, ja, der Vorschlag, ob ein solches, bisher auf natürlichem Wege entstandenes Sprachsystem mit recht einfachen grammatischen Regeln nicht etwa in der Lage wäre, als Weltsprache das Esperanto abzulösen oder ihm in gewisser Hinsicht zur Seite zu treten, ist bereits gemacht worden.

Sprachwissenschaftliche Bedenken gegen ein solches Programm brauche ich hier wohl nicht eigens zu betonen. Ich möchte aber darauf hinweisen, dass man auch mit dem Wunsch einer gelenkten Vereinheitlichung über das Erreichbare hinausgeht. Als erste Aufgabe der Schulbildung der Zigeunerjugend kann nur die Vervollkommnung des Gebrauchs der Sprache ihres Gastlandes gelten. Dass daneben die Haussprache weiterhin der Romdialekt bleiben soll, entspricht der Analogie zur Empfehlung hinsichtlich des Schutzes und der Pflege heimischer Dialekte (im Deutschen). Es sollte aber von sprachkundigen Lehrern, die es dort, wo Zigeunersiedlungen bestehen, geben sollte und müsste, die Verschriftung der Rom-Dialekte angestrebt werden. Grundsätzlich sind dabei zwei Wege möglich. In der internationalen Rom-Linguistik hat sich eine mehr oder weniger vereinheitlichte Schreibung durchgesetzt. Es wäre nun falsch, diese auf die Alltagsbedürfnisse heutiger Romdialekte zu übertragen. Eigene Beobachtungen haben mir vielmehr ergeben, dass des Schreibens kundige Zigeuner, seien sie nun Roma oder Sinti, sich durchaus der Regeln der deutschen Schreibweise bedienen (also z. B. den *ß*-Laut mit unserem *sch* schreiben). Dies ist aber, wenn man hier die Sache im Sinne der Soziolinguistik (und nicht vom Standpunkt eines phonologischen Lautsystems aus) betrachtet, der einzig gangbare Weg. Eine Schulung in zwei verschiedenen orthographischen Systemen müsste zu ständigen Interferenzen, also Fehlern beim Unterricht der Gastsprache führen, was wohl niemand wünschen kann.

These 4: Die Ausarbeitung von orthographischen Systemen der Rom-Dialekte auf der Grundlage der Schreibung der Sprachen der Gastländer zählt zu den wichtigsten soziolinguistischen Aufgaben der Gegenwart.

These 5: Diese Aufgabe und ihre Lösung wird wesentlich dazu beitragen, dass die Akkulturation der Zigeuner als eine soziale Verpflichtung in unserer Generation, in diesem Jahrzehnt, vollendet wird. Die Entfaltung einer Zigeunerliteratur sollte von allen Gastländern durch zuständige Fachleute (nicht zuletzt aus der Linguistik) gefördert werden.

In diesem Zusammenhang darf in Dankbarkeit an die grossen Leistungen der englischen Gypsy Lore Society als ein Vorbild für ähnliche Organisationen in anderen Ländern hingewiesen werden.

4) Gastarbeiterkinder: eine verlorene Generation oder eine Hoffnung für die Zukunft?

Die Bedeutung dieses Problems gerade für die Bundesrepublik Deutschland ist allen Deutschen bewusst. Zur Information unserer nichtdeutschen Kollegen sei mir die Wiedergabe einer Zeitungsmeldung aus einer Bonner Tageszeitung

vom 8. Oktober 1981 gestattet.

Deutsche Grundschul Kinder sind zum Teil schon eine Minderheit. Konzentration von Ausländerkindern führt zu einer neuen Ghettobildung.

Auf die Gefahr einer erneuten 'Ghettobildung' bei Ausländerkindern im Bereich der Grundschulen haben die SPD-Stadtverordneten Franz Stadelmeier und Waltraud Christians sowie die SPD-Fraktion im Schulausschuss hingewiesen. In einer grossen Anfrage wollen sie von der Verwaltung wissen, ob es zutrifft, dass der Ausländeranteil an einigen Grundschulen (Marienschule und Robert-Koch-Schule) mehr als 50, zum Teil sogar mehr als 75 Prozent beträgt...

Was ist bei diesem Sachverhalt zu tun? Jedem von uns ist klar, dass es sich hier um ein eminent soziolinguistisches Problem handelt. Wenn diese Kinder nicht in das Abseits einer verlorenen Generation gedrängt werden sollen, müssen erforderliche Massnahmen ergriffen werden, wobei es sich vor allem um den Einsatz von geschulten Fachkräften handelt. Über die Universitäten hinaus, an denen Kurse für die erforderlichen Sprachkenntnisse angeboten werden, schalten sich zusehends auch die regionalen Volkshochschulen ein, deren Leitungen allerdings durch die derzeitige Restriktion der finanziellen Mittel bedrängt werden.

"Laut Erlass des Kultusministers kann den Kindern ausländischer Arbeitnehmer an weiterführenden Schulen die Amtssprache des Herkunftslandes als erste oder zweite Fremdsprache statt der üblichen Pflichtfremdsprachen angeboten werden, sofern die organisatorischen und personellen Voraussetzungen es zulassen."—Ich glaube, dass es unsere Aufgabe ist, diese personellen Voraussetzungen zu schaffen. Wenn rings um uns ein neuer 'babylonischer Turm' mit seinen Folgeerscheinungen, den Kommunikationsbarrieren, entsteht, können wir Linguisten nicht in unserem 'elfenbeinernen Turm' unseren früheren Lieblingstheoremen allein huldigen.

Die öffentliche Diskussion über die Gastarbeiter und ihre Kinder wird von verschiedenen Positionen aus geführt. Hier ist nicht der Ort, dies weiter darzulegen. Ich möchte aber darauf hinweisen, dass kein Grund für eine pessimistische Beurteilung der Lage gegeben ist, sofern nur für eine Entfaltung der frühkindlichen Zweisprachigkeit der jeweiligen Sprachgruppe einerseits und den Grundschulen andererseits die erforderliche Förderung zuteil wird. Dann wird sich bald erweisen, dass diese heranwachsenden Jugendlichen nicht heimatlos und ohne Orientierung dastehen, dass sie nicht etwa, wie man so sagt, die Muttersprache verlernt und die Sprache des Gastlandes nicht erlernt haben, sondern dass sie vielmehr durch eine gute Beherrschung beider Sprachen eine Brückenstellung einnehmen und für eine Aufgabe bereit sind, wie sie ja Ziel der jeweiligen bilateralen Kulturabkommen ist.

These 6: Die Situation der Gastarbeiterkinder ist nicht bemitleidenswert, sie bietet vielmehr die Chance einer bilingualen und bikulturellen Ausbildung.

These 7: Die soziolinguistische Forschungsrichtung innerhalb der moder-

nen Sprachwissenschaft im Verein mit der Psycholinguistik ist in der Lage, Empfehlungen und Programme auszuarbeiten, wodurch es ermöglicht wird, den Kindern von Gastarbeitern hinsichtlich ihrer Schulprobleme die erforderliche Obsorge zu widmen.

5) Das Problem sprachlicher Minderheiten in Europa

Das Problem der Gastarbeiter hat im Verlauf der letzten Jahrzehnte den Blick der bundesdeutschen Öffentlichkeit geschärft für eine Sachlage, wie sie an den überlappenden Grenzen der Sprachräume in Europa schon immer zu beobachten war. Man mag an die Österreichisch-Ungarische Monarchie denken oder an Jugoslawien oder an die Union der Sozialistischen Sowjetrepubliken: jeweils taucht die Frage des Zusammenlebens, belastet mit sprachlichen, ökonomischen und sozialen Problemen, auf. Wie die leidvolle europäische Geschichte gezeigt hat, gibt es kein Allheilmittel, keine Patentlösung. Und doch ist die Aufgabe des Minderheitenschutzes (und das ist: Schutz der Muttersprache) eine Verpflichtung, die nicht dem einen Staat vertraglich auferlegt wird, während sie andere—das lehrt uns leider die Vergangenheit—mit tödlichen Folgen missachtet haben. Es gehört zu den Aufgaben der Friedenssicherung, dass sich die verantwortlichen Politiker um alle Gefahrenherde kümmern. Umso besser, wenn sie sich dabei auf gesicherte Ergebnisse einer internationalen soziolinguistischen Forschung stützen und ihre Ergebnisse nützen können.

These 8: Durch Sprachenfrieden im Innern kann der Völkerfriede gesichert werden.

6) Sprachwissenschaft als Zweckforschung

Motto: Ich wuchs unter Weisen auf—ein Leben lang. Und fand nichts Besseres als Schweigen. Nicht das Forschen ist wichtig—sondern das Tun.

(Aus der Sammlung jüdischer Aphorismen: Sprüche der Väter)

Und so sollten wir, bei aller Beachtung, die eine zweckfreie Forschung verdient, angesichts der Weltwirtschaftskrise uns mit den brennenden Problemen der sprachlichen Gegenwart befassen. Denn die allgemeine Sprachwissenschaft wird an dem Nutzen gemessen, den sie als angewandte Forschung für die Allgemeinheit erbringt.

Mein Wiener Lehrer Wilhelm Havers mahnte uns, bei der Beschäftigung mit Sprache nicht den sprechenden Menschen zu vergessen. Das gilt heute ebenso. Lebendige Anteilnahme am Sprecher muss die soziolinguistische Forschung motivieren und legitimieren.

Ich möchte nicht missverstanden werden: gründliche und gründlichste Forschung ist die Grundlage unseres Mühens, aber nicht Selbstzweck.

Sociolinguistics: The Past Decade

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In keeping with the general theme of this Congress and its theoretical thrust, this report will treat of the underlying assumptions of the study of language in society as practiced in the past decade and of their implications in recently published research.

The study of sociolinguistics is based on the premise that there can be no society without language and that there can be no language without society. Only people speak and they speak to one another. This main social function of language has in the past been the basis of theories of language, ranging from Saussure's notion of a linguistics to be integrated into an as-yet-non-existent science of signs in society to Firth's people-centered and context-oriented analysis of human speech. It has also been argued that since language, as a basis for social communication depends on the existence of linguistic signs and that these in turn depend on a system of mental representation, such a system must be the basis of all language study. Man, as species must be biologically imprinted with a potential for such representation and for a potential for speech. Such mind-centered theories, however, are not really incompatible with a sociology of language. They simply have not yet been integrated into the biological theories of society such as sociogenetics (the study of the evolution or behavior and of the hypothesis that genetics controls both potential and performance) and sociobiology (the study of the structures and dynamics of social groups)¹.

During the past decade, much of the debate over sociobiology as an integrating discipline in the social sciences has centered around the relative strength of acquired as opposed to hereditary social abilities. It has been argued that ethnicity, which includes language, is essentially biological by nature. Mankind has evolved a basic drive toward ethnicity which has become both biologically and ecologically imprinted².

Yet the societal component of language study is as old as the idea of a standard language—particularly when that standard, often the speech of the king and his court, had to be reduced to writing. Some historical linguists,—Meillet for instance—had seen language in its social context, while sociologists like Durkheim were concerned with the role of language in society³. It is only in the past few decades that the study of language in society has been divorced from "linguistics", which had become pure and formal. Likewise the study of the role of society in language-change, as it conditions variables in speech, has developed separately.

Most of the sociolinguistic studies of the past decade were the outgrowth neither of linguistic theories of language nor of social theories of society. The rapid growth of sociolinguistics has been a response to divergent demands—in developed and undeveloped countries. In the developed world the rapid leveling or regional dialects through post-war mobility, nation-wide media and accelerated urbanization attracted the attention of descriptive linguists to the study of social dialects—mostly urban dialects—co-existing in the same region. The need to delimit and co-relate the social components of these sociolects resulted in the development and refinement of methods in sociolinguistic dialectology⁴. In the developing countries the great increase in the demand for solutions to national language problems and the dilemma of language in education was in part responsible for the phenomenal increase in the number of studies on the sociology of language, especially as it affected language policy⁵.

Yet for most of the period such studies remained largely discipline-oriented. The societal determinants of language varieties were often regarded simply as a new dimension in fashionable linguistics. The latter, having graduated from the phoneme to the sentence and from the sentence to the conversation, was now going to proceed beyond, to the analysis of communication as such. The more analytically oriented linguists turned to the study of the minutiae of social interaction, so detailed that no generalizations were possible. Psychologically-oriented linguists and social psychologists added to the collection of studies labeled "sociolinguistics" with measures of language-related attitudes and values. In this way, the repertoire of "sociolinguistics" was put together from products appearing on the fringes of such disciplines as social psychology, social dialectology and social anthropology. By mid-decade, the field had incorporated so much material that it began to generate a life of its own—in the process, engulfing such young disciplines as ethnolinguistics, exolinguistics, demolinguistics, glottopolitics, language planning and others. During this period it evolved from being programmatic to data-oriented, from sociographic to quantitative and theoretical. It became much more comprehensive than "linguistics" and more disciplinary. At the same time, formal linguistics tended to become more pragmatic and inclusive, tending toward a general theory of language by taking into account its uses, and the role of context. Since context depends on social relation, the line of demarcation between what constitutes sociolinguistics and what does not had become even fuzzier at the end of the decade.

The underlying assumption of sociolinguistics is that there exists a relationship between the groups to which people belong and the way they express themselves. The groups to which people belong, by choice or by necessity, vary in size and nature and range from the close-knit family to work groups, voluntary organizations, professional societies, ethnic groups, nation-states, and multinational societies⁶. The determinants of these groups, may be genetic, regional, religious, economic, cultural or some combination of these and other social characteristics. The life of such groups and their very social existence depends on the possibility of interpersonal communication, and this, to such an

extent that the modes and means of such intercommunication themselves become characteristic of the group. Such characteristics of expression, as language, dialect, accent or lexicon, may sometimes be considered as the sole determinants. It is as if the consensus of the group were based on such a reasoning as: Here is someone who speaks like us. Therefore, this person must be one of us.

One assumption of sociolinguistics has been that such groups are identifiable. This itself is now debated. The groups depend on the structure of the society to such an extent that it becomes difficult to generalize from one society to the next. Society itself is only an abstract category. People do not depend on this abstraction. They depend on other people and this dependence may affect their language behavior. It is therefore such social variables in behavior that must be observed and explained. Such a grouping of interdependent people has been conceived as a network whose working and intercommunication may be analysed by appropriate methods⁷. In the last analysis, the distinction is a philosophical one. It is the problem of the existence of categories and of their comparability⁸.

Sociolinguistics postulates the existence of both social categories and language categories and the possibility of studying the relation between both, that is, the relation between such social categories as ethnicity, culture, occupation, heredity and education and such language categories as dialect, accent, vocabulary, register, and type.

According to our starting point in the analysis of this relationship and the direction taken, the theoretical issues in sociolinguistics during the past decade may be studied from two angles, the language determinants of social grouping, and the social determinants of language behavior. Let us first examine the language determinants of social groupings. Each grouping may vary along the dimensions of space (territoriality) and time (stability), the variations being affected by such social forces as mobility, exogamy and acculturation, the latter ranging from incipient to residual bilingualism. The nature of such groupings, however, depends on the structure of the society, which may vary in complexity from the nomadic tribe to the industrial state. In primitive societies individuals may belong to a single and only group of all their needs. At the other extreme, in a highly complex urban society, persons may belong to a different group for each social need.

A common mode of speech may unite societies as small as a clan; or it may be the bond that links populations extending across many national frontiers. The French language, for instance, is the unifying force behind a cultural commonwealth that embraces a great variety of nations, races, cultures and religions⁹.

Since the possibility of linguistic intercommunication is the primary prerequisite for the existence of any social group, it may be argued that any increase in the availability of communication facilities would be followed by an increase in the number and types of social groups. In fact, this is what has been happening during the past century—especially during the past few decades,

and at an ever accelerating rate.

In every quarter of the globe, primitive societies are becoming features of the past. Rural, stable and simple family-oriented societies have been rapidly evolving into urban, mobile and complex social structures—even in those parts of the world where we used to go to study the working of primitive societies.

One result of this evolution from the isolated traditional society to the mobile, urban industrial megapolis is that we are now polysocial, belong to two or more groups for work, trade, profession, recreation, culture, schooling or religion. To the extent that any of these groups functions in a different language or language variety, people by becoming bi-social must become bilingual and diglossic in language use.

The phenomenal increase in the urbanization, mobility and possibilities of interpersonal communication has transformed some societies into groupings of multisocial individuals. In the process many have, of necessity, become multilingual. In some parts of the world, the very act of attending school supposes the acquisition and use of a new and unrelated language; there, to become educated means to become bilingual.

Not only do people belong to a number of different groups for different purposes but they may belong to each with a different degree of intensity, both as regards commitment and investment. Identity is dependent on the structure of the community, its stratification, degrees of differentiation and group solidarity. Language is an indicator of social status. If the identity is ethnic, status may be achieved through various types of language planning. Such status modification has been attempted in officially bilingual states like Belgium and Canada, and to a lesser extent among non-official language minorities¹⁰.

In developed nations, the requirements of standardization, of norms and behavioral systems of the post-industrial society, with the consequent losses of identity and individual differences, has generated a reaction against conformism, coupled with a quest for community, for cultural identity and self identification through language¹¹. Contrariwise, in the developing countries, ethnic identity has become associated with inherent limitations in living standards and national potential in science and technology. The trend has been toward a cultural of supra-national identity. And it has become associated in some areas with such supra-national groupings as the "Francophonie".¹² In other parts of the developing world the English language has served as means of supra-national cultural identity, determining who exercises power. The term "Afro-Saxons" has been used to identify this English speaking trans-national African elite.¹³

There may admittedly still be societies where the population can be classified as if all people were permanently in one pen or the other. But there are also societies with a multiplicity of groupings for different purposes some more stable than others—each grouping or occasion for association or belonging being

available to each person at certain times and under certain circumstances. Attempts have been made to describe such language communities—particularly the bilingual ones characterized by the co-existence of two official major standard languages—like German and Italian in Alto Adige¹⁴, Dutch and French in Brussels¹⁵, French and English in Montreal¹⁶. These are in contrast with newly revived regional language groups like the Catalan¹⁷, and the Basque¹⁸ where the administrative elite is more bilingual than the majority they administer. Numerous studies have been made on how states have legislated to protect minority language use¹⁹. The consequences of importing formulas from abroad have also been studied²⁰.

Belonging to any language group however, may simply be a matter of degree—at least, that is what recent investigations would seem to indicate. Language minorities, far from being isolated entities, may be part of a wider social system. This seems especially to be true in industrial and post-industrial societies, where the necessity of interaction with the majority presupposes the use of another languages²¹.

In the past decade researchers have also theorized on the determinants of ethnolinguistic identification. Three determinants have been agreed upon—paternity, patrimony and potential. In other words, both ethnic origin, and cultural maintenance can determine ethnic identification, which becomes functional only if the potential can be manifested through existing supportive institutions. In addition to trying to isolate the determinants of ethnic belonging, researchers have examined the process of language identification through the use of variants of regression analysis (path analysis), starting with the ultimate dependent variable and working back to possible predictors²². Secondly, sociolinguistic theorists have studied the social determinant of language behavior in the context of communications theory. In the past decade, much attention has been paid to the notion of the ability to make oneself intelligible, and to the idea of communicative competence²³. Such competence implies that speakers can function in the language; it does not however assume grammatical or phonetic correctness, but rather the ability to initiate conversations, to argue, to convince, to refuse politely and the like, according to situation, topic and participant²⁴. Social behavior in language use has also been expressed in terms of norms and rules. The model has been called deterministic—as if an abstract construct (society) made “rules” to which all had to conform²⁵.

Social control however, has been seen as the price one had to pay for interpersonal intelligibility. This is achieved through limiting the language freedom of the individual by cutting down on his options. The fewer the options the greater the standardization; the greater the standardization, the greater the potential intelligibility, and the greater the social control of language. The more the options, the less potential interintelligibility; but the greater the freedom of choice in language behavior.

In linguistically monolithic societies like those created by long stable nation-states any deviation from the norm is either not understood or not tolerated.

Strangers are expected to conform. In such societies, language standardization constitutes a form of behavior control in the use of the written word, where spelling mistakes may be universally condemned. Contrariwise in multilingual societies there may be a rapidly evolving norm, permitting the individual to adjust to each situation to achieve certain objectives, speaking one way to give one impression (like wanting to conform to outside job requirements) and another way to give the impression of solidarity with the people of the area²⁶. Thus, pluralistic societies may survive through tolerance of free variation.

Interintelligibility depends on a wide range of differences in competence between each individual's idiolect and the capacity for understanding those of others. A high degree of non-reciprocal multidialectalism is the norm.

Such observation have seemed to confirm the theory of language accommodation based on research into similarity attraction, whereby individuals can induce others to evaluate them better by reducing dissimilarities²⁷. This reduction means an expenditure on the part of one or of both parties for which a return is expected. The relationship has been expressed in terms of exchange theory²⁸. The extent of accommodation required to make one's speech acceptable in any given type of situation depends on variables of person, place and type, related to age, sex, education and ethnicity²⁹.

Accommodation theory is concerned with the processes of convergence, divergence and complementarity in social interaction. Speech convergence is a device by which persons make themselves more similar and more likable to others by reducing linguistic and paralinguistic dissimilarities. Beyond this, the observation that most languages contain systems of interpersonal accommodation gave rise to the possibility of describing a language on this basis³⁰. Most languages, for example, have registers of polite speech the purpose of which is social accommodation³¹. Another example is the change one adopts in talking to children, or the register used for foreigners³². When contact involves members of different language groups, however, there is at first a search for dimensions whereby each may be seen as distinct from the relevant outgroups, but positively so. According to the theory of intergroup processes, the different groups compare each other according to attributes important to their respective value systems³³. This may lead not to convergence but to divergence in language behavior, a type of behavior characterized by the "revolt of the minorities" during the past decades. Tired of adjusting to the language of others, a language minority may diverge in behavior, asserting its right to be different and obliging others to try to understand them. This has been noticed in Quebec after the Quiet Revolution, in the Arab world in the mid-seventies and in the appearance of counter-standards in unilingual states³⁴.

Thirdly, the accommodation may lead to a relationship of complementarity which linguistically reflects differing social roles, such as those of master-servant relationship³⁵. In bilingual communities, such accommodation may result in a degree of language alternation or code-switching³⁶. Whether all

such alternation is socially motivated is a moot question.

Dialect complementarity has been studied in the context of urban language surveys. The concept of a social dialect (sociolect) or speech variety has been refined in this context³⁷. Studies of sociolects have also been made in England³⁸, France³⁹ and the Netherlands⁴⁰, and other areas. The standardization and reconstruction of social dialects has often been a particular preoccupation of sociolinguistics in the Soviet Union⁴¹. There have also been a number of descriptive studies on how and why standard speech has actually evolved following urbanization, mass media, mass education and democratization⁴².

In many industrial states the massive influx of millions of foreign workers has given rise to the creation of other language communities whose sociolinguistic characteristics have become the objects of study⁴³. When the focus of observation was to change from that of people to that of the language varieties they use in types of social interaction, the theories are likely to be modified in so far as the interest affects person, place, purpose, function, and time.

Fishman's basic and often quoted question 'who speaks which language to whom' has engendered a good number of descriptive studies in a number of different countries. To answer this question is to predict the life and death of languages. For all theories on language survival relate to "the users and uses of language"⁴⁴. Other theories in turn depend on data, which, for the most part, are lacking. Very little is known about what social forces or combination or proportional distribution of forces is sufficient to change the direction of language use⁴⁵. So little, in fact, that any survey of language use may have potential theoretical significance. But such surveys are so costly that only governments who need them can afford them. The need arises from political urgency for modifications in language policy. For example, results of the careful and extensive survey of language use in Quebec were used to determine some of the key elements of Quebec's language policy. The findings demonstrated that the language of work was, in the last analysis, the decisive element in the survival and valoration of the French language in that part of the world⁴⁶. Similarly in Ireland, an extensive national survey of Irish language use, commissioned by the government, supplied measures of the complex interrelationships between language competence, language attitude and language use⁴⁷.

A number of other surveys on language use became necessary as a basis for the elaboration of language policies made possible as some of the language minorities of the Fourth World (the stateless nations) gained some measure of regional autonomy. One of the most extensive of these was the survey of Basque usage in Spanish Vasconia⁴⁸. The survey also strengthened the policy of the Basque Language Academy in the language planning (standardization and terminology) necessary for the implementation of any future language policy.

Similar sociolinguistic work had been in progress in Catalan areas long before the obtaining of local cultural autonomy in Spain. Observing the changes in language use in Catalan sociolinguists supplied a number of original theories

on the social psychology of language⁵⁰. Some have used the data as a basis to elaborate general theories on language power and prejudice⁵¹.

Surveys in other areas were related to language revival ideology. The Frisian language movement, backed by the Frisian Language Academy (Fryske Akademy) has generated studies of language attitudes and language use with some comparative studies of other minorities⁵².

The effects of the mid-century revolution in communications and media were being felt in the past few decades by official minority language groups in highly developed bilingual states like Belgium, Canada and Finland. Realizing the rapid decline in the use of highly developed languages like French and Swedish in minority status relationship, the speakers demanded to know the reason why. This demand instigated some careful quantitative surveys aimed at testing various hypotheses. For example, in Finland a research group on the relationship between ethnicity and mobility has produced a series of studies on the language consequence of both vertical and horizontal mobility in the bilingual population. For the Swedish minority, emigration to cities like Helsinki produced a regressive bilingualism, which, through the language demands of intermarriage, results in the eventual decline of the Swedish-speaking population⁵³.

The findings of this research group also cast doubt on the ethnic or language-centered model assumed in most policy-oriented studies. It has demonstrated that in situations of contact between two languages groups it is necessary to take the existence of a third group into account,—namely the bilinguals⁵⁴. It seems that some of the problems in the implementation of language policy therefore stem from the presence of ethnic bilingualism maintained through a functional distribution of language uses in the community—but seldom taken into account⁵⁵.

In other areas, where no language revival movement had existed, the fate of the regional language has become the concern of scholars. In Galicia, for example, the fact that Galician had not been used as a school language has become a matter of concern for its survival⁵⁶.

Social problems of regional languages in post-industrial societies of Europe are not comparable with those of the multilingual areas occupied by rural primitive societies in Asia, Africa, and Oceania, some of them harboring hundreds of mutually incomprehensible tongues within restricted areas which, in the past few decades have become dependent on the modern world. This new dependence has changed the social function of the indigenous languages, the attitudes towards them and the consequent development of new types of *lingua franca*⁵⁷.

In other areas of Africa and Latin America the co-existence of partially incomprehensible varieties of different languages produces a type of speech mixture where any comprehensible combination becomes acceptable⁵⁸. The very existence and feasibility of such areas seems to have cast some doubt on the theoretical foundations of synchronic linguistics⁵⁹. Who speaks which language

to whom, it seems, also depends on where the speaking is done. In industrialized states whose wealth depends on interactional trade, the language of the customers may have been taken into account in the development of national language strategies⁶⁰. Independent of the real number of their speakers, languages like German and Dutch may be strong in one part of the world, but weak in another. Studying the geographic determinants of such language use has given rise to a new and growing discipline called geolinguistics⁶¹.

Studies have been produced on the relationship between language geography and social development⁶², the social psychology of some language frontiers has also been quantified⁶³, the viability of bilingual districts has been studied in terms of both human geography⁶⁴ and in terms of socio-political models⁶⁵. Study of the determinants of language borders however are to be distinguished from descriptions of language behavior along such borders—especially traditional ones such as exists in Alsace-Lorraine, Eupen and Malmedy, and in the American Southwest. For such studies the term "border linguistics" has been used. There have been studies of such language usage in the German-Wallon district⁶⁶, the Luxemburg area⁶⁷ and that of the Dutch-Wesphalia border⁶⁸ in addition to studies of the Danish-German language border, among others.

Large scale language use surveys have also been focussed on large metropolitan areas. One of the first such studies was the survey of language use and language usage in the French city of Orleans the results of which are still being analysed⁶⁹. The Orleans study is a survey based on a structured sample of citizens with the purposes to find out exactly what sort of French was actually used by people of different social roles in different everyday circumstances. Another type of urban language study was the one undertaken in the city of Brussels. In this study of urban bilingualism and language use a number of scholars from various disciplines—psychology, sociology, political science, linguistics, history and geography were asked to study a particular aspect of language use in Brussels. In periodic symposia (every three to five years) attempts have been made to arrive at a synthesis or a theoretical consensus on the results⁷⁰.

A more precise focus of the study of language use has been the social institution. The most important of these has been the school. A vast literature has been produced during the past decade—especially in the area of bilingual education⁷¹. There have also been comparative studies on the bilingual university⁷². Other important social institutions, like the courts, have been studied from the point of view of language use⁷³.

In addition to the where and the who of language variation in society investigations have been made into how languages survive in societies, how others become extinct, and how some can even be revived.

Survival of languages and dialects, like *Plattdutch*, Pennsylvanian, Piedmontese, South-Schleswig Danish, Westerland Frisian, Friulian, Ladin, Roussillon Catalan, and Gorizian Slovenian—to name only a few—have become

causes of concern and objects of study. Languages and dialects like these, situated as they are in modern industrial societies have been slated for revival at the very time they have become most threatened with extinction. The threat has come, not from persecution, but from the economic benefits of industrialization, mobility, and urbanization. The implications for Western Europe have been studied on a comparative basis⁷⁴. Various studies on some of the language minorities have also been documented⁷⁵. In bilingual states like Canada, Ireland and Switzerland, the central government has taken a hand in the maintenance of official language minorities⁷⁶.

The central state has at its disposal means of mass communication often unavailable to language minorities. These may be used for the promotion and maintenance of non-official language. The proposed use of the Canadian satellite Anik for Eskimo programs in the Canadian north is a case in point. Nations have pooled technical resources for the mutual promotion of the languages of their respective minorities. Thus the trans-national Scandinavian satellite Norsat can relay Swedish television to Swedish minorities in Finland, and Finnish television to Finnish minorities in Sweden⁷⁷.

How best to guarantee the survival of minority languages has been the subject of numerous symposia. The theoretical framework for such discussions has encompassed such distinctions, as *Abstand* languages and *Ausbau* languages⁷⁸ and policies such as status-planning and corpus-planning⁷⁹. Other models have been proposed by Haugen, Rubin and Fishman⁸⁰.

A recent development in the promotion and maintenance of minority languages has been the official internationalization of languages and language groups. International language treaties recognize language boundaries as being distinct from political ones. An example is the treaty between Belgium and the Netherlands whereby a Dutch language area is recognized as belonging to both countries. It includes a policy for the re-unification of the Dutch language.⁸¹ Even in areas covered by different but related languages, language policy has been based on areas of common interest, aided by partial interintelligibility⁸². A separate Nordic Language secretariat has been created, for example, for Scandinavia⁸³.

Lessons for language survival have also been gleaned from descriptions of how languages become extinct. The very few available studies of language death therefore have become notable. Research has been done on the decline of Breton⁸⁴ and on the causes of the extinction of a dialect of Scots Gaelic, which is revealing in its detail⁸⁵.

Just as rare are documented cases of language revival. The most successful example is surely the revival of Hebrew as a national tongue; it has already been well documented and indeed often cited by would-be revivers of other tongues. Studies on the revival of such languages as Basque, Breton and even Cornish have so far been largely programmatic.

Most of the programs for language revival have to do with increasing the number of language functions, the purposes and areas for which the language

may or must be used. The first step may often be the creation of a diglossic social structure. Relevant to status planning are studies on the functional distribution of languages in the communities. Also relevant have been the studies on the allocation of social function for each language within the community⁸⁶. In Quebec, for example, by September 1982, all public signs must be in French only, according to language implementation regulations based on the official language policy.

The functional distribution of written languages may create a sort of literary diglossia⁸⁷. Theoretically, the distinction between language and dialect, if one can be made, is less relevant than the functional distribution of language varieties in standard and regional languages⁸⁸. Secondly, the creation of language functions has been made possible by the establishment of language rights. Relevant studies of the creation of language rights have been made for countries such as Canada, Finland, a number of African countries, Netherlands, Quebec, Switzerland and the United States⁸⁹. There have also been a number of useful comparative studies⁹⁰. The history and relative success of language policies and language treatment has also been documented for a number of different countries⁹¹. So has the impact of social context on the relative success of such language policies⁹².

In the context of nation-building, language policy often takes on the form of behavior modification. In the creation of monolithic states, the elimination of language varieties becomes a leading national priority. Since all are equal, all should speak the same language, to be propagated through universal education and public schooling for social equality. This has been the main preoccupation of most revolutionary regimes⁹³. The demands of a central administration can also promote social re-organization through the application of a national language policy⁹⁴. The idea of creating a classless language for a classless society has led to the use of ideology as a framework for theorizing on the functions of language varieties in society. Writers have theorized on the relations between language and colonialism⁹⁴, language and power⁹⁵ and language and social influence⁹⁶.

In contradiction to the practice in most socialist states where a standard language is regarded as a force for equalization, Marxists linguists in the West have portrayed it as an instrument of repression⁹⁷. Yet they have been consistently critical of Bernstein's well-quoted theory on the elaborate middle-class code which guarantees economic success⁹⁸. Much of this ideological polemic, however has been long on theory and short on fact—especially historical ones. This lack of historical perspective is characteristic of much sociolinguistic theory⁹⁹. There are nevertheless notable exceptions which bear citing. Several of the studies of the Brussels Centrum have documented the history of Dutch language use in that city¹⁰⁰. The study of the use of French and German in Switzerland before the First World War is another example¹⁰¹. A social history of speech usage in 18th century Germany is also worth noting¹⁰². Much of the material available on sociolinguistic history, however, must be gleaned

from the external history of various languages. Contrariwise, references to titles devoid of historical perspective would number in the thousands—as would any survey of sociolinguistics in the past decade. I have here referred by way of example to only a few studies. There may be important gaps which I should like to know about and fill in.

The foregoing must be regarded simply as a brief overview of the theoretical dimension of the sociology of language in the past decade, with examples from recent research. I hope that most of the important issues have been raised and that the few titles cited below as references are representative.

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Summary of the Plenary Session on Syntax and Semantics

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0. ARRANGEMENT

There were three reporters for the first plenary session: Professor Talmy Givón, Professor Susumu Kuno, and Professor Masayoshi Shibatani. Since Givón's paper and Shibatani's paper had more in common with each other than either had with Kuno's paper, the order of presentation was Givón, Shibatani, and Kuno. Each reporter was given twenty to thirty minutes. In addition, with the approval of the organizing committee, I asked Dr. Alec Marantz to be the discussant of Givón's and Shibatani's papers. Marantz's very interesting dissertation presented to the MIT in 1981 entitled 'On the nature of grammatical relations' takes up problems related to the ones discussed in Givón's and Shibatani's papers from a different angle. I also asked Professor Ellen Prince to be the discussant of Kuno's paper. She had written some interesting papers on discourse phenomena. The time allotted to the discussants was ten minutes for each paper. Questions and comments were invited from the floor after each discussant's presentation.

1.1 GIVÓN'S PAPER

Assuming that the participants in the Congress had read his paper contained in the *Preprints of the Plenary Session Papers* (henceforth *Preprints*), Givón devoted most of his time to stating his basic assumptions or principles reflected or implicit in his *Preprints* paper. He stressed the importance of the study of discourse function and pragmatic factors coupled with cross-linguistic typological study, the neglect of which by Chomsky and others, he says, made it impossible to ask the real interesting question, 'What is natural/non-arbitrary about the coding relation in syntax?' According to him, there are two linguistically-coded functional areas in language (apart from lexical semantics), namely propositional semantics and discourse pragmatics. The former deals with propositional information about events/states/actions, etc., with agent/patient/etc. associated with them. The latter deals with sequencing of successive propositions in discourse. The syntactic structure of a sentence must code both the propositional-semantic and the discourse-pragmatic information associated with the proposition. This will lead to the need for a structural compromise by which neither function is coded perfectly but both are coded sufficiently when context is taken into account.

He then delineates the notion 'functional domain', which plays an impor-

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tant part in his framework. Functional domains are scalar and multi-dimensional. They are defined structurally, on the one hand, by exploiting natural iconicity. On the other hand, examination of texts with a view to categorizing functions will be needed for establishing functional domains. These two approaches must complement each other. He illustrated these points by referring to relevant parts in his *Preprints* paper.¹⁾

1.2. MARANTZ'S COMMENTS ON GIVÓN'S PAPER²⁾

Marantz's discussion was directed to two things: (1) He tried to clarify the role of functional principles in the explanation of syntactic phenomena; (2) He also presented what he believed to be a superior analysis of Givón's UTE data, the description of which lies, according to Givón, at the core of his *Preprints* paper.

(1) Marantz claims that a functional explanation of syntactic principles like the ones proposed by Givón can successfully work only in conjunction with a theory of language change, development, or acquisition. He also claims that at present he does not know any convincing functional explanation for any interesting syntactic principles. As an example, he takes up Principle (9) given by Givón in his *Preprints* paper.

(9) Principle of syntactic coding of transitivity: 'To the extent that the transitivity of a clause is coded by its case-marking, the case-marking of the agent plays a less important role in the coding of transitivity than the case marking of non-agent arguments.' Givón, *Preprints* paper, p. 11; Givón 1981, p. 170

Marantz says that Principle (9) can be immediately explained by autonomous syntactic theories such as Chomsky's Government-Binding Theory. Briefly speaking, in Chomsky's theory, 'Case-marking is performed under government. Verbs govern their objects, but not their subjects. Therefore, the transitivity of a verb will generally show up in the case-marking on its object which it governs, but not on its subject which it does not.'

(2) Marantz says that if we interpret *-ta-* in UTE as an impersonal subject marker on an active verb (i.e. a suffixal pronoun meaning unspecified person(s)) instead of identifying it as a passive marker as Givón did, most of the phenomena Givón discusses will be explained without further ado—such problems as (i) obligatory deletion of agent/subject in *-ta-* clauses, (ii) what Givón thinks is plural agreement of verbs with obligatorily deleted plural subjects, (iii) what Givón calls pronominal agreement, and (iv) active-transitive property of *-ta-* clauses.

1) Givón's *Preprints* paper is missing in this volume, because he has replaced it by what he orally delivered at the plenary session. On the other hand, Marantz's comments are upon Givón's *Preprints* paper, and not upon the one contained in this volume. Interested readers are referred to Givón 1981, which is identical to Givón's *Preprints* paper.

2) See Note 1.

Thus Marantz insists that highly developed autonomous syntactic theories seem better suited than functional principles for interesting and predictive typological study.

PROFESSOR IVAN KALMAR (UNIVERSITY OF TORONTO) from the floor.

Kalmar expressed his agreement with Marantz concerning the role of functional explanation. He also pointed out the necessity of clarifying or specifying innate ability if Chomsky's theory is to stand.

MARANTZ answered that the theory Chomsky is developing does assume the theory of language acquisition, and that although Chomsky himself does not talk much about language acquisition, others working in his framework to take upon themselves to specify the assumptions that need to be made about language acquisition and to make these innate linguistic principles explanatory.

PROFESSOR HENRY KUČERA (BROWN UNIVERSITY) from the floor.

Kučera commented, apropos of the autonomous (or independent) syntax, that it now seems that for most interesting cases, for example in the case of tense and aspect, it is totally impossible to even tackle the simple question of the well-formedness of a sentence in terms of any syntactic concepts. Givón agreed with Kučera's comment, saying that in his recent work on tense and aspect from the functional standpoint, he has found out that discourse factors were indispensable to define precisely which tense and aspect is proper to use, and that he sees no way of specifying such things in terms of independent syntax. He added that the same applies to such topics as definiteness, anaphora, etc., and that in spite of repeated revisions of theory formulations, transformational grammarians have not come out with any viable solutions that can cope with uncooked data in such areas.

2.1. SHIBATANI'S PAPER³⁾

Shibatani's presentation followed fairly faithfully the content of his *Preprints* paper. He points out similar deviation both in accusative languages and ergative languages. Examples are stative sentences such as (3) and (4) ((9) and (10) respectively in this volume), where case markings and grammatical relations/functions do not match. On the basis of this observation, he posits a schema like (5) (missing in this volume), which displays a continuum of the degree of transitivity with clear ergative pattern at one end and clear accusative pattern at the other end, and neutral pattern exemplified by (3, 4) in the middle.

He then discusses the function of case marking, particularly the function of nominative in an accusative language and that of absolutive in an ergative language, and its relation to subject. He points out that in such sentences as

3) Shibatani's paper contained in this volume is a rather extensively revised version of his *Preprints* paper, but my summary presented here as well as Marantz's comments in this volume are based upon Shibatani's *Preprints* paper.

(3), it is the dative NP rather than the nominative NP that exhibits syntactic properties of subject. He also examines pragmatic notions (focus of interest, speaker's perspective, etc.) that have been associated with subject and tries to establish some correlation between them and case-marking. His conclusion is that 'just as syntactic properties associated with subject may be distributed over different NP categories depending on different languages, pragmatic or semantic properties may be shared by different NP categories.'

2.2. MARANTZ'S COMMENTS ON SHIBATANI'S PAPER

Marantz says that sentences (3, 4) cited by Shibatani do not argue for a continuum represented by a schema like (5). According to Marantz, logical subject and object of verbs are 'direct arguments', and receive semantically neutral case marking. Other arguments are 'indirect' and receive semantically 'loaded' case markings or adpositions. In many languages a verb will appear with both a logical subject and object only when it is associated with both an agent and a patient/theme. Since the verbs in (3, 4) lack agents, they will take only *one* direct argument—the patient/theme. As an indirect argument, the experiencer, possessor, etc. must take semantically loaded case—an oblique case. Semantically loaded cases indicate the same syntactic roles cross-linguistically because we identify such cases by the semantic roles they convey. Thus regardless of our theory of the ergative-accusative distinction, we would expect that the hook-up between semantic roles and cases would be the same in both sorts of languages for the dative-nominative and the dative-absolutive sentences in (3, 4). Shibatani's arguments for a continuum is possible only because case markings and grammatical relations are identified and classified outside of any developed grammatical theory. Other arguments (including the function of case marking) presented by Shibatani suffer from the same shortcoming. Marantz concludes: 'The project of uncovering the function of case-marking should best be embedded within some syntactic or other theory which could provide Shibatani's hypotheses with empirical and predictive force.'

PROFESSOR HENG-HSING JENG (NATIONAL TAIWAN UNIVERSITY) from the floor.

(1) Jeng presented some evidence that he claimed would support Shibatani's continuum hypothesis. Referring to some language used in Taiwan and also some Philippine languages, in which diverse cases (agent, objective, instrumental, dative, locative, etc.) can be subjects, he claimed that there is gradience in the choice of subjects depending upon different types of languages, and that the dichotomy of accusative vs ergative would not work in the cross-linguistic typology.

(2) Jeng also questioned the role of 'point of view' posited by Shibatani, saying that the notion was not clear enough. Citing the case of Chinese and an aboriginal language used in Taiwan that he studied, he said that different semantic/pragmatic criteria are used for the choice

of subjects.

MARANTZ responded to Jeng's first comment, saying that it again illustrates the difficulty of talking about subjects/topics outside of a particular linguistic theory. He said that the Philippine languages that he (Marantz) examined in some detail split into two sets, and that by distinguishing between these two sets and by working in a syntactic theory where notions like subjects/topics are clear and predict empirical consequences, the Philippine languages that Jeng mentioned would not pose a problem for the sort of distinction that he (Marantz) made between ergative and accusative languages. SHIBATANI said, apropos of what Jeng and Marantz said, that the question he (Shibatani) raised toward the end of his paper is the one that demands the kind of work done by many people like Givón, Comrie, etc., adding that it would be impossible to get any clear picture of what 'topic', or 'old information', or 'point of view' is in the framework Marantz is working.

GIVÓN claimed that there is ample diachronic evidence indicating gradual and slow change from accusative to ergative patterns or vice versa and that in the process there are lots of intermediate stages. To handle such situations, therefore, the best thing will be to talk about the two extreme types and lots of possible in-betweens.

MARANTZ said that diachrony might disconfirm what he (Marantz) said, but it is not obvious without looking at the theory and its predictions.

3.1. KUNO'S PAPER

Kuno started with a short preamble about the motivation and aim of the kind of study he was pursuing. For the kind of problems he has been interested in, such as pronominalization, reflexivization, deletion, etc., purely syntactic explanation is hardly sufficient to account for the acceptability judgments of native speakers, which are often gradient and variable. We have to clarify extremely complex interactions among syntactic, semantic, pragmatic, and discourse-based rules, of which syntactic part is the easiest and non-syntactic part is the most difficult and challenging. But this latter part is often neglected or even looked down upon by theory-oriented workers. The discourse deletion phenomena that he takes up in his paper is an example of such phenomenon.

With this brief preamble, he presented the outline of the content of his *Preprints* paper, omitting some examples and adding some new ones. In his *Preprints* paper, which is reproduced in this volume, Kuno replaced his notion of old vs new information that he had used earlier by that of relative importance, asserting that deletion of optional constituents in a sentence proceeds from less important information to more important information (and not in the reverse order) provided that syntactic constraints allow it. This 'pecking order of deletion' coupled with another independently motivated principle, which says that only 'intentional' violation of discourse principles result in unacceptability, gives us a means, he claims, for initiating a systematic analysis

of extremely complex and poorly understood discourse deletion phenomena, and he substantiated his claim with abundant examples carefully scrutinized.

3.2. PRINCE'S COMMENTS ON KUNO'S PAPER

Prince agrees with Kuno. Furthermore, saying that Kuno's clear formulation makes it possible to make empirically testable hypotheses, she herself tests one such hypothesis concerning affirmation. Her hypothesis and test is a good follow-up of Kuno's study.

PROFESSOR RICHARD HUDSON (UNIVERSITY COLLEGE, LONDON) from the floor.

(1) Hudson questioned why there was no reference to intonation in Kuno's paper, the introduction of which would have made it unnecessary to refer to relative importance.

KUNO answered that what decides the intonation (more specifically, the location of the nucleus of intonation) is relative importance rather than vice versa.

(2) Hudson also made the following comment: 'Is the contrast between "functional" and "non-functional" approaches anything more than a difference in the personal interest of the linguists concerned? After all, Givón and Bolinger, who are supposed to be "functionalists", have both recognized in print that grammars are needed, and that some parts of grammars may be actually dys-functional, and Chomsky has recognized in print that functional explanations may be possible for some parts of grammars. So where is the difference?'

4. BY WAY OF CONCLUSION⁴⁾

At present there seems to be no consensus or paradigm in grammatical theory. There are more than a dozen syntactic-semantic theories or models and a multiplicity of approaches. See, for example, *Current approaches to syntax* edited by Moravcsik and Wirth (1980). But beneath (or beyond) this variety of theories, we can discern two main, underlying trends. On the one hand, there are Chomsky and his group, who distinguish between competence and performance, concentrate on 'sentence grammar' (and more recently on 'core grammar'), separating syntax and semantics from discourse functions and language use, and insist on the autonomy of syntax (or the computational component), although nobody including Chomsky denies the interaction between these various components. To them explanation in linguistics boils down to clarifying the problems of language acquisition. On the other hand there are a number of other scholars who start with 'communicative competence', emphasizing the importance of discourse function and language use in context. They insist that significant generalizations and true explanation in syntax and semantics

⁴⁾ The greater part of what follows is what I said at the outset of the session, but it seems more appropriate to put it at the end.

can come only or chiefly by paying due consideration to discourse and pragmatic factors.

This plenary session seems to have reflected these two trends, and all in all, the frank exchange of different views and opinions was salutary and fruitful, I believe. It seems to me that the existence of diverse approaches and different conceptual frameworks and theories will eventually prove to be beneficial rather than harmful. Of course, when one theory is pitted against another, we will find a lot of incompatibility between them as well as inadequacies in each, but when in the course of time the inadequacies are corrected or eliminated and the superficial differences are ironed out, what will remain as fundamental difference will reinforce or supplement each other, and together will shed light upon various facets of this complex affair called human language. In the meantime, what seems to be important is to keep our mind open and try to derive benefit from approaches and theories different from our own. In this connection let me quote a passage from Quine: 'Knowledge normally develops in a multiplicity of theories, each with its limited utility and each, unless it harbors more danger than utility, with its internal consistency. These theories overlap very considerably, in their so-called logical laws and in much else, but that they add up to an integrated and consistent whole is only a worthy ideal and happily not a prerequisite of scientific progress.' (Quine 1960, p. 251)

At the same time I got the impression that on whatever basic, underlying assumptions we may work, we will need a fine-grained and precisely formulated theory with predictive force.

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Condensed Summary of Comments on Givón's Paper

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In my comments I clarify the potential role for functional principles in the explanation of syntactic phenomena and provide what I believe to be a superior analysis of Givón's Ute data.

Could functional principles somehow replace syntactic principles in syntactic explanation, where by syntactic principles we mean features of innate linguistic competence? Consider Givón's principle (9), could such a "Principle"—really just a generalization over observations—be *explained* by, e.g., functional coding principles? A *functional* explanation of (9) and other syntactic principles can only work successfully in conjunction with a theory of language change, development, or acquisition. To see this, replace (9) with structural principle (G).

(G) Giraffes have long necks

There is a functional explanation for (G): Giraffes have long necks to reach leaves in tall trees. However, this explanation is valid *only* in the context of the theory of evolution and natural selection. Without such a theory, the functional "explanation" is merely a description of the use of Giraffe structure (long necks), a description without any particularly interesting status. There is an independent structural explanation for (G): Giraffes have long necks because they are genetically preprogrammed for long necks (the theory of developmental biology is concerned with the expression of the genetic program). We thus have two equally valid explanations for (G), one functional, one genetic. But the functional explanation is an historical one, requiring a theory of natural selection.

At the moment, I would claim that we have no convincing functional explanations for any interesting syntactic principles, such as principle (9). On the other hand, autonomous syntactic theories, such as Chomsky's Government-binding theory (Chomsky 1981), immediately explain (9) and a host of other observations and principles. Briefly, in Chomsky's theory case marking is performed under "government." Verbs govern their objects but not their subjects. Transitivity is a property of verb. Therefore, the transitivity of a verb will generally be manifested in the case marking on its object, which it governs, not on its subject, which it does not.

Givón has failed to argue against a simple alternative analysis of his Ute data which much more economically explains most of the phenomena he discusses without special statement. What Givón calls the "passive" marker—the suffix *-ta-*—is actually an impersonal subject marker on an *active* verb, i.e.,

a suffixal pronoun meaning "person or persons unspecified." This hypothesis immediately accounts for the fact that agent/subject is obligatorily deleted from the Ute passive clause" (sec. 4.1.); one cannot express the subject in *-ta*-clauses for the same reason one cannot add an extra subject to any active sentence; there already is a subject in *-ta*-clauses, i.e. *-ta*- itself. The plural subject agreement facts in Ute (4.3.1) follow from our assumption that *-ta*- is a subject meaning person (singular agreement) or persons plural agreement unspecified. What Givón calls "pronominal agreement" in Ute (4.3.2) is actually just a clitic pronoun. Givón's "agreement" may not agree with any argument in a clause but, just like a pronoun, may "agree," i.e., be coreferent with something outside the sentence. If *-ta*- is an impersonal subject, it cannot co-occur in the same clause with a clitic pronominal subject. It should be obvious that the active-transitive properties of *-ta*-clauses that Givón lists (4.4) are predicted on the present analysis because, on this analysis, Ute *-ta*-clauses are active and, when they contain an object, transitive.

Just as on Givón's analysis, it is a mystery on the present account why the Ute suffix *-ta*- may not appear on verbs taking *no* "arguments" (4.2). Givón suggests this restriction follows from the necessity for some constituent to serve as topic in a passive clause, but of course the *-ta*-marked verb itself could qualify as topic if some constituent must.

In short there seems little reason to call a Ute *-ta*-clause a "passive." One might make this appellation in order to emphasize the "impersonal subject function" of *-ta*-clauses, but identifying *-ta*- as an impersonal subject makes this "function" clear as well. Within a highly constrained syntactic theory (see, e.g., Marantz 1983), identifying a verb as passive automatically produces a set of predictions about the verb's syntactic behavior. As we have just seen, the proper predictions for Ute *-ta*-verbs result when we identify them as *active*.

Givón's "functional typologies," which lead, for example, to identifying Ute *-ta*-clauses as passives, are purely descriptive. Interesting and predictive typologies follow from highly developed syntactic theories. See the "principles and parameters" approach of Chomsky (1981), the cross-linguistic analyses in Marantz (1983), and the typological work of Ken Hale.

Autonomous syntactic theories, theories about the innate linguistic competence of children, seem better suited than functional principles to explaining the sort of syntactic phenomena Givón discusses.

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Condensed Summary of Comments on Shibatani's Paper*

Alec Marantz

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In the first section of my comments, I explain why Shibatani's arguments against a clear typological distinction between accusative and ergative languages are not convincing. In the second section, I question Shibatani's approach to discovering possible functions for case markings and his conclusion that "syntactic properties associated with the subject may be distributed over different NP categories depending on different languages."

Shibatani's arguments for a continuum between ergative and accusative languages only work when case markings and grammatical relations are identified and classified outside any developed linguistic theory. For example, none of his considerations undermine the clean ergative-accusative split proposed and supported in Marantz (1981; see Marantz 1983 for summary). Consider first the dative-nominative/absolutive constructions in Shibatani's (3-4). ((9-10 respectively in this volume —A. Ota). In Marantz (1983) I explain why logical subjects and objects of verbs are "direct arguments" and receive semantically neutral case-marking. Other arguments are "indirect" and receive semantically loaded case-markings or adpositions, markings associated with some set of semantic roles. In many languages, a verb will appear with both a logical subject and a logical object *only* when it is associated with both an agent and a patient or theme. Since the verbs in (3-11) lack agents, they will appear with only *one* direct argument in such languages—the theme/patient. As an indirect argument, the experiencer/possessor/etc. in sentences (3-4) must take semantically loaded case—here, the "dative." Semantically loaded cases indicate the same semantic roles crosslinguistically because we identify such cases by the semantic roles they convey. For example, we call "dative" the case-marking on certain goal/possessors. Thus, regardless of our theory of the ergative-accusative distinction, we would expect that the hook-up between semantic roles and cases would be the same in both sorts of languages for the sentences in (3-4). Such sentences cannot argue for a continuum between ergative and accusative languages (see Tsunoda, this volume, for similar reasoning).

Nor can the existence of so-called "split-ergative" languages be used as evidence for such a continuum. Shibatani identifies case-markings on the basis of morphological form. However, theoretically significant classifications of case-markings are based rather on the manner in which the case-markings are *assigned* or *determined*. For example, "accusative" may be defined as the case

* The following comments were upon Shibatani's paper contained in *Preprints of the Plenary Session Papers*, the rather extensively revised version of which is contained in this volume.—A. Ota

assigned by verbs. In split-ergative languages, different types of nominals (e.g., animates vs. inanimates) may require different morphological forms for, say, the accusative case as defined above, but this does not imply anything about the uniformity of the case-marking system in such languages when this system is analyzed with an interesting syntactic theory. From a theoretically interesting point of view, split-ergative languages may be consistently "ergative" (or "accusative").

Shibatani wishes to investigate the function of case-marking, yet he works outside any highly developed syntactic theory which might be able to identify case-markings in a consistent manner crosslinguistically. As should be clear from the above comments, outside a syntactic theory we cannot individuate or identify case-markings in preparation for examining their functions. In the wildness between syntactic theories, what *is*, e.g., "nominative case"?

Shibatani's conclusions about the non-uniform syntactic behavior of NPs bearing the "same" case or grammatical relations also suffer from his approach of working outside a theory. How can he determine what the *same* case or relation is? In Marantz (1983) I show that a highly constrained theory of grammatical relations may possess a great deal of predictive power. Within such a theory, syntactic properties associated with, e.g., the subject may *not* be distributed over different NP categories depending on different languages. It is not possible to refute such a theory by examining languages while using fuzzy concepts of "subject," "object," "nominative," and "accusative." Rather, one must show that the theory makes wrong predictions or that some competing theory performs better. My remarks on Givón's paper (this volume) are relevant to Shibatani's search for a functional account of case-marking. When Shibatani claims, "A most obvious function of case-marking is that distinguishing A from O in a transitive clause," what is the empirical content of his claim? How could we decide if case-marking does or does not serve this function? Does Shibatani mean, for example, that children could not learn a language in which As were not formally distinguished from Os? The project of uncovering the function of case marking would best be embedded within some syntactic theory and some theory of language change or acquisition, theories which could provide Shibatani's hypotheses with empirical and predictive force.

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Discussion: Kuno's 'Principles of Discourse Deletion'

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Kuno's paper, 'Principles of discourse deletion', represents an important advance in functional syntax in its refinement of the notion of old/new information. In particular, a new information-status is distinguished, called *important/unimportant*, where *important* represents focal information (more or less) and *unimportant* represents background information (more or less). Important/unimportant information, shown to be scalar rather than dichotomous, is related to Chomsky's (1972) *focus/presupposition* and to Wilson and Sperber's (1979) *focal/background*, but is more fine-tuned than either in its correlation with linguistic form.

In addition to defining a new information-status, Kuno in this paper provided us with a clear formulation that enables us to make empirically testable hypotheses about discourse deletion. In what follows, I shall briefly make and test one such hypothesis relating to affirmation. First, however, a very brief discussion of affirmation is in order.

In English, a tensed V may simultaneously convey (at least) three pieces of information: verb meaning, tense, and affirmation. Consider, for example, the following three question-answer pairs:

(1) Q: What did John do after we left?

A: He ate.

(2) Q: Is John eating?

A: He ate.

(3) Q: Did John eat?

A: He ate.

In 1Q, it is assumed that John 'verbed'; 1A supplies the lexical content of the verb: it was eating that John did. In 2A, the lexical content of the verb is assumed and what is added is the past tense. In 3A, both the lexical content of the verb and the tense are assumed; what has been requested is the polarity—affirmation or negation—and affirmation is what 3A supplies. The facts concerning the morphology of affirmation in English are poorly understood (by me, at least), but it seems as though, in (nongapped) affirmative finite clauses, the affirmative 'allomorph' is somehow absorbed by the tensed item; if there is no tensed item, *yes* occurs. Compare 3 with 4:

(4) Q: Did John eat?

A1: Yes.

A2: Yes, and he was sick afterwards.

I shall now propose a hypothesis concerning affirmation and discourse deletion:

(5) Hypothesis:

- A. The tensed VP (i.e. Aux VP) is deletable, leaving yes, if and only if
 1. the sentence is affirmative, and
 2. polarity is most important.
- B. The subject remains with yes only if it is more important than the deleted tensed VP.

The first part of 5 tests the claim that discourse deletion depends upon importance, and the second part tests the claim that importance is scalar rather than binary.

Now consider 6:

(6) Q: Do you like pizza?

A1: I like pizza.

A2: I do \emptyset .

A3: #I yes \emptyset .

A4: #I \emptyset .

A5: Yes.

In 6Q, the information requested is simply the polarity—affirmation or negation—and thus affirmation is the most important item in the answer. Since discourse deletion is not obligatory, 6A1 is ok. In 6A2, VP Deletion has occurred, leaving (tensed) *do* to carry affirmation, and, since tensed verbs generally require subjects, *I* also survives. In contrast, a sentence of the form of 6A3 is predicted to be acceptable just in case the subject (here *I*) is more important than the deleted VP (here *like pizza*), which it is not, and thus 6A3 is correctly predicted to be unacceptable. Similarly, 6A4 is correctly predicted to be unacceptable by Kuno's Pecking Order Principle, since the most important information, affirmation, has been deleted, leaving less important information, *I*. Finally, 6A5 is correctly predicted to be acceptable, since the most important information is affirmation.

Now consider 7:

(7) Q: Does your family like pizza?

A1: I like pizza. My husband and child do not, however.

A2: I do \emptyset . " " " " " " "

A3: I yes \emptyset . " " " " " " "

A4: #I \emptyset . " " " " " " "

A5: #Yes. " " " " " " "

Here again, the information requested, and hence the most important information in the answer, is the polarity. Different from the situation in 6, however, the answerer in 7 has chosen to subdivide the referent of the subject such that, for one subset (= the speaker), the polarity supplied is affirmative, while, for the

complement of the set (= husband and child), the polarity is negative. Considering only the relevant, i.e. affirmative, half of each response, we find once again that the predictions are borne out. 7A1 is exactly analogous to 6A1, in that no deletions have occurred. Note, however, that the information in 7A1 represents not two but three levels of importance: affirmation is most important, as in 6A1, tense and liking pizza is unimportant, again as in 6A1, and *I* is in between. This situation is, of course, compatible with 7A2, which is thus correctly predicted to be acceptable by the Pecking Order Principle. The crucial test for the hypothesis in 5, however, is 7A3: since *I* is more important than the deleted material, 7A3 should be acceptable—and it is. Again, 7A4 is correctly predicted to be unacceptable by the Pecking Order Principle. On the other hand, 7A5 raises an interesting question for Kuno's theory: while it does not invalidate our hypothesis or Kuno's principles of discourse deletion, it suggests that *relative* unimportance of an item is not sufficient to warrant discourse deletion and that some notion of *absolute* unimportance is required. That is, *I* having *some* importance, is undeletable, even though nothing of greater importance has been deleted. This suggests that 5B should be revised as follows:

- (5') B. The subject remains with *yes* if and only if it is more important than the deleted tensed VP.

Now consider 8:

- (8): Q: Who in your family likes pizza?

A1: I like pizza.

A2: I do Ø.

A3: #I yes Ø.

A4: I Ø.

A5: #Yes.

In contrast to 6 and 7, the information requested in 8 is not the polarity but for which member(s) of the answerer's family is it true that that member likes pizza. Polarity here is thus assumed to be affirmative and is part of the unimportant information. As always, discourse deletion is not obligatory and hence 8A1 is acceptable, *I* being most important, everything else—polarity, tense, VP—being equally unimportant. On the other hand, 8A2 raises a second interesting question for Kuno's theory: while it does not violate the hypothesis in 5, it does show that it is possible to delete unimportant information ('liking pizza') while retaining *equally* unimportant information (tense and polarity), without any obvious grammatical compulsion to do so. Perhaps, though, the reason is grammatical, after all: possibly, one always has the grammatical option of uttering a sentence of the form NP VP. Further examination is clearly required. More pertinent to the hypothesis in 5, 8A3 is correctly predicted to be unacceptable, while 8A4 is correctly predicted to be ok. Finally, 8A5 is predictably unacceptable.

In sum, the hypothesis in 5, formulated on the basis of Kuno's principles of

discourse deletion, has been shown to make the right predictions, although two questions have been raised about possible further refinements of the principles. Regardless of how these questions are to be answered, however, it is very difficult to imagine how something other than a function-based theory of discourse deletion of the sort Kuno provides could succeed in accounting for the data presented above.

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Summary of the Plenary Session on Syntax

Henk van Riemsdijk

Tilburg University, The Netherlands

Ladies and Gentlemen,

When one is given the last word in some discussion, as I am now, one is always torn between the option of shamelessly exploiting the opportunity and the option of being noncommittal and vague. Since the latter course of action is quite foreign to me, I will do the former, shameless though that may be, and hope that I may be forgiven for speaking my mind with a few admittedly very personal observations about the plenary syntax session held last Monday afternoon.

In my opening words I said then that we would be presented with four vastly different points of view and that hopefully there would be occasion to bridge some of the gaps. And if it is possible at all to speak about results after such a session, I would say that the four reporters have to a certain extent succeeded in making their models and persuasions more comprehensible to each other and the audience, though perhaps not necessarily more palatable. We heard a general and helpful introduction to professor Culioli's quite complicated paper on "The role of metalinguistic representations in syntax". This was followed by Professor Dik's elucidation of a number of salient properties of his Functional Grammar. Then Professor Hudson introduced his most recent non-transformational theory of language structure. Finally, in a rather theory-neutral presentation, Professor Wasow offered a new analysis of idioms. In the discussion period there were a number of excellent interventions from the floor from which I would like to single out in particular those by Professors Hasegawa and Bierwisch.

My own overwhelming impression, especially after thinking about what I should say today, is that I am amazed at how little I have learned in the way of new insights into the structure of language. I immediately hasten to exempt Professor Wasow's paper which presented a fine classification of idiom types and made a good case for a more compositional analysis of the meaning of idioms. There were facts there that I was not familiar with and the proposal contains much food for thought. I also immediately add that I am quite willing to accept part of the blame myself, particularly in the case of Professor Culioli's paper, since I had a lot of difficulty in understanding his system. By and large, however, the nagging feeling remains that we have heard some proposals for new models of grammar by researchers who are more concerned with affirming and reaffirming their beliefs and persuasions as to the nature of language and with finding their own nicely protected little niche in the pluralistic

domain of modern linguistics than they are concerned with endeavoring to broaden and especially deepen our understanding of syntax.

Let me substantiate this overall impression by highlighting a few points that were raised during the discussion. Professor Hagège stated that he felt that Functional Grammar was a notational variant of Transformational Generative Grammar. Professor Dik answered that this meant extending the term notational variant to a much too loose and uninteresting concept. I think he is right, though not quite for the right reasons. One might sharpen Professor Hagège's observation to mean that present day Functional Grammar is a notational variant of the Transformational Grammars of the mid sixties, certainly in the sense that these two systems offer an enormous descriptive power. Functional Grammar may not have any transformations in the technical sense, but this is more than counterbalanced by a host of other and largely unconstrained rule types. It is to be feared that functional grammarians, sharing the concern for constraining their model, will have to spend years and years rediscovering all sorts of principles and constraints that generative grammarians have been searching for over the past fifteen years. Where Generative Grammar has frequently been guilty of ignoring and then rediscovering earlier insights, as Professor Uhlenbeck correctly observed during the banquet, we are now facing a second generation of similar problems. What is said here about Functional Grammar carries over to, and is illustrated by, Word Grammar. In his treatment of unbounded dependencies, Professor Hudson has rediscovered the successive cyclic treatment of such dependencies. We will eagerly await the rediscovery that there are problems which arise in languages with multiple filler-gap dependencies. As long as new theories of this type are not answerable for the whole wide range of facts that present-day syntactic theory is struggling with, it is not surprising that Professor Hasegawa didn't get any answer when he asked where the empirical differences were that distinguished these models.

Finally, it was disturbing to see how great the difference often is between what is said and what is actually done. Professor Bierwisch pointed out that claims with respect to the autonomy of syntax are empirical claims. This was flatly denied by Professor Dik who countered that the non-autonomy of syntax in Functional Grammar follows from the underlying assumptions. However, a short look at the organization of his model suffices to demonstrate that there are no less than seven subcomponents of rules, neatly separated by levels of representation. If these subcomponents are not autonomous with respect to one another, what sense does the drawing make?

Ladies and Gentlemen, I announced that my remarks would be personal and that I would vent my feelings freely. I hope you will forgive me for having done so. I would like to end this report by expressing a wish for the 1987 CIPL-conference. I sincerely hope that we will then have a plenary syntax session which is bursting with truly new discoveries and exciting new insights.

Thank you.

Summary of the Plenary Session on Semantics

F. R. Palmer

University of Reading

The four papers that were read and the questions addressed to them left me with two general impressions, one a familiar one, one a little surprising.

The first impression was that, superficially at least, there is little general agreement about the subject matter of semantics. The debate seems to be not so much about models for describing meaning as about the nature and definition of meaning itself.

For Akhmanova meaning is simply a 'fact of language'—words have meaning. The problem appears to be a practical one, based on the needs of the dictionary. (In Akhmanova's absence the paper was read by the Chairman; there was no discussion.)

For Bierwisch the subject matter of semantics concerns the interface between the formal linguistic system and the conceptual system. There is, then, not one kind of meaning, but two kinds, though only the former appears to have been the subject of serious investigation. Indeed, does the conceptual system have some kind of logical form (I-K Lee)?

For Coseriu semic analysis ('analyse sémique') is concerned with mental facts; his is a mentalist version of componential analysis.

The paper by Schank, Birnbaum and Mey (read by Birnbaum) seemed different from the others. They conclude, from their work in AI (Artificial Intelligence), that there is no independent semantics, that semantics and pragmatics are completely integrated—'there is no "dictionary", only an "encyclopedia"'.

On second thoughts, are these so different? There was one telling question (B. Rieger). He asked Bierwisch how he could test his empirical assumption that there is an independent level of semantic representation and he asked Birnbaum where he gets his lexical data from if he has no independent semantic representation. He could have asked Coseriu to justify the existence of his mental facts. For here is a dilemma. It is difficult to see how we can proceed without some kind of orderly semantic representation in terms of structures, relations, components, etc., and equally difficult to justify their assumption in the face of the complex and vague features of meaning with which we are presented in experience. One way out is to adopt the mentalist view—there are mental facts. This is, of course, invulnerable, a matter of faith, not observation. Another is to postulate a logical formal semantic system that somehow underlies human speech. But isn't there an element of faith here too? On the other side, one can simply deny the existence of semantic representation, but surely

only in theory, never in practice. Without some abstraction or generalisation nothing can be said.

When we look at specific examples we find that, after all, the different scholars are concerned with the same things. Both Coseriu and Schank et al, for instance, discuss the relation of *buy* and *get*. (Lyons had suggested that they are synonymous in e.g. *I'll go to the shop and — some bread.*) For Coseriu there is a simple dogmatic statement of fact that these do not have the same 'signification'. Schank et al work their way from the primitive concept PTRANS 'to physically transfer an object from one location to another' to M-BUY 'the commercial transaction scenario', and fail to see that such representations as these are precisely the kinds of abstractions that belong to semantics as opposed to pragmatics. In fact, one suggestion (S. Morag) was that they do not go beyond the semantic markers approach. For Bierwisch the problem would be one of contextual specification, clearly involving the relation between the (semantic) language system and the conceptual system.

The second impression was that the session was more remarkable for what was left out than for what was said. In particular, apart from a little in Bierwisch's paper, where was the discussion of formal truth-conditional semantics, which is undoubtedly the latest fashion in linguistics? Where, too, was the discussion of pragmatics in the form of presupposition, implicatures and speech acts? (I noticed some of these were discussed in section meetings.)

Many of the questions addressed to Bierwisch and Coseriu were concerned with their failure to deal with such topics. How can Bierwisch handle expressions whose meanings are definable with reference to some data base—adjectives like *expensive*, *tall*, *rich* or quantifiers (H. Kučera), or presuppositions (I-H. Lee), or contextually determined types of language function (A. Newman)? How can Coseriu handle theme/rheme or analytic/synthetic (H. Rosén), or the role of the hearer or reader (B Schlerath)?

Inevitably the paper by Schank et al came in for particular criticism and questioning. Was there any psychological reality (W. Dressler)? Wasn't it concerned only with the 'significant' and not the 'signifié'? But I cannot help feeling that such questions simply miss the point that workers in AI are trying to find out whether notions of psychological reality or the *signifié* are in any way necessary for understanding semantics-pragmatics.

As a convinced empiricist (which pace Coseriu is a very different animal from a *behaviourist*), I was a little bemused by much of what I heard. For me the answers are in one sense simpler, in another more complex. If we ask what we need to know, in order to understand the meaning of a linguistic expression, the answer lies in such things as denotation, the context in which it may be uttered and its syntagmatic and paradigmatic relations (collocation, field and sense relations) with other linguistic elements. There is plenty of room here for semantics and plenty of work for the human mind, but no obvious or special place for concepts and conceptual representation.

Finally, it was suggested that it would have been helpful to have had a

survey of the substantial achievements made since the last Congress (S. Morag). I listened to several excellent, most scholarly, accounts in other plenary sessions, and fully concur with this suggestion.

Discussion: participants and questions/comments (put in writing).

W. Dressler (Birnbbaum): Modelling of linguistic processes with AI is fine. But do you also claim 'psychological reality' for (or a mental correspondence to) your model and, if yes, has such a claim been substantiated?

J. Hewson (Birnbbaum): Workers in Artificial Intelligence have so far failed to face a fundamental problem: that what they enter as language in the computer is only one aspect of language. When a 'word' is entered it is only as a *signifiant* in the Saussurian sense, without its *signifié*. Furthermore, this is still done under the same format (i.e. by *signifiants*) and the problem is recursive. If this fundamental problem is not solved, does it not invalidate all of the work in AI?

H. Kučera (Bierwisch): How would you propose to handle—within a model of independent semantic representation—the many expressions whose meaning (and, consequently, the truth value of the sentences containing them) are definable only with reference to some data base, e.g. adjectives such as *expensive*, *tall*, *rich*, *small*, etc., quantifiers such as *many*, *a great many*, *a good deal*, *several*, etc., etc., cf. sentences such as: *Many/Most rich people in Los Angeles own at least several expensive cars* (as well as others given by Zadeh and other 'fuzzy logicians').

E. Keenan (Birnbbaum): I wish to challenge the claim that expressions with similar meaning should have similar meaning representations. Rather expressions with similar structure (syntactic) should have similar meaning representations. Examples like (1) and (2) below support this claim:

(1) Only John left.

(2) John left and no one other than John left.

In general the work in formal logic—showing that syntactically distinct logical forms are logically equivalent—supports my claim more generally.

I-H. Lee (Bierwisch):

(1) Given a sentence, does your utterance meaning *m* include the so-called presupposition (or implicatures) that the sentence may have? If not, where do they belong?

(2) As you mentioned in your paper, your *sem* represents a sort of logical translation. Do you intend to represent your *m* in a similar logical form?

C. Lehmann (Bierwisch): From your paper it becomes clear that the relation between semantic representations and conceptual representations belongs to the subject matter of semantics. It does not become clear whether the relation between syntactic and semantic representations also belongs

to it. Traditional structural semantics, such as represented by E. Coseriu, has been much more concerned about the latter than with the former. If both these relations constitute the subject matter of semantics, there would have to be, according to you, one part of semantics which belongs to the grammar and another part which does not. Could you clarify this?

S. Löbner (Bierwisch): You talked about the phenomena of conceptual shift and conceptual specification. The examples you used were *nouns* for conceptual shift and *verbs* for conceptual specification. I would like to ask whether this is just a coincidence or whether you have some evidence indicating that the phenomena of conceptual shift and conceptual specification are *characteristic* for the categories of nouns and verbs respectively?

S. Morag

(a) (Birnbaum): Could Dr. Birnbaum please summarize the new elements presented in his paper which are of some relevance to semantic theory? As I see it, his discussion of the difference between sentences such as *John bought it at Macy's/John got it at Macy's* does not go beyond the semantic markers approach.

(b) (general): Would it perhaps be helpful to include in the plenary session devoted to semantics in coming congresses a survey of the substantial achievements attained since the former congress?

A. Newman (Bierwisch): Since the formal framework cannot be determined without reference to the context of situation or frame of reference almost all ambiguity arises from context or lack of context. Why not determine your type of classification or semantic markers by the purpose of the exercise? I suggest just a few:

- (a) writing a thesis supervised by yourself;
- (b) translating between two specific languages;
- (c) a specific pedagogic purpose;
- (d) a computerised restricted language, etc., etc.

Surely this would be a more feasible and rewarding approach? Your system would thus vary with the context. For which context is your system designed?

B. Rieger

(a) (Bierwisch): 'There is an independent level of semantic representation (as an interface between grammar and conceptual systems) and this is an *empirical assumption!* How can it be tested?

(b) (Birnbaum): 'Lexical structure is virtually indistinguishable from memory and/or knowledge structure—there is *no* computation of an independent semantic representation'. Where do you get the lexical structure/memory or knowledge structure data from?

H. Rosén (Coseriu): Assumant le caractère *exhaustif* de la dichotomie 'expression': 'contenu' (dans le sens que toutes distinctions, qui s'expriment ou qui sont exprimables dans un système de langue donné, appartiennent au domaine du 'contenu', où situerons-nous dans la tripartition proposée (contenu—a) désignation, b) signifié c) sens) les distinctions portant sur les fonctions énonciatives (p. ex., le rhématique vs. le non-rhématique)? Est-ce qu'une telle distinction peut être classée parmi les signifiés syntaxiques, ou bien devrait-on élargir la sous-division des 'contenus' afin d'y pouvoir inclure la distinction dont nous parlons? Ou bien, ne fait-elle, selon M. Coseriu, pas du tout partie du 'plan de contenu'? Même question pour les distinctions, exprimables dans quelques langues, entre des types de prédication, tels que la prédication analytique et la prédication synthétique?

B. Schlerath (Coseriu): How can we include in semantic analysis the role of the listener (reader, dialogue partner)? Understanding of the speaker and listener is never completely identical. What is not understood did not become a social fact—cannot be included in semantic analysis. For example: Impossibility of giving a *final* interpretation of poems (like Rilkes Duineser Elegien).

T. Slama-Cazacu (Birnbaum): oral comment only.

Summary of the Plenary Session on Morphology

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The four reporters at this plenary session on morphology who commented on their preprinted papers were:

Mark Aronoff, State University of New York, Stonybrook;

Wolfgang U. Dressler, University of Vienna;

Judith N. Levi, Northwestern University, Illinois;

Arnld Zwicky, Ohio State University.

The topic of the plenary session was very much in keeping with the theme of this 13th International Congress of Linguists —“Linguistics in the 1980's”— since the beginning of this decade has been marked by, among other things, a reawakening of the traditional linguistic interest in the nature of the word, and that part of linguistics concerned with the form and meaning of this unit, morphology. The interest, of course, is not a new one since it goes back at least as far as Plato and Panini. There was however a gap in the recent history of our science which may have been in part a reaction to the fact that for many years most linguistic research concerned only phonology and morphology; during this gap a number of linguists viewed the interface between phonology and syntax as a direct path. Fortunately, this path has once more been extended, and morphology has again captured the interests of both young and seasoned scholars. Nor is the interest simply a repetition of what went before; present research has already gone far beyond earlier studies with concern for the simple word, the derivational and inflectional complex word, the form, structure, and meaning of the word, how the word is represented in the grammar and in our mental lexicon, how it is formed, generated, and derived, how it is processed in both speech production and perception. These concerns also rely on different kinds of evidence in the search for an understanding of word formation and meaning. It is thus that I started this summary with the view of the appropriateness of including a plenary session on morphology at this Congress, and both I and the plenary reporters are thankful to the Congress organizers for providing us with the opportunity to hear and discuss the rich and stimulating ideas presented.

An important characteristic of our session was the lack of ad-hominem and non-substantive argumentation. Although there were different and alternative views and theories presented and discussed, a basic assumption was that only through listening and debating the theoretical and methodological principles could we arrive at the 'truth'. Sadly, all the sessions at this Congress did

not appear to be as united in their goal.

The first reporter, Dr. Aronoff, discussed two related pairs of oppositions related to the word: actual words vs potential words, and frequency vs productivity, showing how the two interact and how they can shed light on some theoretical issues. He then presented a number of experimental and statistical results, analyzing them in terms of these theoretical constructs, and showing how such results can lead to a better understanding of certain questions; most particularly, he showed how an unproductive rule might be distinguished from one which is no longer part of the grammar. Finally, he outlined a method for studying changes in productivity, using the OED and presented the results of some preliminary work involving this method.

Dr. Dressler then outlined his theory of word formation in Natural Morphology. He contrasted the functions of language—(1) communicative (2) as a support of cognition—with the functions of word formation in language which he described as (1) lexical enrichment (2) lexical motivation (3) predication and (4) textual cohesion. He also discussed a major approach used in Natural Morphology which aims to establish universal scales of naturalness from best to worst, and language types which are determined by the specific constellation of (good and bad) choices from these various universal scale, and finally language specific systems and processes. Dressler also contrasted the views propounded by adherents of Natural Morphology with those of generative and structural models, providing an overview for the session in the form of a metatheory which, he later reiterated, was compatible with various alternative theories of grammar.

In Dr. Levi's comments on her preprinted paper, she stressed the fact that her theory of complex nominals aims at predicting all regular form-meaning pairs (which cluster as one form matched with multiple—about eleven—potential meanings). These patterns are, according to her theory, context-independent and generate all "potential" complex nominals in grammars. She pointed out, however, that a related and distinct problem is the pragmatic one of achieving a three-way match of form, meaning, and referent, a task obviously context dependent. While her theory is concerned with the syntax and semantics of complex nominals, she recognizes that a pragmatic account of complex nominal use in context must complement her analysis; it does not, however, she emphasized, replace a syntactic/semantic analysis.

Dr. Zwicky concluded the reporters' primary comments by reiterating the traditional role of morphology as a separate system between syntax and phonology. He summarized some basic assumptions which he showed have important consequences for linguistic and morphological research:

(1) The linguistic system is composed of separable, autonomous components or modules; morphology itself encompasses separate modules, i.e. word formation rules, allomorphy rules, morphophonemic rules.

(2) The linguistic system itself is autonomous although it interacts with other cognitive and extralinguistic systems and abilities. It is, however, in-

dependent of them in principle.

(3) The linguistic modules mentioned in (1) above, interact with one another in non-random specific ways.

(4) The interactions of the modules, mapping one level of linguistic representation onto another, e.g. morphological onto phonological, have directionality.

Given these assumptions, one goal of linguistics is then to provide substance to the nature of the autonomous modules and the ways in which they interact with themselves. Morphology then becomes enriched by providing deeper description and explanation of the form and structure of word units and rules.

Following the initial presentation, the discussion was lively and substantive. Some of the questions and comments raised the following issues:

(1) Synchrony vs diachrony in the speakers' lexicon. It was noted that while we need to recognize diachronic residues, it is important to distinguish them from productive synchronic morphological rules and processes.

(2) The problem of determining when complex forms become lexicalized was raised. It was also pointed out that there are degrees of lexicalization and it is not always a simple matter to decide when, for example, a complex nominal or a derived word has been generated by productive rules as opposed to being fixed forms in the lexicon.

(3) The question of experimentation in morphology was raised, and what constitutes a reliable experiment.

(4) The relations between scales of naturalness in morphology and markedness in phonology was questioned. Dr. Dressler disassociated the two.

Other specific examples, comments, and questions were addressed to the speakers, whose answers not surprisingly sometimes did and sometimes did not satisfy the questioners. Morphology is a vital part of a tremendously complex phenomenon—human language. Substantive debate and disagreement is therefore not only to be expected but to be cherished. We hope through listening to each other as well as continuing our own research to keep approaching the 'truth' knowing that, as in all science, we have not yet attained it.

Summary of the Plenary Session on Phonetics and Phonology

Herbert Penzl

University of California, Berkeley

The three reporters briefly amplified their accounts as contained in the *Preprints*, pp. 179–208. The first speaker, Professor Eugénie J. A. Henderson, University of London, had provided a detailed account of significant phonological studies following Chomsky and Halle's *The Sound Pattern of English* (1960). She characterized such approaches as natural phonology, natural generative phonology, autosegmental phonology, dynamic nonsegmental phonology. She rightly pointed out that hardly any investigations following the Chomsky-Halle canon of distinctive features (which Ladefoged partly defended) had differentiated between the classificatory features of the alleged 'deep structure' and the scalar features of the 'surface'. Generativists may have smashed the phoneme into its distinctive features but, as Mrs. Henderson indicated, continued phonetic research in the Eighties should lead to a drastic revision of this feature matrix. Mrs. Henderson did not use the distinction, not just facetiously proposed by scholars like W. F. Twaddell, M. Joos, later F. Householder in the days of phonological structuralism, between "hocuspocus linguists" and "God's truth linguists" but her prediction was a continued 'swing away from the highly abstract constructs proposed in earlier studies towards phonological forms which are felt to be closer to the surface forms'. Judging by questions and comments the audience completely agreed with Mrs. Henderson's account.

Professor Ilse Lehisté of Ohio State University reported on the role of prosody in the internal structuring of a sentence, containing various word groups. She referred to her own experimental work as well as to a variety of other studies recently completed. Her results that temporal cues were more important than intonation in the comprehension of word grouping within the sentence seemed to surprise some of the audience. The suggestion was made that in whispered speech some additional factor beside timing marks junctures.

Professor John J. Ohala of the University of California at Berkeley, discussed the use and usefulness of data and methods of neighbouring fields like acoustics, psychology, and others for phonological interpretation. The participants at the meeting emphatically endorsed this position without discussing specifically his examples.

The comments and questions from the audience were only made in English, although the reporters and Professor H. Fujisaki had agreed to translate, if necessary, remarks in Japanese, German, French. It has been one of the striking features of this international congress of language experts that one language, English has had practically a monopoly, not because of the organizers but

because of the participants. Practically all members from German-speaking countries and most members from French-speaking countries have used English exclusively.

Summary of the Plenary Session on Historical Linguistics

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Mr. President! Ladis & Gentlemen! I have had the honour and a great privilege to chair at our Congress the plenary session on historical linguistics.

The historical linguistic studies at the present Congress, unlike some previous linguistic Congresses, were in the center of attention and scholarly interest, this being a reflexion of general tendencies in the development of contemporary linguistics.

The latter half of the 20th century has been marked in the history of linguistics by an enhanced interest in problems of diachronic studies of language, this being in a sense a return to the treatment of traditional problems which arose in the classic comparative Indo-European linguistics.

This growing concern with problems of diachronic linguistics stems from the general development of linguistic thought over the past decades. Overcoming the Saussurean antinomy between synchronic and diachronic linguistics, it is striving to build a linguistic theory that would have more explanatory power in comparison to strictly synchronic theories of taxonomic structural and transformational generative grammar.

That is why the present linguistic Congress was held under the imprint of ever growing concern for problems of historical comparative linguistics, of diachronic linguistics in general; that is why the plenary session on historical linguistics at this Congress aroused a lively interest among the audience, so more so as the speakers were eminent contemporary historical linguists.

This growing concern with problems of language history and linguistic diachrony is realized in contemporary historical linguistics at a new methodological level, with new, more rigorous techniques of linguistic analysis elaborated in structural synchronic linguistics of diverse contemporary schools and directions. One of these trends in contemporary linguistics—typology and the linguistics of language universals—has special implications for comparative historical linguistics and language reconstruction being a discipline intimately connected with the problems and methodologies of the latter. This has been clearly demonstrated by the late Roman Jakobson in his famous paper on Typological linguistics and its implications for historical comparative studies held at the VIII International Congress of Linguists in Oslo, 1957.

But this relationship between the typological and historical comparative linguistics appears to be not unilateral, and typological linguistics may also receive new impetus and fresh ideas from historical studies, as we have learnt at our plenary session from an excellent paper presented by Calvert Watkins and entitled "New directions in Indo-European: historical comparative lin-

guistics and its contribution to typological studies", the subtitle of which is a clear allusion to the classic paper of his great teacher—Roman Jakobson, the sad news of whose passing away has recently reached us.

Prof. Watkins maintains that—and I quote: a "proto-language is an artifact, a model, a scientific hypothesis which is always subject to alteration, and improvement in the light of new data or superior analysis." This statement which many a historical linguist would subscribe to, should apply indiscriminately to the status of any reconstructed structure of a proto-language, even to the traditionally established and the most time-honoured structures of Proto-Indo-European, postulated in the classic Indo-European historical comparative studies. In due course, we have to alter and improve on them, if the traditional paradigm turns out to be contradictory and implausible in the light of new insights into the structure of language gained in course of later linguistic research.

Prof. Antonio Tovar of Madrid dealt in a highly interesting report on "Linguistic similarity and its significance: Comparative procedures" with typology and problems of genetic classification of languages. He assumes that linguistic taxonomy can be used as a classificatory instrument, both in a genealogical taxonomy and in a purely descriptive taxonomy of external features.

Dr. Bynon of the School of Oriental and African Studies, University of London, discussed in her most stimulating paper: "Syntactic reconstruction: A case study and some implications" questions of diachronic reconstruction of syntactic structures that have previously been thoroughly analysed synchronically in individual languages, in particular certain verb-complement constructions in contemporary English and German, the idea involved being an illustration of the procedure of interrelating the two Saussurean dimensions in a unitary synthetic process of linguistic analysis.

In the ensuing discussions from the floor quite a number of participants took their stand on diverse issues raised in the presented reports. The many interventions on the part of the discussants also contained a host of new ideas and viewpoints. Although necessarily limited in time and scope they contributed a great deal to the elaboration of topics dealt with in the main reports. Some participants from the floor were more active than others in exposing their views on diverse issues of historical linguistics, some of them even more active than the main speakers on the stage, requesting to come to the microphone more than once.

Historical comparative linguistics emerges once again in the light of the scholarly work done at this Congress as a strictly systemic discipline dealing with language as ever variable and evolving a phenomenon subject to a rigorous structural analysis in terms of synchronic and diachronic rules of transformation. I hold that the present Congress was a complete success in this respect and it seems to have contributed considerably to further developing the theory and methodology of historical comparative linguistics, of diachronic linguistics in general.

Thank you.

Tokyo, September 4, 1982

Summary of the Plenary Session on Psycholinguistics

Walburga von Raffler-Engel

Vanderbilt University

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Madame chairman, Professor Hattori, Professor Inoue, ladies and gentlemen: I have the honor to report on the Plenary Session on Psycholinguistics. Going from the general to the specific, the order of presentation of the three major speakers was a survey paper by Professor Oksaar, a position paper by Professor Slama-Cazacu, and a research paper by Professor Levelt. The formal presentations were followed by a lively discussion from the audience and the panelists among themselves. Before the closure of the session each major speaker had a chance to briefly summarize his main points. The emphasis of my report will be on the discussions as the official presentations are available for reading in the Preprints and eventually in the Proceedings.

Before I report on the issues discussed during the session, I would like to talk about something that was not mentioned in the session. The most recent research in neurology points to inherent differences in the brains of men and women. It is therefore quite possible that the differences in the conversational behavior of the sexes are largely innate. Men and women may differ in what they have to contribute to linguistics. This was the first International Congress of Linguists where women were adequately represented in the plenary sessions. Professor Hattori, president of the Congress, and Professor Inoue, secretary-general, should be complimented by all of us for making linguistics more open and diversified.

In other ways, too, this has been for me a most rewarding congress. About this same day ten years ago at our XIth International Congress in Bologna I stressed the concept of the unit of communication and it was gratifying to see that the idea has now become commonplace. It was the basic concept underlying all three presentations and explicitly mentioned by two speakers, Professors Oksaar and Slama-Cazacu.

This was also the first of our congresses to have two Working Groups on Nonverbal Behavior (no. 2 and no. 26). In the same vein, the two general papers by Professors Slama-Cazacu and Oksaar insisted on the necessity of including nonverbal behavior in linguistic analysis and the author of the research paper, Professor Levelt, did not challenge this contention which throughout the last ten years seems to be firmly established. Professor Oksaar classified paralanguage with verbal behavior and grouped time and space within an extraverbal category but none of the speakers entered into a discussion of the relationship of the verbal and the nonverbal components of the unit of communication. According to Pribram, the brain stores language separately from paralanguage and

kinesics. Physiologically, however, language and paralinguistics are obviously together in contrast with the visual channel of kinesics. The dichotomy still needs to be addressed in our research.

Work in any of the many sub-fields of psycholinguistics entails the study of language, paralinguistics, and kinesics. Language must be examined against the context, as was suggested long ago by Professor Slama-Cazacu and reiterated again by her in this session. We have come a long way but Slama-Cazacu warns us not to overshoot our goal by abandoning the study of linguistics for the sake of psycholinguistics and sociolinguistics. The study of man as a social being must constitute a separate science from the study of language as an abstract system of signs. Her opinion is not shared by Professor Oksaar who believes that the traditional concern with language as a system at the expense of speech communication constitutes a faulty approach rather than a difference in scientific orientation.

There was a substantial discussion concerning the aims of psycholinguistics. Professor Slama-Cazacu had expressed the need for the study of "the real concrete man" while Professor Levelt insisted on discovering "the underlying principles of mental organization." Eventually, this proved to be a pseudo-argumentation as all participants were interested in language-and-cognition. Opinions differed concerning the interdependency of the latter and Professor Fromkin clarified the issue by providing the example of a mentally retarded individual who had the correct use of lexicon and grammar without cognizance of the corresponding referential notions. One participant challenged our competence in dealing with brain functions as none of the linguists present had the necessary background in psychiatry. This, which in my opinion is a valid objection, was not taken up by the audience.

The question period brought out a further theoretical divergence among the panelists. Professors Oksaar and Slama-Cazacu view the production of language exclusively as communicational process while Professor Levelt's research, in addition to the latter, points to aspects of language behavior which do not show communicative interaction. I must side with Professor Levelt as our own empirical studies on self-monitoring during translation support his point of view.

A large portion of Professor Oksaar's paper was devoted to language acquisition. I was very pleased to hear that the meeting I organized in Florence in 1970 where for the first time there were separate sections for maternal language, for the acquisition of kinesics, and for child bilingualism had borne fruit, and that these have since become standard sections at congresses on language acquisition. Professor Oksaar insisted on the necessity of "motherese" while Professor Levelt quoted data to show that the use of this register is not a *since qua non*. Professor D'Arcais agreed with Professor Levelt and concluded that before we spend valuable time on the study of maternal registers we should concentrate our efforts on what is truly essential for the child's acquisition of language. Professor Ito gave examples from Japanese to document that emotive com-

munication is prior to intellectual language. Professor Oksaar said that she did not deny that affect plays an important role in language development but in the ensuing discussion it became apparent that the two scholars were not talking about the same concept. Prof. Ito's examples were concerned with specifics of affect while Prof. Oksaar mentioned "mood" which is a different and much broader concept. Needless to say, I am with Professor Ito. There was general consensus on the desirability of in-depth case studies to understand how language is acquired and stored in the brain.

Professor Oksaar made an important point when she stated that monolingualism is not necessarily the norm but did not go into any of the psycholinguistic issues, concerning neither the storage capacity of the brain nor the semantic structuring of linguistically distinct but ostensibly identical referents. I would also have liked to see some statistics on the diffusion of individual bilingualism as distinct from societal bilingualism. The latter pertains to the area of sociolinguistics. It is an external factor which bears greatly on psycholinguistics. The presentation on first language acquisition by Professor Oksaar was oriented more towards sociolinguistics than psycholinguistics and this, probably, was a limiting factor in the discussion of problems in linguistics proper. Nobody touched on some of the main controversies of our time, such as whether language acquisition constitutes addition or specification.

Concerning second language question, Professor Oksaar listed some of the major factors influencing motivation and then described the well known areas of conflict between language and culture. In this context, two points were hotly debated. Professor Oksaar made a distinction between "behavioreme" and "cultureme". These are well established terms. The crux of the matter is to isolate a behavioreme that goes beyond the most fundamental human emotions and Discovered Actions in the sense of Desmond Morris. Professor Slama-Cazacu pointed out that all behavior is culture-specific and would disband with the term behavioreme all together while Professor Oksaar held on to the dichotomy but without adequate documentation. The most ardently debated issue of the session concerned foreign language teaching. Professor Oksaar proposed that the goal of second language teaching should be perfect bilingualism-cum-biculturalism. Professor Nickel, on the contrary, insisted that his experience in this field makes him doubt that genuine bilingualism is possible or even desirable in a realistic situation, where it would be better to concentrate on simply eliminating errors that cause miscommunication.

The final summaries of the panelists showed how difficult it is for us linguists to move into the eighties. Professors Oksaar and Slama-Cazacu were still totally enmeshed in the controversies of the sixties attacking transformational-generative grammar and Professor Slama-Cazacu even attacked American linguists as a whole. I realize that at the time I was in a very small minority but I am an American linguist and I have criticized transformational grammar from its inception but I do believe that time has come that we devote our efforts to more constructive endeavours. It is my hope that we can finally emerge

as true scholars willing to acknowledge the contributions of our predecessors and co-workers, whatever their theoretical persuasion. I look forward to an international community of linguists who base their theories on solid empirical research and who all share their findings. This is our first congress in the Orient and as we have finally, at long last, crossed the Pacific Ocean may-be we can also cross our sectarian boundaries.

I am grateful to the Organizing Committee of the Congress for the opportunity to speak to my distinguished colleagues in this beautiful Toshi Center and look forward to five fruitful years of international and inter-sectarian cooperation until our next congress. The committee was wise in assigning a secretary to each chairman and Prof. Kunihiro made sure that I did not miss anybody in the audience who wanted to speak. I am grateful for his attentiveness and if I am guilty of any omission or misinterpretations the fault is mine and not his.

Summary of the Plenary Session on Sociolinguistics

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The plenary session on sociolinguistics followed the pattern that had gradually evolved in the course of the Congress. After a brief opening statement by the chairman, who stressed the differences in approach found in the papers by the three reporters, Professors Haugen, Knobloch, and Mackey proceeded to a presentation on their papers or of comments on these papers. Einar Haugen, who in the preprint version had chosen a very personal way of discussing the problems of language choice and of values attached to languages that were within the range of choice for the individual, concentrated on the nonautobiographic issues which in the preprint version had been both backgrounded and highlighted by an account of the experiences, negative in part, but overwhelmingly positive, which life as a bilingual or even trilingual had given to him. Johann Knobloch in effect read his preprinted paper, with the notable exception that he now placed his comments on orthography reform in German-speaking countries at the end of his talk—a decision which was to have rather surprising consequences. William F. Mackey, whose concise overview of the sociolinguistic literature of the past decade given in the preprints could hardly be repeated, let alone condensed in an oral presentation, chose to raise very general questions: Is sociolinguistics part of linguistics proper, a branch of it as it were, or does it combine the two disciplines of linguistics and sociology, or is it even to be considered part of sociology rather than of linguistics? Can one predict what direction (or directions) sociolinguistics will take in the remaining years of the eighties? As the preprinted papers had done, the oral statements thus focused on three aspects of the general topic of the session: How do (or should) individual users of languages react to the conditions a society provides for the use of particular languages? How do (or should) societies provide answers for language-use based problems encountered by groups within these societies? What have been the most notable results, in recent years, of the scientific study of language within societies and of society as a conditioning factor in the development and change of languages, and what are the prospects for the future?

Apart from the panelists and the chairman, twelve members of the congress participated in the discussion. A slightly amused, slightly exasperated remark by Einar Haugen to the effect that the problem of capitalization or noncapitalization of nouns in German orthography seemed hardly of central importance for a session on sociolinguistics, led to an extended exchange of views on this issue, not surprisingly mostly presented in German and rarely

nonspecific enough to permit generalizations. Other components of Professor Knobloch's paper, however, also attracted attention, *inter alia* the much-discussed question of the desirability of total linguistic integration of the children of immigrants and of these immigrants themselves, with a concomitant controversy about the desirability or harmfulness of pidgin-like foreigner talk. Here, connections with Professor Haugen's observations became obvious, and general agreement seemed to prevail that a preservation of language diversity, including protective measures for endangered minority languages, would be in the best interest of a society, but that social integration nevertheless ought not to be lost sight of. As to be expected, some of the comments made remained isolated, which did not deprive them of their innate value; thus, remarks concerning the much greater difficulties to be faced by members of non-Western cultures and speakers of non-Indo-European languages when trying to become fluent in a Western language and to acquire a culture competence, were duly appreciated; it was obvious to everyone that the converse was equally true, viz., that the barriers were equally formidable for a Westerner trying to become a member of a non-Western linguistic and cultural community.

The diversity of the contributions, both by panelists and discussants, did not lend itself to an easy generalization beyond mere enumeration; it would seem to me that this fact can be taken as an indication that sociolinguistics is the study of a field with many directions to take and many interesting developments ahead; it will be worth watching and, more important, worth pursuing.

nonspecific to permit generalization. Other components of Knobel's paper, however, also received attention, particularly the question of the desirability of preserving the linguistic identity of immigrants and the desirability of bilingualism. Here connections with Professor Hargreaves' observations about the preservation of language seemed to prevail that a preservation of language and protective measures for endangered minority languages would be of a value to the community. It was to be expected, some of the community members, which did not deprive them of their linguistic value, that the much greater difficulties to be faced by members of the community of non-Indo-European languages when they were in a Western language and to acquire a cultural competence were obvious to everyone that the community was equally. The barriers were equally formidable for a non-Indo-European member of a non-Western linguistic and cultural community. The barriers, the contributions, both by panels and discussion, not lend itself to an easy generalization beyond mere enumeration; it seems to me that this fact can be taken as an indication that sociolinguistics is the study of a field with many directions to take and many intricate developments ahead; it will be worth watching and, more important, pursuing.

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Un problème d'équivalence sémantique: les nominalisations

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Vrije Universiteit Brussel

1. Les nominalisations se définissent comme l'enchâssement dans une phrase matrice d'une proposition nominalisée, c'est-à-dire transformée en syntagme nominal. On peut alors distinguer quatre grandes catégories de nominalisations: les nominalisations simples, infinitives, affixales et complétives. Seules ces dernières retiendront notre attention (a. *Paul viendra, je crois cela*; b. *Je crois que Paul viendra*). Dans le cadre de la grammaire générative et transformationnelle, les complétives (par exemple *que Paul viendra* dans *Je crois que Paul viendra*) sont généralement considérées comme des phrases nominalisées qui sont dominées dans l'indicateur syntagmatique par un constituant de type nominal. Nous limiterons notre étude à l'examen de quelques cas de complétives objet direct de verbes personnels.

La théorie générativiste qui veut que les complétives soient dominées par le noeud SN (syntagme nominal) en structure profonde se heurte à de nombreux problèmes d'ordre syntaxique. Car, s'il est vrai que les complétives et les SN ont un certain nombre de propriétés communes, ils ne se comportent pas toujours de la même façon par rapport à la transformation pronominale et passive, par rapport à la transformation dite d' "extraction", etc. (Boone, 1980). C'est, toutefois, sur la soi-disant équivalence sémantique entre les phrases comportant une complétive et celles avec un SN que nous voudrions attirer l'attention.

2. Les grammairiens assimilent habituellement la complétive à un substantif. Certains disent simplement que la complétive est "comparable" grammaticalement à un substantif ou qu'elle peut jouer le même rôle qu'un substantif. D'autres établissent des liens transformationnels entre les deux types de phrase suivants: a. *On m'annonce que Jean est parti*; b. *On m'annonce le départ de Jean*. L'assimilation étant complète on dira que la complétive est transposée dans la catégorie du substantif et qu'il n'y a aucune différence syntaxique entre un complément phrastique et un complément nominal.

Comme nous l'avons noté plus haut, la construction complétive et la construction nominale ont des propriétés communes. La question se pose alors de savoir si l'une des deux constructions est réductible à l'autre ou si elles sont l'une et l'autre autonomes. A première vue, il semble qu'on puisse passer, ou repasser, de:

(1.a) *Je crains qu'il (ne) vienne*

(2.a) *On m'annonce que Jean est parti*

(1.b) *Je crains sa venue*

(2.b) *On m'annonce le départ de Jean*

Mais partant de, par exemple:

(3.a) *Je pense que Jean est parti*

nous ne pouvons aboutir à:

(3.b) **Je pense le départ de Jean*

Cette substituabilité, dans le sens où la conçoivent les grammaires traditionnelles, a donc ses limites. Si nous constatons que la double construction est possible (construction par *que* et construction nominale), tous les problèmes n'en sont pas pour autant résolus: on peut encore se demander si on exprime exactement la même chose par l'une ou l'autre construction.

Nous essayerons de démontrer que, si l'on compare le système de l'expansion nominale à celui de l'expansion verbale, on peut dans bien des cas démontrer l'autonomie sémantique (et syntaxique) des constructions complétives. Des verbes tels que *dire*, *croire*, *penser*, etc. acquièrent soit un sens totalement différent, soit un sens supplémentaire dans la construction complétive.

3. Les verbes déclaratifs entrent habituellement dans la construction:

$$SN_1 + V_1 + \left\{ \begin{matrix} SN_2 \\ \text{que } P \end{matrix} \right\} + (\text{à} + SN_3)$$

où *que* P symbolise la complétive introduite par *que*. Dans les phrases:

(4) *Jean dit que Pierre a raison*

(5) *Jean dit la vérité*

que Pierre a raison et *la vérité* auraient, en vertu de certaines propriétés communes (par exemple, la pronominalisation au moyen de *le*, *la*), la même fonction, à savoir la fonction objet direct. On peut toutefois se demander si, en procédant de cette façon, on ne déguise pas sous une même étiquette deux réalités linguistiques différentes. La phrase (4) illustre ce qu'on appelle le „discours indirect”. Il est loisible de dire la même chose en adoptant le „discours direct”:

(6) *Jean dit: „Pierre a raison”*

Le choix de l'un ou l'autre de ces modes d'expression peut dépendre de différents facteurs, dont la capacité d'„imiter” la personne dont on rapporte les propos. Le nombre d'énoncés pouvant apparaître derrière *dire* dans (4) et (6) est illimité. Le nombre de SN admis après ce même verbe est, en revanche, extrêmement restreint et limité au „proférable” (exception faite du conditionnel de *dire* à certaines personnes). Nous pouvons conclure, avec G. Bernard (1971, p. 73) que dans (4) et (6) le complément du verbe est le „propos relaté”, alors que dans (5) et (7)-(8), cités ci-dessous, il est le „contenu du propos”:

(7) *Jean a dit un mensonge à Paul*

(8) *Jean a dit des bêtises à Paul*

Cela explique pourquoi nous n'avons pas, à côté de:

(9) *Il dit que Paul part*

une phrase telle que:

(10) **Il dit le départ de Paul*

Les „noms d'action", qui ne renvoient pas au „contenu du propos", ne peuvent donc apparaître dans la construction nominale.

Ce qui précède nous amène nécessairement à la conclusion que, sémantiquement, il est nécessaire de séparer la construction nominale et la construction complétive. Le choix de l'une ou l'autre construction est, entre autres, fonction du message: doit-on référer au „propos relaté" (complétive) ou au „contenu du propos" (SN)?

Le verbe *assurer* ne devient un verbe déclaratif que lorsqu'il entre dans la construction complétive. Dans la construction nominale *assurer* a le sens de „garantir l'octroi": „assurer {une rente, un avantage, une pension} à quelqu'un". Lorsqu'il est suivi d'une complétive *assurer* est un verbe de *dire*. Nous avons l'expression d'une communication (*que* P) vers un interlocuteur (*à* + SN):

(11) *Pierre a assuré à Paul qu'il était sincère*

Aucun lien transformationnel ne peut être établi entre la construction complétive et la construction nominale: le verbe *a*, dans les deux constructions, un sens totalement différent.

Avec certains verbes, tels *prétendre*, seule une classe restreinte de SN „lexicalement vides" (*quelque chose, un truc, etc.*) peut venir prendre la place de la complétive: Dans:

(12) *Il le prétend*(13) *Il prétend cela*

le et *cela* ne peuvent renvoyer qu'à une complétive.

4. Le problème de la relation entre la construction nominale et la construction verbale présente pour un verbe d'opinion tel que *croire* des aspects intéressants. Dans la construction nominale $SN_1 + croire + SN_2$, le substantif objet direct peut être */humain/* ou */non humain/*. Seul le cas où N_2 est */non humain/* nous intéresse lors de la comparaison avec la complétive.

Le nombre de SN admis couramment en position postverbale directe de *croire* est assez restreint:

(14) *Pierre croit {cette histoire, cette nouvelle...}*

C'est le „contenu" de l'histoire, de la nouvelle auquel on peut croire ou ne pas croire. C'est la raison pour laquelle la catégorie des noms verbaux semble exclue dans la position objet direct:

(15) **Je crois le départ de vos enfants*(16) **Pierre croit l'étonnement de Paul*

Les phrases suivantes:

(17) *Je crois que vos enfants sont partis*(18) *Pierre croit que Paul est étonné*

ne sont, par conséquent, pas transformables en des énoncés nominaux, bien qu'il existe un nom sémantiquement et formellement analogue au verbe.

Le verbe *croire* entre également dans la construction indirecte $SN_1 + V_1 + à + SN_2$ et derrière la préposition *à* nous trouvons, entre autres, des noms verbaux:

(19) *Pierre croit à l'étonnement de Paul*

Mais à la construction nominale $SN_1 + V_1 + à + SN_2$, ne peut correspondre une construction verbale du type $*SN_1 + V_1 + à + ce + que$ P où que P serait une complétive.

On peut se demander s'il y a une différence de sens entre la construction nominale directe et la construction nominale indirecte et, s'il y en a une, de laquelle de ces constructions il faut rapprocher la construction complétive.

Dans la construction directe (*croire quelque chose*) la sélection des SN est beaucoup plus sévère que dans le cas de la construction indirecte. Nous avons vu que dans:

(20) *Pierre croit (cette histoire, cette nouvelle...)*

c'est le „contenu” de l'histoire (de la nouvelle) que Pierre tient pour véridique. Nous pouvons analyser de la même façon:

(21) *Pierre croit à cette histoire qu'on lui a racontée*

Mais la construction indirecte offre de nouvelles possibilités:

(22) *Pierre croit à la victoire de son équipe*(23) *Pierre croit au départ de Paul*

Les énoncés:

(24) *Pierre croit (au fait) que son équipe sera victorieuse*(25) *Pierre croit (au fait) que Paul va partir*

peuvent, dans des contextes identiques, fonctionner comme des paraphrases satisfaisantes de (22)-(23), bien qu'ils soient susceptibles d'être interprétés d'une autre façon lorsqu'on omet *au fait*.

Le fait que l'on ne puisse avoir (à côté de *Pierre croit que Paul a raison*):

(26) **Pierre croit le fait que Paul a raison*

semble tenir à ce que *croire* n'admet pas un substantif tel que *fait* en position directe. En revanche, on peut avoir:

(27) *Pierre croit au fait que Paul a raison*

Dans la construction complétive directe $SN_1 + V_1 + que$ P, *croire* peut signifier, tout comme dans la construction nominale directe et indirecte, „considérer comme vrai”. Mais lorsque ce verbe a, dans la construction complétive, le sens de „estimer probable ou possible”, il ne peut être rapprocher sémantiquement de la construction nominale, qu'elle soit directe ou indirecte. On peut percevoir une nuance de sens entre:

(28) *Je crois qu'il va partir (croire: „estimer probable”)*

et:

(29) *Je crois à son départ (croire: „considérer comme vrai”)*

nuance que l'on peut retrouver aussi entre (28) et:

(30) *Je crois au fait qu'il va partir (croire: „considérer comme vrai”)*

La complétive de (28) peut être pronominalisée au moyen de *le*, alors que *y* peut venir se substituer au SN de (29) et à la complétive de (30).

5. Il nous semble qu'une description syntaxique et sémantique correcte des complétives implique nécessairement que l'on renonce à considérer que ces phrases sont des SN. Dans la grande majorité des cas il n'y a pas d'équivalence

sémantique entre les phrases comportant une expansion nominale et celles comportant une expansion verbale. Lorsque cette équivalence existe, nous nous trouvons confrontés au problème de la dérivation paraphrastique à l'intérieur d'une famille d'énoncés (Culioli, 1976).

Dans:

(31) *Je crains son départ*

(32) *Je crains qu'il (ne) parte*

on peut, sans doute, établir une certaine identité entre *son départ* et *qu'il (ne) parte* et constater qu'il existe un rapport entre, par exemple, l'emploi du pronom possessif et du pronom personnel. Mais l'on peut se demander si une telle assimilation paradigmatique implique une équivalence syntaxique entre le SV et le SN. Quand on choisit le verbe, on choisit également l'expression du temps, du mode et de l'aspect. On peut, à la suite de G. Bernard (1971, p. 58), émettre l'hypothèse que plus la gamme des temps admissibles dans la complétive est étendue, moins il y a de chance de voir s'y substituer une construction nominale.

Dans le cadre d'une grammaire générative du français, les différences sémantiques et syntaxiques que nous avons notées entre la structure nominale et la structure verbale devraient pouvoir être rendues au moyen de traits de sélection et de sous-catégorisation stricte. Il faut toutefois noter que si ces traits nous indiquent *comment* les morphèmes peuvent être agencés, ils ne nous expliquent pas pourquoi certaines combinaisons sont possibles et d'autres impossibles. S'il y a concurrence entre la construction avec un SN et la construction complétive, celle-ci ne peut être expliquée comme relevant *uniquement* de la performance. Le but de l'étude doit être de déterminer si tel choix de construction est motivé sémantiquement et syntaxiquement.

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The Semantic Value of Syntactic Relations

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Since 1978, when I first gave a paper on the topic to the L.A.G.B., I have been working on the semantic value of syntax. It is a substantial issue, and in the confines of the present format I shall not attempt more than to make one or two critical comments on views sometimes expressed about syntax, and to present one or two fragments to illustrate the sort of results I offer.

The basic datum is that words in construction convey more than is expressed merely by the words themselves. Of the several ways to appreciate this, the easiest is to reflect that a sentence such as

(1) I was the shadow of the waxwing slain
has considerably more semantic content than the list which happens to be made up of the same words, separated by semi-colons or commas. That is to say, the syntactic arrangements of the words in (1) are meaningful and not mere arbitrary formulae. I use the term *structural relations* in this paper for such meanings. I believe they constitute a naturally defined area of language description, which might very properly claim the term *syntax* for its own.

However it appears that, quite generally, discussion of syntax in this sense has been bedevilled by the intrusion of matters which should be kept theoretically apart. I take two cases.

First, it should hardly be necessary to make the observation that structural relations are not at all the same thing as physical order. Yet one widely finds implicit acceptance of the view that e.g. (2) and (3):

(2) The cat was stalking an owl

(3) An owl was stalking the cat

differ in their meanings because of the order in which the two noun phrases are mentioned. Presumably this is a fallacy to which English-speaking linguists are especially liable. The fact is that the order of constituents is simply a way to show that, for instance in (2), the relation Subject-verb holds between *the cat* and *was stalking*. It casts no light whatever on what the Subject-verb relation *means*, and to suppose that it could is a category mistake, since it is no more than a form of expression.

Second, we should beware of confusing things and relations that are reflected in syntax, with the (completely different) elements and relations of syntax itself. An adverb is just an adverb. Any temptation to speak of such a thing as an adverb of place is a temptation to talk about two different things at the same time. This sort of temptation is perhaps understandable since in many languages some adverbs of place can appear in sentence positions denied to other

adverbs—for example, as prefixes to the verb, by contrast with independent words following a verb phrase. But there are after all many things and relations which by some historical quirk receive special syntactic treatment in one language or another without giving us the slightest justification for treating them as *being* syntactic elements or relations. Thus *such* alone among English (non-quantitative) adjectives goes in front of an indefinite article rather than after it—we say *such an idea* and not **a such idea*. It does not thereby cease to be an adjective nor is there any reason to suppose that the relation it bears to its noun is in some curious semantic way unique. These observations carry over to the cases of Case Grammar, an important area but not one that I see as concerned with the essence of syntax. To take one example, the distinction between Factitive and Objective objects is one that belongs to the perceived world, and which then may act as the trigger for e.g. the use of a particular affix on the noun. It does not follow that the structural relations involved differ in any way from those involving other affixes, other objects, other verbs. In general we should not imagine that relations between the things which words designate have any bearing on the relations between words themselves as syntactic units.

To turn now to my own ideas, I should like to present without further ado the proposal that there are three fundamental types of semantically relevant relation underlying syntactic constructions. I shall illustrate these by means of various English qualifying constructions. Let us take first sentences like

(4) the graph plunged alarmingly.

There is a straightforward and well-known ambiguity here; *alarmingly* may describe the manner, or comment on the fact, of the event. One temptation is to assume that in the former value it is part of the verb phrase, while in the latter case it is a sentence adverb, possibly moved from elsewhere by a transform. Such transfers certainly can confuse the issue but the value of *factual comment* may also be found *within* the verb phrase. Contrast (5) and (6).

(5) Unexpectedly, nobody arrived

(6) Nobody arrived unexpectedly.

(6) can mean either that no-one arrived in an unusual fashion—the 'manner' value—or that of no-one was it true that their arrival was unexpected; the latter is certainly a 'comment' value, but clearly not to the same effect as in (5). Equally clearly the adverb is in (6) part of the verb phrase and not a sentential adverb.

(5) and (6) together show that one surface syntactic phrase can accommodate two types of semantic link, while a single such semantic bond may be found in two different surface syntactic positions.

Consider next some further familiar instances:

(7) a heavy drinker; a late developer.

Apart from an unusual interpretation in which they are roughly equivalent to (8) a drinker who is heavy; a developer who is late, these seem to convey much the same effect as

- (9) someone who drinks heavily; someone who developed late.

Here, many will at once suggest deriving the usual interpretation of (7) from a structure like (9). Bolinger considered that proposal and rightly rejected it, citing the crippling lack of generality. One can multiply almost indefinitely cases like

- (10) a good marksman; a keen golfer; a terrible flautist

which all have a natural interpretation parallel to (9), yet for which no such verbal origin exists. (Appeal to abstract verbs simply begs the question).

The data however remain, with their manifest double interpretations. I propose that in the more natural of these we merely find again the 'manner' value as in (4) and (6), which I shall call *mode-qualification*. The unlikely interpretation parallel to (8) (but which is the standard one for most adjectives) I describe as one of *character-modification*, where the property of the qualifying word is assigned to the entity of which the head word is predicated.

Beside (7)-(10) with their mode-qualification and character-qualification, we can find instances where the structural relation between adjective and noun is one of *comment-qualification*. Such cases will include

- (11) a true Frenchman; a former wife; an Old Etonian.

Highly desirably, it is possible to produce examples, without recourse to lexical ambiguity, which show a single word functioning in all three ways, thus

- (12) the normal delay ensued (normal in e.g. length; character-q.)

normal eaters don't need to worry about vitamins (mode-q.)

the normal delay ensued (normally a delay followed; comment-q.).

Variation in value is similarly found in predicative position.

- (13) In Seyl's Fever, this sort of rash is usual (character-q.)

The guard outside his room was usual (comment-q.)

- (14) His courtesy was intimidating (comment-q, character-q.).

There will be interaction between the structural relation and the lexical meaning of particular words. Many words will be virtually excluded from one or more types of qualification; *however* could scarcely be anything but a comment; *a strong socialist* may involve mode-qualification, but a *strong arch* must surely exhibit character-modification.

Similarly there are correlations with syntactic position. The ordinary case is for adjectives to show character-qualification, verbal adverbs mode-qualification, and sentential adverbs comment-qualification, but contrary examples occur for the latter two types as well as for the adjective. An interesting matter is raised by sentences like

- (15) the agent acted incompetent and helpless

Incompetent and *helpless* clearly qualify the verb and not the noun; (15) is not equivalent to e.g. *the incompetent agent acted helpless*. Equally they do not show mode-qualification; the performance may have been skilful. Apparently English here responds to the presence of character-qualification by using the adjective form even though elements qualifying a verb 'ordinarily' take an adverbial form.

Recognition of these three varieties of structural relation enables us to do several things. First of all it describes the ways structures are actually interpreted. It accounts for many ambiguities, some of which cannot easily be dealt with otherwise. It offers interpretations for 'deviant' phrases such as *an occasional sailor*. It can handle certain missing co-ordinations such as *a missing and red book*. And it explains why some transforms seem to work, while, bearing in mind the interaction with the lexical factor, it may even show why they sometimes fail.

On Reciprocity

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1. Reciprocal verbs have eluded linguists for a long time. Jespersen's brief contributions (1924: 161, 234; 1933: 112, 180; 1937: 24) may be regarded as the latest substantial attempt to understand this phenomenon. His view may be summed up as follows: (1) reciprocal verbs and reflexive verbs are identical; the only difference being the identify or non-identity of subject and object; (2) reciprocity is expressed by transitive verb and reciprocal pronoun, such as E. *each other*, *one another*, Fr. *l'un l'autre*, G. *einander*. However, he added that "... In English the verb in itself suffices to express reciprocity" (1924: 161)—(It should be noted that this additional remark has escaped later scholars who deal with reciprocal verbs).

Several transformationalists have tried to analyse reciprocals, but because of their main preoccupation with reciprocal pronouns, especially in English, rather than with reciprocal verbs, they have gained no further than Jespersen's contribution.

2. The purpose of this paper is to refine and revise Jespersen's analysis, by examining reciprocity as found in Indonesian with the hope that by comparing this phenomenon in other languages, a cross-linguistic generalization can subsequently be formulated.

It is claimed here that by examining reciprocity from the syntactic, pragmatic and semantic viewpoints, we will be able to rectify what weaknesses there are in Jespersen's analysis, namely that he could not differentiate reciprocals from reflexives, and that reciprocity involves only subject and object. The first weakness can be attributed to the fact that Jespersen and other scholars after him based their analyses only upon English and other Indo-European languages; and the second to the fact that at his time no theoretical distinction has been made between concepts such as *agent* and *subject*, and *goal* or *recipient* and *object* (although in practice he distinguished them).

3. Indonesian has various morphological and lexical devices to differentiate reciprocal verbs from other verbs. They are found in the forms: (1) *ber* + inherently reciprocal verbal or verbalized root, e.g. *berkelahi* 'fight', *berperang* 'be at war with'; (2) *ber* + verbal root + *an*, *bersentuhan* 'touch each other'; (3) *ber* + reduplicated verbal root + *an*, e.g. *bermaaf-maafan* 'forgive one another'; (4) *saling meN* + verbal root, e.g. *saling memberi* 'give each other'; (5) *baku* + verbal root, e.g. *baku tembak* 'shoot at each other'; (6) verbal root, +

meN + verbal root₁, e.g. *tolong-menolong* 'help each other'; (7) reduplicated verbal root + *an*, e.g. *cubit-cubitan* 'pinch each other', *senggol-senggolan* 'nudge one another'; (8) *saling ter* + verbal root, e.g. *saling tertarik* 'mutually attracted'; (9) *saling ke* + verbal root + *an*, e.g. *saling kehilangan* 'feel lost each other'.

The *ber* + root + *an* form is convertible to the *saling men* + root form with a slight difference in the degree of transitivity: the first is less telic than the second (for the term telic and related concepts see Hopper and Thompson 1980).

Of all those forms, the *saling meN* + root form is the most productive; the *baku* + verbal root form is used mostly in the eastern dialects of Indonesian; and the reduplicated verbal root + *an* form is used in non-standard Indonesian.

In contrast with other verb forms, the deverbalization of reciprocal verbs is very limited. The two most productive forms are the *ber* + inherently reciprocal verbal root which can be nominalized into *per* + inherently verbal root + *an* (e.g. *berkelahi* 'fight' > *perkelahian* 'fight'), and the verbal root₁ + *meN* + verbal root₁ which can be zero nominalized (e.g. *tuduh-menyuduh* 'accuse one another' > 'accusation between one and another'). Whereas the other forms cannot productively be deverbalized: *saling memukul* 'strike each other' > **saling pemukulan*, *baku tembak* 'shoot at each other' > **baku penembakan*. The only exception is *saling mengerti* 'understand each other' > *saling pengertian* 'mutual understanding'.

Another aspect that deserves mention here is the fact that several reflexive verbs can be reciprocalized, e.g. *menyesuaikan diri* 'adapt one's self' > *saling menyesuaikan diri* 'adapt themselves for each other', *memperkenalkan diri* 'introduce one's self' > *saling memperkenalkan diri* 'introduce themselves to each other'. This fact weakens the generalization put forward by Jespersen that reciprocal verbs and reflexive verbs are identical.

4. Syntactically, a reciprocal verb always governs a plural subject; and where that action involves clearly two participants, two parties or two sides, the subject must be dual. Hence the ungrammaticality of the following:

- (1) **Sukri berunding* 'Sukri is deliberating'.

Pragmatically, if one of the participants is foregrounded, it will be realized as singular subject with the other noun functioning as an obligatory complement:

- (2) *Sukri berunding dengan istrinya* 'Sukri is deliberating with his wife'.
If no foregrounding on one of the participants is considered, the dual subject is obligatory; so we will have:

- (3) *Sukri dan istrinya berunding* 'Sukri and his wife are deliberating'.

Problems arise when the number of participants is uneven: while the simple declarative sentence with a singular subject

- (4) **Ia berkelahi* 'He fights' is unanimously rejected by our informants, the sentence (also with singular subject)

- (5) *Ia sedang berkelahi* 'He is fighting' is accepted as grammatical with

the understanding that it is an elliptical sentence; if not, it is rejected as ungrammatical.

No unanimity is reached by the informants with the sentence

(6) ?Ketiga orang itu berkelahi 'The three people fight', although all of them accepted the grammaticality of

(7) Kelima orang itu bersalaman 'The five people are shaking hands'. While all of the informants accepted also

(8) Kedua orang itu bertinju 'The two men are boxing', the sentence

(9) *Ketiga orang itu bertinju 'The three men are boxing' is unanimously rejected, although

(10) Aman bertinju dengan Didi dan Koko 'Aman is boxing with Didi and Koko'

is regarded as grammatical, obviously because two sides are involved: *Aman* on the one hand, *Didi* and *Koko* on the other.

5. Jespersen (1924: 161) gave the impression that reciprocal verb involves only two participants, subject and object. The following Indonesian data present more complicated picture:

(11) Kiki dan Koko bertukar tempat 'Kiki and Koko exchange places'

(12) Kedua sarjana itu berperang pena di surat kabar 'The two scholars fight with pens (=are having polemic) in the newspaper'.

Each of these two sentences has three-term reciprocal verbs, and their role structure can be represented as follows:

(11a)	V	Goal	Agent \longleftrightarrow Beneficiary	
	reciprocal			
	atelic			
	bertukar	tempat	Kiki	Koko
	'exchange'	'place'		
(12a)	V	Instrument	Agent \longleftrightarrow Goal	
	reciprocal			
	atelic			
	berperang	pena	sarjana ₁	sarjana ₂
	'to make war'	'pen'	'scholar ₁ '	'scholar ₂ '
	(= fight)			

Although these two sentences have the propositional structure of Agent-Verb-Goal which makes them fully active sentences, no passive counterpart can be formed. We may make the generalization that no reciprocal clause can be passivized.

This is valid also for sentences like

(13) Keduanya merasa saling kehilangan 'Both feel lost each other'

(14) Kami merasa saling tertarik 'We feel mutually attracted'

which have intransitive verbs with experiential affixes *ke—an* and *ter—* respectively.

6. The role of each participant in (11) is clear, as we have here the case of "who for whom" or "who against whom" reciprocity. It is less so, however, in (2) and (12), as we have here the case of "who with whom" reciprocity. Beside *berunding* 'to deliberate' and *berperang* 'to make war', other reciprocal verbs which involve "shared or common action" rather than "chiasmic action"—or "crosswise relation" to use Jespersen's term—are *berkelahi* 'fight', *bertengkar* 'quarrel', *bercakap-cakap* 'converse', *bersepakat* 'agree', *bergulat* 'wrestle', *bersamaan* 'coincide', *bergumul* 'wrestle', *beradu* 'collide, clash', *berlaga* 'fight', *bersua* 'meet'. Semantically, the participants of the latter group of verbs are all agent or actor, and there are neither goal nor beneficiary nor recipient; syntactically, the verbs cannot be converted to their corresponding telic reciprocal form *saling meN + root*, which is the form of chiasmic reciprocity. (Note that the English glosses of the data are not informative enough to understand the fact just outlined. Undoubtedly, the English verb *fight* is mutual reciprocal verb, as it is clear 'who fight whom').

7. In this short paper we have shown that in Indonesian we have to distinguish between reciprocal verb and reflexive verb, that reciprocity governs even subject more than uneven subject, that it may involve more than two participants, and that no passive counterpart can be found for any active reciprocal sentence. Semantically, a distinction can be made between mutual reciprocity and chiasmic reciprocity.

To recapitulate, the study of reciprocity from the syntactic, pragmatic and semantic viewpoints deserves a place in general linguistics. The Indonesian data presented in this paper exemplify the line of research that could be carried out in other languages.

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Nominalizations in Korean

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There are three major types of nominalizations (i.e. nominals derived from verbs or adjectives) in Korean, and they are expressed by the suffixes *-i*, *-ki* and *-um*. The purpose of this paper is to discover the meaning of each of the suffixes. The meaning of *-i* is rather straightforward and it poses no great difficulty. However, the other two suffixes (*-ki* and *-um*) have been the subject of numerous studies, and no study seems to have succeeded in differentiating the two suffixes.

Some studies, carried out in the structuralist framework, simply classified and labelled the morphemes. Others in the transformational framework treated them as meaningless elements, whose sole function is to fill in gaps in the structure of sentences. In such an analysis, the co-occurrence restrictions between a nominal and a predicate are accidental and arbitrary. But in this paper, it is shown that even the so-called functional morphemes have their meanings, although the meanings must be schematic and abstract.

The three types of Korean nominalizations seem to reflect human perception and cognition, and they seem to bear out the findings made by Vendler (1968), Lyons (1978) and Givón (1979). Lyons, for example, classifies the nominals into three categories: first-order entities (physical objects); second-order entities (events, processes, states of affairs), which are located in time and are said to occur or to take place; and third-order entities, which are outside of space and time.

Nominals expressed by *-i* correspond to the first-order nominals:

cap-ta 'to hold'

cap-i 'handle'

kup-ta 'to curve'

kup-i 'curve'

Nominals expressed by *-ki* correspond to the second-order entities:

cap-ta 'to hold'

cap-ki

kup-ta 'to roast'

kup-ki

The stem itself (*cap-* or *kup-*) denotes the type of action only. The nominal *cap-ki* denotes temporal existence of the action. The existence can be real and unreal depending upon context:

na-nun ku-ka o-ki-lul pala-nun-ta. (not realized)

I TM he-SM come want-present-

'I want him to come.'

ku-nun- cwuk-ki-ey ilulu-ess-ta. (realized)

he-TM die -to come-past-

'He came to die.'

Predicates such as *kkuthnay-ta* 'to finish' and *sicakha-ta* 'to begin' accept only *ki*-nominals:

*ku-nun phyenci ssu-ki-lul/*ssu-um-ul kkuthnay-ess-ta.*

he-TM letter write finish-past

'He finished writing the letter.'

Nominals expressed by *-um* correspond to the third-order nominals and they do not have the meaning of the temporal existence. They cannot be said to occur or to take place in either time or space. They are something to be asserted or denied, remembered or forgotten. Thus, the *um*-nominals occur with verbs of proposition such as:

alta 'to know'

icta

'to forget'

nukkita 'to feel'

mitta

'to believe'

And these verbs do not occur with *ki*-nominals.

*na-nun ku-ka o-um-ul/*o-ki-lul ic-ess-ta.*

I TM he-SM come

forget-past-

'I forget about his coming.'

So far it is shown that the three types of nominalization correspond to Lyons' classification of nominals. But the correspondence is not complete. Some of the nominals are completely lexicalized, and it is impossible to find complete regularity. For example, in the following nominals the suffixes *-i*, *-ki* and *-um* are used to derive the first order nominals:

mi tat-i

'sliding door'

(*tat-ta* 'to close')

mos ppay-ki

'nail puller'

(*ppay-ta* 'to pull out')

el-um

'ice'

(*el-ta* 'to freeze')

Except for such lexicalized nominals the correspondence is regular.

The semantic characterization of the three suffixes throws some light on the meanings of the suffixes *-kela*, *-kena*, and *-key*. *-kela* was thought to be a variant form of *-ela* 'imperative.' But in this paper it is claimed that it is made up of the two morphemes: *-ki* and *-ela*. Accordingly they differ in meaning. Compare the two imperatives below:

Ca-ela. 'Sleep'

Ca-kela. 'Sleep'

The first imperative simply names a type of action, whereas the second one focuses on the temporal existence of the action: the name of the action is presupposed. Similarly, *-una* names an alternative action, whereas *-kena* is used where the name is presupposed. Observe the following conversational give-and-

take:

- A: *ku-selam nayil o-nun-ta-te-la.*
the man tomorrow come—
'I heard that the man is coming tomorrow.'
- B: *o-kena mal-kena. (*o-una mal-una.)*
come not
'(I don't care whether) he is coming or not.'

From what A said, the name of coming can be presupposed, and *-una* is not acceptable in B's response. What is at issue here is the temporal existence of the man's coming and only *-kena* form is allowed.

The causative morpheme *-key* 'causative' was thought to be a single unanalyzable morpheme. But it seems to be made up of the two morphemes *-ki* 'existence' and *-ey* 'goal postposition.' Through this analysis, the meaning of causativization becomes clearer. In one sense, causativization can mean bringing a state, a process, or an action *into existence*. The existence is expressed by *-ki* and the change (of state) by *-ey*, as in the following:

- na-nun ku-ka ka-ki-ey mantul-ess-ta. (ka-ki-ey is contracted into kakey)*
I -TM he-SM go- -to make-past-
'I made him go.'

At one point of time, he was in the state of not going: his going was not in existence. Later I made his going come into existence.

Lastly, *-ki* is used to emphasize that the temporal existence of a state, an action, etc. is in question. Observe the following:

- A: *ku sikyey olay toyess-kwuna.*
the watch old become—
'I see that the watch is very old.'
- B: *kulena cal ka-nun-ta.*
but well go-present—
'But it goes well.'
- C: *kulena ka-ki-nun cal ka-nun-ta.*
'But it does go well.'

The two utterances (B and C) have the same propositional content. But C differs from B in that C emphasizes the temporal existence of the process.

To summarize, I have started with the semantic analysis of the three nominalizing suffixes (*-i*, *-ki* and *-um*), and the resulting meanings make it possible first to distinguish the two pairs of suffixes (*-ela* and *-kela*; *-una* and *-kena*), and second to analyse the causative suffix *-key* into *-ki* and *-ey*, which is more consistent with the definition of causativization.

A Montague Grammar for Case Languages

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This paper proposes a grammar system for case-marking languages based on Montague's PTQ theory (1974). The newly proposed system RPTQ, as I claim, accommodates case assignment and other related problems such as free word order and derived complex predicates in Korean and Japanese and possibly in other case languages. This accommodation will be shown to be successful with some slight modification of PTQ.

RPTQ mainly differs from PTQ in defining syntactic categories. Unlike PTQ, RPTQ introduces denumerably many basic category indices. Each ordinal number $0, 1, 2, \dots$ is a basic category such that 0 corresponds to t in PTQ and each of the other numbers n , to e in PTQ. A derived category (A, B) corresponds to B/A or $B//A$ in PTQ: an *IV*-category is thus defined in RPTQ as $(n, 0)$, a *T*-category as $((n, 0), 0)$ or simply n^* , and a *TV*-category as $(n^*, (m, 0))$, where n and m are both basic entity denoting categories like e in PTQ.

Case assignment in RPTQ employs three basic devices of case marking, case indexing, and case shifting. Case marking simply attaches a case particle to a term phrase (Maria) of the category n^* , replacing the parameter n with a case index while keeping its denotation. The case-marked term phrases *Maria-ka*(*nom*), *Maria-lil*(*acc*), and *Maria-eke*(*dat*) in Korean or *Maria-ga*(*nom*), *Maria-o*(*acc*), and *Maria-ni*(*dat*) are of categories 1^* , 2^* , and 3^* , respectively. They are all of the same semantic type $\langle\langle s, \langle e, t \rangle \rangle, t \rangle$, denoting a set of properties of an individual.

Case indexing subcategorizes various types of verb phrases with respect to the choice of case-marked term phrases. An *IV*-phrase like *ca-sleep* in Korean or *nemu-sleep* in Japanese belongs to the category $(1, 0)$ denoting a set of individuals, since it forms a well-formed sentence by taking a nominative case-marked term phrase like *Maria-ka* or *Maria-ga* of the category 1^* . A *TV*-phrase like *saranha-love* in Korean or *tabe-eat* in Japanese belongs to the category $(2^*, (1, 0))$ denoting a two-place relation between two individuals, for it concatenates with an accusative term phrase deriving an *IV*-phrase of the category $(1, 0)$ and then with a nominative term phrase to form a sentence as shown in:

- (1) a. $[[\text{Mary-ka}]_1, [[\text{Bill-}i]_2, [\text{saranhanta}]_{(2^*, (1, 0))}]_{(1, 0)}]_0$
 'Mary loves Bill'

This order of concatenation is, however, free in RPTQ. As in (1b), a nominative term phrase may concatenate with a *TV*-phrase of the category $(2^*, (1, 0))$ before an accusative term phrase does, deriving an *IV*-phrase of the

category (2,0):

- (1) b. $[[\text{Bill-il}]_2, [[\text{Mary-ka}]_1, [\text{saranhanta}]_{(2^*,(1,0))}]_{(2,0)}]_0$

Note that in RPTQ subcategories (1,0) and (2,0) of verb phrases are well-defined syntactic categories, both denoting a set of individuals, but only differing syntactically in each having a different case index.

Despite a difference in word order, sentences (1a) and (1b) are synonymous, for their respective translations are derived to be equivalent in RPTQ:

- (2) a. $\lambda\text{PP}\{m\} (\text{'love'}(\lambda\text{PP}\{b\}))$
 b. $\lambda\text{PP}\{b\} (\lambda x[\text{'love'}(\lambda\text{PP}\{x\})(m))]$

Syntactically analyzed sentences (1a) and (1b) differ from each other as to the order of concatenation while their respective translations (2a) and (2b), only as to the scope of binding: the term phrase of $\lambda\text{PP}\{b\}$ of (2b) is in wide scope and binds the variable x by way of abstraction λx . This abstraction makes (2b) equivalent with (2a). In deriving (1b), no scrambling of the term phrases has occurred.

Concatenation in RPTQ is strictly constrained by the case indexing of each verb phrase in a given language. A term phrase may concatenate with a verb phrase only if the case marker of the former occurs among the case indices of the latter. Otherwise, ill-formed strings are derived:

- (3) a. Korean: $*[[\text{Marry-lil}]_2, [\text{ča-n-ta}]_{(1,0)}]$
 b. Japanese: $*[[\text{Marry-ni}]_3, [\text{nemu-ru}]_{(1,0)}]$

But the following concatenations, for instance, derive well-formed Japanese sentences all having the same meaning:

- (4) a. Mary-ga nihoNgo-o $[\text{waku}]_{(2^*,(1,0))}$
 b. Mary-ga nihoNgo-ga $[\text{waku}]_{(1^*,(1,0))}$
 c. Mary-ni nihoNgo-ga $[\text{waku}]_{(1^*,(3,0))}$

These sentences are well-formed because the case markers of the term phrases in each sentence match with the case indices of the verb *waku-understand*. Here the variation of case assignment in (4a,b,c) is accounted for at the lexical level by treating a basic verb expression like *waku* as belonging to the categories $(2^*,(1,0))$, $(1^*,(1,0))$, and $(1^*,(3,0))$.

Finally, case shifting accounts for the alternation of case assignment that arises in complex predicate constructions such as desideratives, potentials, passives, and causatives in both Korean and Japanese. In Korean, for example, we obtain a well-formed sentence like (5a), but (5b) is ill-formed because *mak-eat* is of the category $(2^*,(1,0))$:

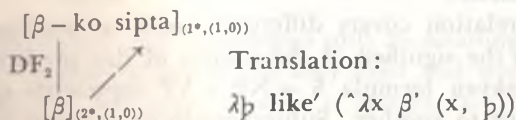
- (5) a. nae-ka sakwa-lil $[\text{mak-əs'-ta}]_{(2^*,(1,0))}$
 'I ate an apple'
 b. *nae-ka sakwa-ka $[\text{mak-əs'-ta}]_{(2^*,(1,0))}$

Note, however, that (6b) and (6a) are both equally acceptable, although (6b) contains two nominative marked term phrases.

- (6) a. nae-ka sakwa-lil məkko-sipta.
 b. nae-ka sakwa-ka məkko-sipta.
 'I like to eat an apple'

I claim in proposing RPTQ that such case variation is possible in Korean because case shifting may occur in forming desiderative verbs from transitive verb stems as can be seen from the following desiderative formation rule for Korean in RPTQ:

(7) *Desiderative Formation 2:*



In deriving a desiderative mək-ko sipta, a case shifting from 2^* to 1^* may occur in the first coordinate of the category index of mək \rightarrow that the newly derived verb məkko sipta is assigned a new category index $(1^*, (1,0))$. Because of such a case shift in DF_2 , (6b) is obtained simply by the ordinary rules of concatenation that retain their functional character of combination.

Case shifts in Japanese potentials are also easily handled by similar case shifting lexical rules, yielding grammatical sentences:

- (8) a. Mary-ga nihoNgo-o [[hanas]_{(2^*, (1,0))}-e-ru]_{(2^*, (1,0))}
 b. Mary-ga nihoNgo-ga [hanas-e-ru]_{(1^*, (1,0))}
 c. Mary-ni nihoNgo-ga [hanas-e-ru]_{(1^*, (3,0))}
 'Mary can speak Japanese'

Note that this complex derived potential hanas-e-ru behaves exactly like wakar and that neither belongs to the category $(2^*, (3,0))$. So sentences (9a,b) are ill-formed;

- (9) a. *Mary-ni nihoNgo-o wakar.
 b. *Mary-ni nihoNgo-o hanaseru.

Because of case shifting, complex predicates like hanas-e-ru are treated like a simple basic predicate wakar. We can thus obtain sentences like (8a,b,c) by concatenating a complex predicate with appropriately case-marked term phrases.

Case shifting is a nice descriptive device because it minimizes the complexity of describing case assignments for complex predicates and keeps this complexity down at the lexical or phrasal level without carrying it all the way up to the sentential level. Finally, case shifting preserves a Montague syntax as a simple categorial grammar based on the principle of compositionality.

Semantics of Parts of Speech and Semantics of Relations, (examples in Tagalog)

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It has often been said that grammatical relations have no sense. Maybe but they cover phenomena very different from a language to another, and these phenomena pertain to semantics.

Thus subject-predicate relation covers different modes of attributing the signified of the predicate to the signified of the subject of this predicate. The sign + of the famous chomskyan formula $S \rightarrow NP + VP$ represents quite different things from a language to another. Subject-predicate relation cannot be the same in languages where all the major parts of speech have exactly the same behaviour as regards functions and in other languages where, for instance, verb behaves a way of its own, quite different from that of nouns, and is necessarily present in all the predicates at least as a copula.

Observe firstly the identical behaviour of Tagalog nouns, adjectives and verbs when predicate:

- ex. 1 *kumuha ng libro ang bata* "a/the child got a book"
having-got book child
- ex. 2 *kinuha ng bata ang libro* "the book was got by a/the child"
being-got child book
- ex. 3 *maganda ang dalaga* "a/the girl is pretty"
pretty girl
- ex. 4 *doktor si Pedro* "Pedro is a doctor"
doctor

and when attribute (relativization):

- ex. 5 *bata-ng kumuha ng libro* "child who got (having-got) a book"
child having-got book
- ex. 6 *libro-ng kinuha ng bata* "book which the child got (got by the child)"
book got child
- ex. 7 *dalaga-ng maganda* "girl who is (being) pretty"
girl pretty
maganda-ng dalaga "pretty girl"
pretty girl
- ex. 8 *si Pedro-ng doktor* "Pedro the doctor"
doctor

Here the transformation will consist only in preposing the *na* marker and (-ng

after vowel) and in word reordering without changing anything in the form of the relativized element itself. Let's observe now how the same parts of speech behave after *ang*:

- ex. 9 (*ang doktor*) *ang kumuha ng libro* "the one who got a book
having-got book (is the doctor)"
- ex. 10 (*ang libro*) *ang kinuha ng bata* "the one which was got by the child
book is-got child (is the book)"
- ex. 11 (*ang maganda* (*si Maria*)) "(Maria is) the pretty one"
pretty
- ex. 12 *ang doktor* (*si Pedro*) "(Pedro is) the doctor"
doctor

Should we regard such homologies only as meaningless morphological accidents although they are stable where they exist and widespread all over the world? Can linguistics only state, or must it also explain facts?

But the problem is: how to explain such facts? I'll answer: by semantics.

A first basic principle is not to treat differently what is alike: the identical behaviours of nouns, adjectives and verbs as to substantivization, relativization and predicative function define a particular distribution and semantics of parts of speech: as long as functions are concerned, they form only one "superpart" of speech.

If we translate or gloss all parts of speech by means of a "being X" or of a "doing X", all become clear. The verb itself is orientated towards the term to which it refers like an adjective or a noun: *dalagang maganda* (ex. 7) can be translated into "girl being pretty" and *ang maganda* (ex. 11) "a pretty one"; so, *ang kumuha ng libro* (ex. 9) can be translated into "the one having got a book". The verb functions always as a kind of participle or verbal adjective: when it is the predicate, the sentence must be understood as a kind of nominal sentence with a participle as predicate and without copula as it exists in sanskrit: for example (ex. 13): *sa vas dṛṣṭavān* (without sandhi) which means "he=having-seen you". So, *kumuha ng libro ang bata* (ex. 1) must be understood as "a/the child (is) getting a book". The Middle Ages rewriting rule (ex. 14) *cantat=est + cantans* is a reality and no longer a fiction in this type of languages except that there is no copula but mere juxtaposition and preposing to mark the predicate. And as the three major parts of speech, which all can be predicate, behave alike, we can say that they all must be understood everywhere as "being X" and even *ang doktor* is basically "the one being a doctor". Then we can understand why verbs can be substantivized or function as an attribute exactly in the same way as noun and without any change of form. All that defines a peculiar mode of orientation.

The comparison between equative and non-equative sentences enables us to understand another side of the semantics of parts of speech. Through non-equative sentences (as ex. 1, 4), we are only told that Pedro has the quality of being a doctor or of getting a book, while through equative sentences (ex. 9, 12)

we are told that the child or Pedro is the same person as the one who has been previously identified as a doctor or as having got a book. *ang* indicates persons or things as individualized while Ø—that is to say every major part of speech used alone—indicates the quality of being something; *ang* indicates substances and Ø attributes in the traditional philosophical sense of the words; I prefer to name this superpart of speech “qualificatives” in order to avoid any misunderstanding.

Such distribution and meaning of parts of speech have implications as to semantics of all grammatical relations. The subject-predicate relation is a relation of equivalence between two qualificatives. In ex. 1, as well as in ex. 9, *bata* and *kumuha ng libro* are in fact two equivalent designations of one and the same object of the real in a given situation, that is why both can be substantivized in order to name this object.

Tagalog and English or French, for example, use very different ways to express the relation between the child and the action itself of getting. In Tagalog, there is on one side, a relation of equivalence between “child” and “(one) having got”, and on the other side, the relation between “having got” and the action of “getting” as an abstract idea without particular orientation: there is in Tagalog a series of what I have called participles expressing the qualities of the different possible participants in a given process, such as actor, patient, beneficiary, goal, location, purpose, and all the other well-known focuses proper to Philippinian languages, but all these qualificatives, derived from a same stem by different affixes, are always in the same relation of equivalence with the word they qualify.

In french, for example, in *l'enfant obtient*, *obtient* is orientated towards the prime actant or subject which is the actor like the tagalog *kumuha*, but the mode of orientation is quite different and, as *obtient* is not *obtenant* or *qui obtient*, it is not possible to speak of equivalence relation in the precise sense in which I have used this word here.

How does Tagalog express relations other than those expressed inside of the verbal qualificative? Tagalog distinguishes only three relations between the elements of the real as expressed by words: firstly, the already mentioned relation of equivalence—between subject and predicate and between the two terms linked by means of *na*; secondly, a relation marked by *ng*, and thirdly, another one marked by *sa* and other markers containing *sa*. These markers introduce noun extensions as well as governed complements and even adverbials and predicates in the case of *sa*. Thus *ng* is a genitive accusative and actor marker!

We are again in front of puzzling homologies. Semantics appears again to provide the key of the mystery. Instead of considering these markers to be purely grammatical and even to constitute as many homophonous items as there are functions, it is preferable to look for a constant meaning proper to each marker throughout its different uses since function is sufficiently marked by the sequential constraints: noun, adjective, numeral extensions are postposed to head noun, adjective or number; predicate *sa* phrases are at the first place in

the sentence as all predicates; verbal complements and adverbials are not integrated, but in spite of some sequential freedom, there may be a significant preferential word order.

ng expresses close relationships such as possession, relation between a part and the whole, intrinsic characteristic, when introducing a noun extension; when introducing a verb extension, *ng* expresses the relation between actor or patient and the action, since they are the closest participant (ex. 1, 2). *sa* expresses looser relationships such as direction and circumstances when introducing complements, as well as adverbials or predicates.

Thus there are three relations: equivalence versus close relationship versus loose relationship. To choose between them is a matter of real situation: it depends upon the situation that Pedro is identical with the doctor or that the car is belonging to Pedro, and so on. It is a matter of situation once given the categorization of the real according to which the language, as every language, must classify the elements of the real.

Is this semantics? Yes it is if categorizations are the very basis from which the informal reference is transformed into something discrete and articulate which can be communicated. It is grammatical as far as the classification of the elements of the real into parts of speech and subclasses of parts of speech as well as the classification of the relations between these elements are facts of language. But the expression of these real relations and the indication of the hierarchy of the utterances (through topicalisation and subordinations of all kinds) are not the same thing and here, the word "grammatical" is, as often, ambiguous.

Semantics has been the only way, firstly, for preserving the logic of the system of the language and even for identifying a certain kind of information communicated by the language (an abstract, logical one such as categorization of the elements of the real and of the relations between them), secondly to found a general linguistics or a typology capable of explaining and not only stating the homologies and other specific facts.

Meaning-Text Linguistic Models and the Role of the Dictionary in Linguistic Description

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(I) 1. From the Meaning-Text viewpoint (cf. Žolkovskij-Mel'čuk 1967, Mel'čuk 1974, 1981), the study of natural language is the construction of FUNCTIONAL MODELS of languages, i.e. systems of rules that imitate (= "predict") linguistic behavior. 2. The most important feature of the linguistic competence of speakers is their ability to establish CORRESPONDENCES between any given MEANING and all the TEXTS that carry it (and vice versa; but see I. 9). This feature takes priority in a Meaning-Text Model (MTM) of a language over any other characteristics of language (such as language acquisition; linguistic variety and change; drawing inferences from and/or disambiguating texts; all these are not purely linguistic). 3. An MTM can be diagrammed as (1):

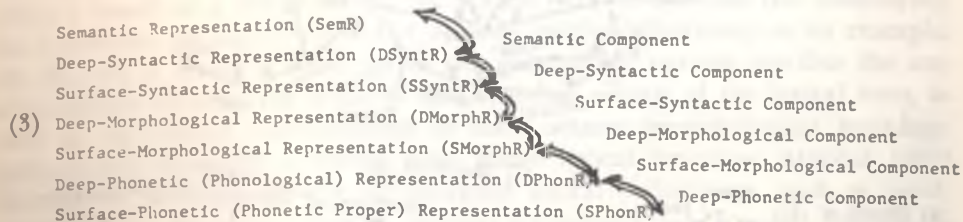
$$(1) \left\{ \text{MEANING}_i \right\} \xleftrightarrow{\text{language}} \left\{ \text{TEXT}_j \right\} \mid 0 < i < \infty, 0 < j < \infty$$

An MTM is only a part of the model which covers the entire range of linguistic behavior:

$$(2) \left\{ \text{REALITY} \right\} \xleftrightarrow{\text{perception, logic, ...}} \left\{ \text{MEANING}_i \right\} \xleftrightarrow{\text{language proper}} \left\{ \text{TEXT}_j \right\} \xleftrightarrow{\text{phonetics}} \left\{ \text{ACTUAL SOUND} \right\}$$

Although the correspondences between extralinguistic reality and linguistic meanings are of utmost importance for linguistic communication, they do not belong to language and should be excluded from linguistic models. (They require different means and methods for their description.) An MTM is thus limited by meaning as described by the Semantic Representation (SemR) of utterances and text as described by the Phonetic Representation (PhonR). 4. Due to the fact (a) that the Meaning-Text correspondence is of a many-to-many nature (synonymy and homonymy) and (b) that it easily lends itself to being described in terms of independent mechanisms, two INTERMEDIATE LEVELS of representation are introduced (consistent with tradition): syntax and morphology. Each of our levels, except the semantic one, is split into a DEEP (sub)level, geared to meaning, and a SURFACE (sub)level, oriented towards

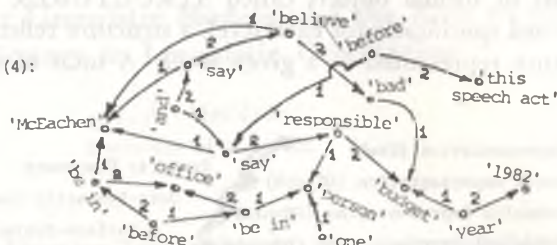
sound. 5. On each level, an utterance appears as a formal REPRESENTATION, which is a set of formal objects called STRUCTURES; written in a formal language devised specifically for each level, a structure reflects a particular aspect of the utterance represented at a given level. A total of seven representations is adopted:



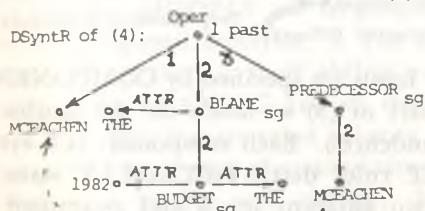
6. The correspondences between adjacent levels are specified by COMPONENTS of an MTM (listed in the right-hand part of (3) as labels on the double bi-directional arrows standing for 'correspondence'). Each component is a system of UNORDERED CORRESPONDENCE rules that STATICALLY state the relationship between the units of the two adjacent levels and guarantee the observance of their compositional properties. (The dynamics of rule application, i.e. the process which actually leads from a representation of level n to a representation of level $n + 1$, is intentionally excluded from MTMs as being non-linguistic.) 7. The FORMALISMS used are: in SemRs, labeled networks without circuits; in SyntRs, unordered dependency trees with labeled arcs; in MorphRs and PhonRs, strings. (The more familiar P(hrase) S(tructure) formalism is banned from the MTM because it does not explicitly show subordination relations and because it imposes a linear order, which is irrelevant at the semantic and syntactic levels.) Examples of three MTM representations (highly simplified) for sentence (4) are given below: (5)–(7). 8. An MTM is neither GENERATIVE nor TRANSFORMATIONAL. Nothing is generated since meanings and texts are considered as given. (The set of all possible meanings expressed within a given dictionary can be specified by almost trivial rules; the set of all possible texts is specified by an MTM itself.) Nothing is transformed, either, since rules applied to a representation of level n do not change it but simply construct for it a representation of level $n + 1$. An R of level n serves as a blueprint for an R of level $n + 1$ and is not itself converted into it: Rs of two different levels are quite different in nature. 9. An MTM is, in principle, bidirectional, but the meaning-to-text direction is judged to be more important: when expressing a given meaning, a speaker uses (almost) exclusively his linguistic competence but when understanding a given text he usually has recourse to logic, encyclopedic knowledge, understanding of previous texts, etc., which lie outside linguistics. (This reflects the basic asymmetry between the speaker and the listener, of whom the first is logically more important.) Therefore, the MTM concentrates on the expression only, i.e. on the production of grammatical texts for given meanings.

- (4) *McEachen put the blame for the 1982 budget on his predecessor.*

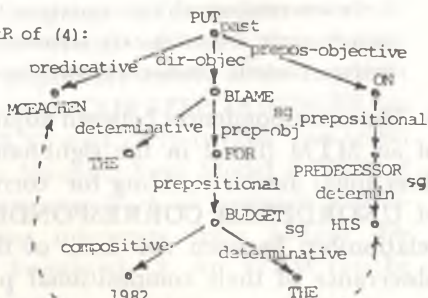
(5) SemR of (4):



(6) DSyntR of (4):



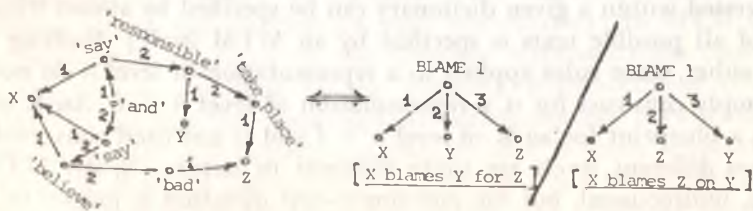
(7) SSyntR of (4):



[No explanations can be supplied for lack of space. The reader is kindly asked to consult the references. The only important warning: the communicative structures having to do with topic-comment division, etc., are not shown.]

(II) 1. The most important part of any MTM is the SEMANTIC COMPONENT, which includes a dictionary specifying the correspondence between fragments of the SemR and the lexemes of the language. (It is the lexemes that, once chosen, impose a particular syntactic pattern on the sentence and fully determine all its morphological features.) A typical semantico-lexical rule (of English):

(8)



[X blames Y for Z 'X says that Y is responsible for Z, which X says he believes is bad']

Such a rule is a dictionary entry for one sense of BLAME (distinguished by the index "1"). 2. A new type of dictionary is proposed: the Explanatory Combinatorial Dictionary (ECD; Žolkovskij-Mel'čuk 1967; Mel'čuk 1978; Mel'čuk *et al.* 1981). It must contain all the linguistic information about individual lexemes and thus form a basis for a grammar, not an appendix to it. In contrast

to the widely-accepted view that a linguistic description is first and foremost a GRAMMAR, it is claimed here that the central component of any linguistic description should be a DICTIONARY. (Of course a grammar is also needed: to express useful generalizations and introduce convenient abbreviations.) 3. An entry of the ECD contains three main zones: (a) A semantic zone that provides a SemR or a definition for the lexical item in question (no homonymy, no synonymy, no logical circles are allowed in the definitions); as an example, cf. (8). (b) A syntactic zone, where the government pattern specifies the correspondence between the semantic and syntactic actants of the lexical item, as well as the surface manifestations of these actants (morphological markings, prepositions, etc.). (c) A lexical zone, where lexical functions (Mel'čuk 1982) describe in a systematic way all restricted lexical collocations, such as *vivid: contrast ≈ greatly: vary ≈ staunch: believer ≈ notorious: bore ≈ snow-: white ≈ the hell out of: beat ≈ ... or make: effort ≈ do: favor ≈ have: doubts ≈ be in: love ≈ offer: apologies ≈ take: trip ...* 4. A fragment of an ECD of Modern Russian is being published by Wiener Slavistischer Almanach (Vienna, Austria; 1984). An ECD of Modern French is being developed at the Univ. of Montreal (Mel'čuk *et al.* 1981); the beginning of a sample entry (about 1/3 of the entry) is reproduced below as an illustration.

ADMIRATION, noun, fem.

1. no plural. *Admiration de X devant Y pour son Z* = Emotional attitude of X, positive with respect to Y, which is caused by the following fact: X believes that actions, state, or properties Z of Y are completely extraordinary; this attitude is one which a normal human being has in such situations.

Government

1=X	2=Y	3=Z
1. <i>de</i>	1. <i>de</i>	1. <i>pour</i> A _{poss} N
2. A _{poss}	2. <i>pour</i>	
3. A	3. <i>devant</i>	
	4. <i>envers</i>	

- 1) C_{2.4} : N denotes a person
 2) C₃ without C₂
 3) C₁+C_{4.1}
 4) C_{2.2}+C_{3.1} } : impossible

l'admiration du public, son admiration, l'admiration nationale; son admiration des (pour/devant les) tableaux anciens; l'admiration de Pierre devant (envers) Jacques pour son courage
 Impossible: **l'admiration envers ces tableaux* (1) [=l'admiration pour ces tableaux]; **l'admiration de Pierre pour le courage* (2) [=l'admiration de Pierre envers Jacques pour son courage]; **l'admiration de Pierre de son père* (3) [=l'admiration de Pierre pour son père]; **admiration pour son père pour son courage* (4) [=l'admiration envers son père pour son courage]

Lexical functions

Syn ₀	:	enthousiasme 1, ravissement, émerveillement
Anti ₀	:	aversion
Gener	:	sentiment [d'~] C ₁ =A
V ₀	:	admirer
S ₁	:	admirateur; admiratrice
S ₂	:	source, objet [de Ø / ART~] // admiration 2 [Il devint l'admiration de la superbe Ninive]
A ₁	:	plein, rempli [de Ø / ART~], dans [ART~], en [~] // admiratif 1 [Elle est admirative pour tout ce qu'il dit]
Able ₁	:	sujet, enclin, porté [à ART~]
Able ₂	:	digne [de Ø / ART~] //admirable
Adv ₁ Able ₂	:	// admirablement [Une rivière aux eaux admirablement claires]
PredAble ₂	:	mériter [ART / de ART~]
Magn	:	grande, vive, profonde<immense<sans bornes
PredMagn	:	ne plus connaître de bornes
AntiVerMagn	:	excessive, démesurée, exagérée
Magn _{quant}	:	commune<générale, universelle
IncepPredPlus ^{refl}	:	grandir, s'accroître, augmenter, se développer
CausPredPlus ^{refl}	:	augmenter, accroître [ART~]
IncepPredMinus ^{refl}	:	diminuer, faiblir
CausPredMinus ^{refl}	:	diminuer, affaiblir [ART~]

References

All the relevant references can be found in: Mel'čuk, I. A. 1982. "Lexical functions in lexicographic description". *Proceedings of the IXth Annual Meeting of the Berkeley Linguistic Society*, Berkeley: UCB, 427-444.

Acknowledgments

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On a Possible Extension of the Scope Theory

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1. Referential Transparency in Opaque Contexts

The "specific-nonspecific" ambiguity of an indefinite NP in opaque contexts can be explained by the scope theory of quantification, as shown by Fodor (1970). Consider the example (1):

(1) Ed hopes that Mary believes that *a friend of mine* is a busdriver.

The three readings of the example (1) can be represented by logical forms (2a, b, c):

- (2) a. Ed hopes that Mary believes that $(\exists x, x \text{ a friend of mine}) (x \text{ is a busdriver})$
- b. Ed hopes that $(\exists x, x \text{ a friend of mine})$ (Mary believes that $x \text{ is a busdriver}$).
- c. $(\exists x, x \text{ a friend of mine})$ (Ed hopes that Mary believes that $x \text{ is a busdriver}$)

(2a) represents the following reading: "Ed hopes Mary believes that there is a friend of mine who is a busdriver"; namely, no one has any particular friend in mind. (2b) represents the reading: "Ed hopes there is some friend of mine such that Mary believes that he is a busdriver"; namely, only Mary has a particular friend in mind. And (2c) represents the reading: "there is a friend of mine such that Ed hopes that Mary believes that he is a busdriver"; namely, both Ed and Mary have a particular friend in mind. The sentence (1) does not have the fourth interpretation that there is a friend of mine of whom Ed hopes that Mary believes that some friend or other of mine is a busdriver, where Ed alone has a particular friend in mind. Fodor (1970: 33) notes that if a noun phrase is interpreted as transparent with respect to a given clause, then it must also be interpreted as transparent, on that reading of the sentence, with respect to all lower clauses.

A sentence-grammar rule like May's (1977) "QR" optionally preposes a quantifier expression to clause-initial position, deriving only (2a, b, c) from (1) and thus excluding the impossible fourth reading on a principled basis.

2. Extension of a Scope Theory to Transparent Contexts

A somewhat similar ambiguity is observable in transparent contexts. Consider the example (3):

(3) *Someone* is lying.

On one reading, the speaker has a certain person in mind, and on the other reading, he has no particular person in mind.

However, what is involved in (3) is not the ambiguity in semantic reading *per se* but the ambiguity in use. That is, it is not the case that the sentence (3) has two distinct readings representable by two distinct logical forms associated with distinct truth conditions. There is no situation which could distinguish two readings by making one true and the other false, with respect to (3). On the other hand, (1) does have such three readings.

To accommodate this type of ambiguity in transparent contexts, we propose to graft a pragmatic analysis onto sentence grammar. Our pragmatic account consists of two rules: Force Indication or FI (obl.) and Quantifier Preposing or QP (opt.). FI prefixes to a logical form like (2a, b, c) some appropriate force-indicator, which specifies the kind of illocutionary force carried by the sentence in actual use. This rule is needed independently to specify the use of a sentence in the context. Some of the illocutionary forces are listed in (4) below.

- (4) a. "┐" for assertion, i.e., "I hereby assert"
 b. "?" for question, i.e., "I wonder"
 c. "!" for command, i.e., "I hereby order" (Dummett, 1973)

QP, the pragmatic version of our QR-like rule, optionally preposes a quantifier over a force-indicator (and the intervening elements, if any). This mechanism is near minimum.

Consider the example (5).

- (5) John believes that *someone* is lying.

The QR-like rule applies to (5), preposing the quantifier to the initial position of the embedded sentence and of the matrix sentence, and deriving (6a) and (6b), respectively.

- (6) a. John believes that ($\exists x$, x a person) (x is lying)
 b. ($\exists x$, x a person) (John believes that x is lying)

Then pragmatic grammar takes over. FI applies to (6a, b) and derives logico-pragmatic forms (7a, b), respectively.

- (7) a. \vdash (John believes that ($\exists x$, x a person) (x is lying))
 b. \vdash (($\exists x$, x a person) (John believes that x is lying))

Next QP may apply to (7a, b), deriving the identical form (8).

- (8) ($\exists x$, x a person) \vdash (John believes that x is lying)

(7a), (7b) and (8) are all the forms that can be derived for (5) under our analysis. And they indeed represent all the possible interpretations of (5) in relevant respects. (7a) indicates that neither speaker nor John has any specific person in mind, (7b) that John does but not the speaker, and (8) that both speaker and John do. The sentence (5) has two readings, namely the opaque

reading represented by (7a) and the transparent reading. The latter has two uses: the opaque use represented by (7b) and the transparent use represented by (8).

Our semantico-pragmatic analysis allows for drawing a natural distinction between differences of meaning and differences of use. Note that a force-indicator indicates the illocutionary force of a particular use of a sentence. Thus the opacity with respect to a force-indicator relates to differences of use, while the opacity with respect to a higher (non-performative) clause relates to semantic ambiguity. Only the latter opacity affects truth value.

This extended scope analysis generalizes to cover universal quantifier expressions and definite descriptions. Take definite descriptions for example.

- (9) Mary thinks John wants to talk to *the boy who failed the exam*.
(Fodor, 1970)

The italicized definite NP exhibits a three-way *semantic* ambiguity with respect to opacity as predicted by our analysis.

Suppose we posit "Rx" as the referentiality operator and generalize our QR-like rule to apply to Rx. This rule followed by FI gives (10a, b, c) from (9), and QP, generalized to apply to Rx, derives the identical form (11) from (10a, b, c).

- (10) a. \vdash (Mary thinks (John wants ((Rx, x the boy who failed the exam) (John talk to x))))
b. \vdash (Mary thinks ((Rx, x the boy who failed the exam) (John wants (John talk to x))))
c. \vdash ((Rx, x the boy who failed the exam) (Mary thinks (John wants (John talk to x))))
(11) (Rx, x the boy who failed the exam) \vdash (Mary thinks (John wants (John talk to x)))

For the sake of space, suffice it to say that this analysis accounts for similar ambiguities in interrogative and imperative sentences in principle. Further, negative sentences are also amenable to our analysis, though they introduce some complications.

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Some Peculiar Uses of the Particle *no* in Japanese

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The examples in (1) exhibit the well-known alternation between the particles *ga* and *no* in Japanese.

- (1) a. Taroo *ga/no* kita koto
Taro come thing "that Taro came"
b. minato *ga/no* mieru oka
harbor is visible hill
"a hill from where the harbor can be seen"

This phenomenon is treated in Harada (1971) and Shibatani (1975). Shibatani's treatment is more successful in that it introduces a perceptual rule and an output condition in place of the suspicious grammatical rules proposed by Harada. But Shibatani's approach is different from mine in some essential respects, as I shall mention later.

What is peculiar is that there are examples such as (2), (3), and (4), which seem to point to the existence of *o/no* conversion in addition to the *ga/no* conversion mentioned above.

- (2) kippu *o/no* katte nai kata wa, syasyoo made o-mooside kudasai
ticket buy not person conductor to tell (honorific imperative)
"Those of you who have not yet bought tickets will kindly tell the conductor."
(3) kippu *o/(?)no* o-kaimotome de nai kata wa, syasyoo made o-mooside
buy (honorific)
kudasai
((3) means about the same as (2).)
(4) kippu *o/(?)no* o-moti de nai kata wa, syasyoo made o-mooside kudasai
have (honorific)

"Those of you who don't have tickets will kindly tell the conductor."

In rapid, colloquial speech, the stem *i* of the durative aspect indicator *iru* is often dropped. Thus (5b) derives from (5a).

- (5) a. kippu *o/*no* motte i nai kata
have DURATIVE not person
"a person who does not have a ticket"
b. kippu *o/*no* motte nai kata

The first half of (2) is apparently similar to (5b) and seems to be derived from (2'), just as (5b) is derived from (5a).

- (2') kippu *o/*no* katte i nai kata

These are, however, quite different in that in (5a) and (5b) the particle

to be used must be the object-marking *o*, whereas (2), though not (2'), allows *no* beside the object-marking *o*.

This is due to the fact that the relative clause with *no* in (2), i.e., *kippu no katte nai*, does **not** come from the deletion of the *i* of the relative clause of (2') but from (6) through *ga/no* conversion.

(6) *kippu ga katte nai*

(6) is the negative counterpart of (7), in which *aru* is the resultative or existential aspect indicator.

(7) *kippu ga katte aru*

RESULTATIVE/EXISTENTIAL

"A ticket has been bought." (Literally, "There is a ticket bought.") Most transitive verbs of Japanese, that is, most Japanese verbs which take NP's marked with *o* as objects can be put in this existential construction, and *motu* 'have' is one of the very few transitive verbs which cannot, because this verb itself implies "resultant existence." Thus it is impossible to say (8).

(8) a. **kippu ga motte aru*

b. **kippu ga motte nai*

Thus (5b) with *no* is quite impossible.

Ga/no conversion is an optional rule, but corresponding NP-*ga* and NP-*no* in a dependent clause often produce a different effect, perceptually. This is accounted for by the perceptual rules (9) and (10).

(9) a. $X \text{ NP-}ga \text{ Y} \rightarrow X \text{ [NP-}ga \text{ Y}$

S

b. $V\text{-Tn } X \rightarrow V\text{-Tn}] X$

S

(10) a. $X \text{ NP-no } Y \rightarrow X \text{ [NP-no } Y$

NP

b. $\text{NP-no } X \text{ N(-Particle) } Y \rightarrow \text{NP-no } X \text{ N(-Particle)] } Y$

[head noun]

[head noun] NP

Note: X may be null.

The rules in (9), which are from Shibatani, are for indicating sentence boundaries. The bracketing they indicate also points to the perceptual "independence" of a clause from the head noun, if any, which it modifies. The rules in (10), on the other hand, suggest the close linkage between the NP-*no* and the head noun and consequently the lower degree of independence of the modifying clause starting with NP-*no* from its head noun. The rules in (10) take care of both the readings of a sentence like (11). The two readings are given through the English translations (a) and (b), the (a) reading having to do with *ga/no* conversion and the (b) reading indicating normal possessive relation.

(11) *watasi no yoku tokau zisyo*

I

often use dictionary

(a) The dictionary I often use

(b) My dictionary, which I often use

Shibatani's perceptual rule on NP-*no*-NP unlike the above rule (10b), which contains the term X that may be null, covers the possessive relation only and cannot explain the close-linkage problem I have mentioned. I also think his Output Condition on NP-*no*-NP, whose details I cannot discuss here, could safely be replaced by an informal statement of a "strategy for excluding perceptual ambiguity." The acceptability status of (12) will be quite suggestive in this connection.

- (12) a. *ʔkore ga Taroo no hon o katta mise da*
 this book bought store be
 "This is the store where Taro bought a book."
 b. *Taroo no hon o yonde iru no o mita koto ga nai*
 read DURATIVE that saw thing not
 "I never saw Taro reading a book."

Taroo no hon in (a) is usually bracketed as NP because of its semantic plausibility and it is this anticipation of ambiguity that prevents the speaker of (a) from using *no* instead of *ga*. This sentence with the given reading might be said to be grammatical but unacceptable. In (b), on the other hand, the anticipation of the production of the whole sentence renders the bracketing of *Taroo no hon* as NP quite impossible. Hence the free use of the particle *no* there. Even the unacceptable reading of (a) can be made acceptable by breaking the unity of *Taroo no hon* prosodically or perceptually, that is, by placing a slight pause between *Taroo no* and *hon* and by destressing the former element and stressing the latter contrastively. This kind of thing cannot be handled by Shibatani's output condition. We know from these observations that speech perception and speech production are at least partially interdependent phases of speech behavior.

The function of *no* to establish close linkage is responsible for the fact that *no* tends to be preferred to *ga* in examples like those in (13).

- (13) a. *see no/ga takai hito*
 height tall person "a person who is tall; a tall person"
 b. *iro no/ga kuroi hito*
 color dark person "a person who is dark-complexioned; a dark-complexioned person"
 c. *suzi no/(ʔ)ga tootta giron*
 "an argument that is coherent; a coherent argument"
 d. *ma no/(ʔ)ga nuketa kao*
 "a face that is silly-looking; a silly-looking face"

The relative clause in each of these examples, en bloc, conveys an inherent quality of the person or thing indicated by the head noun and therefore the close linkage between the two parts is almost needed. Hence the preference of *no* over *ga*. This is particularly true of (c) and (d), in which the relative clauses might be said to form idioms that are incapable of word-by-word translation. Sequences like (1b), on the other hand, allow either particle in their relative clauses with equal acceptability, because in them the semantic connection be-

tween the relative clauses and their head nouns is not so close as in the examples of (13).

Let us next consider the at least near acceptability of the use of *no* in (3) and (4). In these sentences, *ga* in place of *no* is utterly impossible; thus the *no* in them cannot be ascribed to *ga/no* conversion. As Bever, Carroll, and Hurtig (1976, 161) say, "we listen ahead as we talk to see if what we are going to say represents what we mean." The use of *no* in (3) and (4), as well as the usability of *Taroo no* in (12), is due to this feature of speech production. To make this clear, let us consider (14), which is a sort of bracketing of (3).

- (14) [[kippu no o-kaimotome] de nai kata] wa, ...

NP₁NP₂

O-kaimotome is an honorific nominalized form of the verb *kaimotomeru* 'buy' and this type of nominalized form requires before it either the subjective NP-*ga* or the objective NP-*o*, or NP-*no*, the NP-*no* being, as it were, a subjective or objective possessive. Consider (15).

- (15) a. o-kyaku-sama ga/no o-tuki (da)
 guest (honorific) arriving (honorific)
 "the arrival of the guests"
 b. o-kyaku-sama o/no o-mukae (ni)
 meeting (honorific)
 "going to meet the guests"

O-tuki and *o-mukae* are derived, respectively, from the intransitive *tuku* 'arrive' and the transitive *mukaeru* 'meet'. Further, as is well known, a stative predicate requires *ga* as the object-marking particle and this sort of *ga* is sensitive to *ga/no* conversion in dependent clauses. Thus we have common examples like (16).

- (16) koohii ga/no {suki na} hito
 {hosi} }
 coffee {like } person "one who {likes } coffee"
 {want }

The *o-kaimotome de nai* of (14) as a whole conveys a resultant state and, like the predicates of (16), accelerates the use of *no*. Thus the speaker of (14) is urged to use *kippu no* by his listening ahead to the NP₁ and NP₂.

These two factors are necessary for the use of *no* in (14) or (3). The same can be said of (4). In the examples of (5), on the other hand, *no* is impossible because they lack the first condition. (17b), which continues (17a) and which contains *o-kaimotome de nai* as in (3), cannot have *no* because that predicate is not stative, that is, the sentence lacks the second condition.

- (17) a. kore wa kippu o kau tame no retu desu
 this ticket buy for line be
 "This is the line of those who are buying tickets."
 b. kippu o/*no o-kaimotome de nai kata wa kono naka ni haira nai

de kudasai

this inside to enter not

HONORIFIC IMPERATIVE

"Those who are not intending to buy tickets will please keep away from this."

(3) and (4) are analogical neologisms that are ungrammatical because their production is mismatched with the application of the grammatical rule *ga/no* conversion. They are, however, quite utterable though not entirely acceptable.

The conclusions reached in this paper are as follows. (a) We can do without *o/no* conversion despite apparent *o/no* alternations. (b) Many of the phenomena treated in this paper serve as data supporting the "interactionist view," i.e., the view that there are ways of interaction between grammar and the systems of speech perception and production (extragrammatical systems) which determine the acceptability status of given sentences.

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Syntax and Semantics: on the Search of Constants in Verbal Polysemy

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0.1. The following reflexions on verbal polysemy are part of a larger study on the relations between syntax and lexicon in the domain of the French verb. A systematic description of the syntactic properties of a great number of verbs reveals, for a given language, the existence of a lexical structure of homogeneous classes, sharing common syntactic and semantic characteristics. We list, not exhaustively, for French: the verbs of *existence*, *possession*, *donation*, *transformation*, *association*, *judgment*, *movement* etc. The very existence of these syntactico-lexical classes illustrates how the distribution of syntactic properties on the lexicon is not arbitrary and the fact that roughly the same classes can be discovered across languages, although the boundaries between them and the syntactic properties may differ, may even point towards some universal characterization of language.

Besides the discovery, definition and delimitation of these verb classes, this systematic approach also highlights the important role played by the *antonymic*, the *inverse* and more particularly the *factitive* relation in the structure of the verbal lexicon. These relations operate between verb classes or within one single class and permit the link between related classes to be made explicit.

0.2. Given the principle of lexical economy however, the same lexemes appear in different syntactico-semantic classes and the systematic relation between syntax and semantics can be observed not only at the level of the whole lexicon, but also within the limits of a single lexeme, each meaning of a polysemic verb being—at varying degrees—conditioned by syntactic factors.

In fact, little attention has been paid until now to the way in which this relation operates within the limits of the single lexeme, how the different meanings of a polysemic verb are organized and structured in relation to their syntax and how these polysemic verbs relate to the general structure of the lexicon.

We have concerned ourselves mainly with the following questions: (1) Do polysemic verbs share common syntactic and semantic characteristics? (2) In which lexical classes are polysemic verbs most represented? (3) What transitions are most frequent? (4) What are the modalities of transition? More generally: are there constants to be detected in verbal polysemy and to what extent does the discovery of semantic regularities give a solution to the problem of distinguishing between monosemy, polysemy and homonymy.

Given the very strict time limits, we will only be able to summarize some of the results of our study.

1.1. The first point we want to emphasize is the narrow relation between *polysemy and derivation*. As has already been observed by other linguists (f.i. Apresjan, Mel'čuk), semantic relationships between the meanings of some polysemic words can be similar to the semantic relationships between words within a certain type of word formation. This is particularly true, as far as verbs are concerned, for those meaning transitions resulting from the introduction of an inverse or a factitive relation. The concurrence between a morphological or polysemic solution for expressing those relations can be observed within a given language (*dormir*_(-fact) /v/ *endormir*_(+fact) but *guérir*_(-fact) /v/ *guérir*_(-fact)), but the fact is even more obvious in a comparative approach, some languages having systematic recourse to derivation, while others prefer a polysemic solution (f.i. the inverse relation in Dutch as opposed to French). Semantic derivation cannot be separated from morphological derivation.

1.2. The relation between polysemy and derivation is also evident in another way: polysemic verbs are often morphologically derived verbs, the polysemy resulting either from the concurrence between morphemic and syntactic meaning (*embarquer des marchandises* [*embarquer* = mettre dans une barque] /v/ *embarquer des marchandises dans un wagon* [*embarquer* = mettre]), or from an ambiguity already present in the adjectival or substantival base (*désargenter*: argent [métal] /v/ argent [monnaie]).

The large number of derived polysemic verbs is in apparent contradiction with the general statement that polysemy goes together with high frequency (Guiraud). In fact, as far as verbs are concerned, polysemy appears mainly in the high and low frequency verbs, the intermediate values having a tendency to monosemy.

2. With the relationship between *polysemy and lexical verb classification*, we undoubtedly enter upon the most interesting problem in the domain of verbal polysemy. Given the organisation of the verbal lexicon in semantic classes with precise syntactic properties, in which lexical classes are polysemic verbs best represented? What transitions are most frequent? Are there regularities to be observed, within one language and between languages?

2.1. The first thing to be noted is the existence of syntactic structures which are both semantically compelling and lexically productive. This is the case, in French, for the syntactic properties characteristic for the verb *donner* on the one hand, *dire* on the other. A considerable number of verbs, from whatever class they originate, (movement, activity, association etc.) may figure in this particular construction, adopting at the same time the semantic meaning of the construction. We could almost state that any verb, whatever its basic meaning, can become in French (and in other languages) a "declarative" verb or a "donation" verb, given that it adopts the syntactic properties characteristic for those verb classes:

"donner": *donner* (*avancer, glisser, rendre, abandonner, mesurer...*) *de l'argent à quelqu'un* cf. also: *allonger une gifle à quelqu'un, coller une retenue à quelqu'un, accorder une faveur à quelqu'un* etc.

"dire": *dire* (*glisser, souffler, rapporter, exposer...*) *à quelqu'un qu'il a raison*. Many polysemic verbs can therefore be found in these two lexical classes. Certain transitions are however more regular than others (f.i. movement—donation).

It should be noticed that the use of a subjunctive in the declarative verb construction involves the addition of a sense of "command". This syntactic property consequently establishes a systematic polysemic relationship for all declarative verbs. A real chain of transitions can thus be described.

The comment clause construction constitutes another example of a highly productive syntactic structure with precise semantic meaning (*dire* ← *faire, éclater, exploser, imiter* etc.). This seems to be a constant in many languages, with varying degrees of productivity (it seems f.i. less productive in Spanish than in French).

This highly productive transitions are not very significant lexically, the syntax being so compelling that the lexicon does not really matter. In order to guarantee the economy of the lexical description, we propose to treat those cases of purely syntactic polysemy in the grammar rather than in the lexicon: they could be described as "effects of syntax" rather than as real polysemy.

2.2. Other transitions are less productive, but lexically more significant and cross-linguistically more interesting. Such transitions present a certain regularity, without having the very general character of the first category. The following transitions are particularly frequent in French, but also cross-linguistically: possession and existence, opinion and volition, possession and necessity, perception and speech, reflexion and intention. It should be noted that there is no "natural" relationship between the two syntactic structures involved, as is the case with polysemy due to the introduction of an inverse or factitive relation.

It seems essential for semantic description to provide an exhaustive inventory of these regularities in verbal polysemy. They seem to make explicit a vision of the world, some fundamental relationships which are established throughout the lexicon. They should be submitted to a more exhaustive comparison between languages—a preliminary study points towards the existence of constants between languages as different as Greek, French, Dutch, Finnish—and might be explained by psycholinguistic or sociolinguistic approaches.

Some general trends can be detected: the movement from the concrete to the abstract, from the specific to the general (as has been stated also for nominal polysemy, cf. P. Imbs).

2.3. There are lastly, other transitions, which occur for only one lexeme and which seem more "accidental" (f.i. Fr. *différer*: "to be different" and "to put off"). Such transitions lead us to the problem of the distinction between

homonymy and polysemy.

3. Apart from the establishment of a typology of polysemic regularities, our research has also concerned itself with the study of the *modalities of transition*. If, in some cases there seems to be a pure superposition of meaning—the new meaning of the verbal construction is impressed upon the basic lexical meaning (f.i. *glisser*, *abandonner* in a “donation” construction)—in other cases, polysemy is carried by the identity of either one or a small number of common semes. In yet other cases, transition occurs without relevant common semes. The relative autonomy of lexical meanings seems to be related to the global frequency of the verb: cases of superposition of meaning are mostly observed with specialised, often derived verbs, whereas meaning autonomy seems a characteristic of high frequency verbs.

It is mainly on this criterion of the presence or absence and the number of common semes that the distinction between polysemy and monosemy on the one hand, polysemy and homonymy on the other hand, has been based. This criterion, which relevance depends mostly on the definition of the notion “common seme” and which ultimately presupposes a hierarchical analysis of semes, leads more to a continuum of differences than to a discrete opposition. Other criteria should be added. In particular, the regularity of the transition should be taken into account: this criterion, based on the relationship between the general structure of the vocabulary and the structure of the meanings of one word, has the advantage of linking these two levels of analysis. In this perspective, polysemic transitions would represent an intermediate value, while monosemic transitions would be characterized by considerable productivity, and homonymic transitions by their irregular occurrence.

A third criterion, based on a logical characterization of the transitions—as proposed by R. Martin for French substantival polysemy—should also be considered. In cases of homonymy, the semantic differences would not be logically predictable, while polysemic meanings would proceed “naturally” one from the other, with the help of certain general rules.

A study which would take into account the regularity of the transitions, their semic modalities as well as their logical characteristics, might lead to a scalar classification of meaning transitions, avoiding on the one hand a fruitless and counterintuitive atomization of meaning by multiplying homonymic analysis (as is done in many syntactic studies), and on the other hand an uneconomical multiplication of polysemic solutions (as is apparent in many dictionaries). The limits will however remain fuzzy and arbitrary as the unicity or plurality of meaning is not a language “fact” but must be established by and through semantic theory.

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The Interaction of *Like*-expressions and Negation

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This paper will show that *like*-expressions and negation interact in extremely interesting ways, for almost all languages. The data are concerned with sentences where the main clause contains a negative element and the comparison clause contains a comparison element such as *like*, *unlike*, *as*, or the like. For example,

- (1) It is *not* windy in Seoul, *as* in Chicago.
(2) a. It is not windy in Seoul. It is not windy in Chicago.
 b. It is not windy in Seoul. It is windy in Chicago.

Like-constructions such as (1) possess potential ambiguity between the reading (2a) and the reading (2b). I refer to the former reading as the 'like-reading' and the latter as the 'unlike-reading'. The like-reading means that both clauses are negated, and the parallel semantic content is shared. The unlike-reading means that the main clause alone is negated, and the parallel semantic content is not shared. I refer to this and related matters as the 'like-unlike-phenomena'; accordingly, the ambiguity as the 'like-unlike-ambiguity'.

The like-unlike-ambiguity is sensitive to the word order between the negative element of the main clause and the *like*-expression of the comparison clause. In languages like English, for example, the *not-like* order (cf. 1) entails the ambiguity, whereas the *like-not* order (cf. 3) does not; it has the like-reading alone.

- (3) *As* in Chicago, it is *not* windy in Seoul.

Interestingly enough, the disambiguation device is not limited only to the word order. The construction under consideration is subject to various kinds of disambiguation devices such as word order, pause, intonation, syntactic distance, and the like. For example, if we put some pause between the two clauses of sentence (1), then the like-reading alone is available. With the normal intonation in sentence (1), it is potentially ambiguous; however, if we have the intonation break at the end of the first clause, the like-reading alone is available. The syntactic distance between the negative element of the main clause and the *like*-expression of the comparison clause has the same effect. Suppose we have a relative clause or some other syntactic materials between the two clauses. In this case, only the like-reading is available.

The disambiguation devices referred to in the above seem to have something in common. That is, this category of disambiguation devices can be characterized by the concept of psychological adjacency relation between the negative element and the comparison element. If the comparison element is

adjacent to the negative element, the ambiguity results; otherwise, only the like-reading results.

In addition to the above category, we have another category of disambiguation devices such as the hearer's (or writer's) background knowledge, contextual information, semantic factors, and the like. In sentence (1) for example, for those who are aware of the fact that Chicago is windy, that sentence is not ambiguous: it has only the unlike-reading. In contrast, for those who are not aware of this fact, the sentence is potentially ambiguous: Chicago may or may not be windy. The same effect may result if the contextual information is sufficient. Consider the following example:

- (4) a. It is notorious that Chicago is windy.
- b. But it is not windy in Seoul, as in Chicago.

Sentence (4a) provides a contextual information for the potentially ambiguous sentence (4b). Sentence (4b) is disambiguated, due to the contextual information (4a).

In connection with the disambiguation, semantic factors play a very significant role. For example,

- (5) a. Seoul is not windy, unlike Chicago.
- b. Unlike Chicago, Seoul is not windy.

Regardless of the word order, pause, intonation, and syntactic distance, sentences in (5) are not ambiguous: the unlike-reading alone is available. This is due to the semantic factor of the comparison element, *unlike*.

What is more interesting is the disambiguation device which is connected with the semantic nature of verbs, adjectives, and adverbs of the comparison clause. There is a class of words (i.e., verbs, adjectives, adverbs) which express the speaker's (or writer's) strong affirmation on a proposition, so that propositions with such words may not be negatively reversible by the force of the negative element in the main clause. For example,

- (6) a. Grammatical relations can not be satisfactorily defined in the framework of structural approach, as *attested* in X's dissertation.
- b. ----, as is *evident* in X's dissertation.
- c. ----, as *convincingly* argued in X's dissertation.

Note that the comparison clause in (6a) contains the verb *attest*, which belongs to the verbs of strong affirmation (e.g., *testify*, *prove*, *certify*, *confirm*, *convince*, *evidence*, *ascertain*, etc.), and that (6a) is not ambiguous; it has the like-reading alone. The same thing can be said for sentences like (6b) where the adjective *evident* is a word of strong affirmation, and for sentences like (6c) where the adverb *convincingly* behaves in the same way. Such adverbs include: *convincingly*, *clearly*, *evidently*, *correctly*, *successfully*, *obviously*, etc.

We have observed that certain words disambiguate the like-unlike-ambiguity. These words express the speaker's (or writer's) strong affirmation. Now I would like to indicate just one constraint (among others) to this generalization. The conceptualization of strong-affirmation words has two different generative sources. One source is the case where the speaker directly conceptualizes such

words in his mind (=the speaker-source). The other source is the case where the conceptualization is secondarily formed through somebody else's initial conceptualization (=the secondary source). In the case of indirectly obtained conceptualization, the comparison clause in the negative context is still ambiguous. For example, suppose that the comparison clause in (6a) has such a form as: "*as attested by John.*" In this case, the verb *attest* is used not initially by the speaker (or writer) but secondarily by adopting (or more precisely, by quoting) the original utterer's conceptualization of the verb, that is, John's initial conceptualization. For the original speaker, the verb *attest* expresses the strong affirmation; but for the quoter, the verb *attest* may or may not be adopted as the strong affirmation. One man's strong affirmation may or may not be another's strong affirmation.

The second category of disambiguation devices (i.e., hearer's background knowledge, contextual information, semantic factors, etc.) can be characterized by the fact that the semantic content of the comparison clause is determined unambiguously one way or another, so that the interpretation of the comparison clause becomes immune to the effect of the negative force of the main clause.

It should be noted that individual languages may have their own specific disambiguation devices (cf. Yang 1980). The Korean delimiter *nin* (which is usually called the topic marker) and the corresponding Japanese *wa* are cases in point. When these elements are attached to the comparison element, the unlike-reading alone is available. In English, the '*not-so-as*' construction entails the unlike-reading alone. And also some typological patterns emerge with respect to the like-unlike-phenomena. For just one example, Korean and Japanese are different from English and the related languages, in that the 'like-not' order, but not the 'not-like' order, is subject to the potential ambiguity in the former languages, whereas the 'not-like' order, but not the 'like-not' order, is subject to the potential ambiguity in the latter languages (cf. Yang 1980).

With respect to the explanation of the like-unlike-phenomena, we may evoke the scope ambiguity mechanism. According to the (relative) scope theory, the negative element of the main clause may or may not extend its scope to the comparison clause; thus, the like-unlike-ambiguity results. However, the traditional scope theory, which is a structural approach (thus, the structural scope theory), cannot explain the disambiguation devices. Consider the first category of disambiguation devices (word order, pause, intonation, syntactic distance). This category rejects the influence of the negative scope of the main clause. Such scope-immune phenomena cannot be explained within the framework of the structural scope theory, without positing ad hoc exceptions. The same can be said of the second category of disambiguation devices, which are also always outside the negative scope. If we introduce the notion of the functional scope theory, the ad hoc exception treatment under the structural scope theory can be naturally explained. Furthermore, the two categories of disambiguation devices can be unified under the functional scope theory.

What is also interesting is the fact that the like-unlike-phenomena are near-

universal. In this regard, Chinese and Vietnamese seem to be exceptional, as far as I have examined. The two languages do not allow like-unlike-ambiguous structures; they structurally disambiguate the potentially ambiguous construction. It will be interesting to know how many other languages behave like Chinese and Vietnamese.

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Semantic Restrictions on Certain Complementizers

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Any researcher interested in the relation between linguistic form and its semantic content will find quite natural the question of whether or not the absence of some particular linguistic form can be explained on semantic grounds. In this note I will show that the impossibility of a *wh*-complementizer after *neg*-transportation verbs has semantic justification.

Formally, one defines *neg*-transportation verbs as verbs taking two arguments, a nominal one and a sentential one, such that negation of the verb itself can be "transported" to the sentential argument without important semantic change. More precisely a *neg*-transportation verb forms, with its subject NP, a sentential operator O such that "not-O(P)" implies "O(not-P)" (where 'P' is the sentential argument of the *neg*-transportation verb).

Thus, in the following examples, (1) implies (2) and (3) implies (4):

- (1) Bill does not believe that Sue will call
- (2) Bill believes that Sue will not call
- (3) It does not seem that it will rain
- (4) It seems that it will not rain

There exists a well-known objection to the claim that (1) implies (2). According to this objection (1) cannot imply (2) since there can be situations, when Bill does not exist or when Bill does not know Sue, in which (1) is true and (2) is false. This objection raises the problem of specifying the kind of the negation used in (1) and (3). Without entering into details I can affirm that this objection can be avoided if we consider that the negation in question is *opacity preserving*. *Neg*-transportation verbs are propositional attitude verbs (or are reducible to them) and as such they are (strongly) opaque or intensional. It means that the replacement of an argument-expression under the scope of the *neg*-transportation verb by another argument-expression which is extensionally equivalent to the replaced one, can provoke the change of the truth value of the whole complex construction. Thus, in the following examples, (5) and (6) can have different truth values even if *Sue* and *John's sister* refer to the same person:

- (5) Bill thinks that Sue will call
- (6) Bill thinks that John's sister will call

In fact verbs of propositional attitude are very strongly opaque; their opacity can be detected, as opposed to that of classical modal operators, by contingent

sentences. More precisely "O" is strongly opaque iff, for every contingent sentence *P* and for every situation *s*, if *O*(*P*) is true at *s*, then there exists a contingent sentence *P'* with the same truth value as *P* at *s* and such that *O*(*P'*) is false at *s*. Neg-transportation verbs cannot take *wh*-complementizers: the following forms are impossible: **to think whether*, **to hope who*, **to believe why*, **to be afraid what*, etc. If these forms were possible they would have semantic properties similar to those with which such complementizers are possible. To obtain these properties we will consider the constructions with *to know whether* and semantic relations into which they enter. It can be noticed first of all that (7) and (8) both imply (9):

- (7) Bill knows that Sue will call
- (8) Bill knows that Sue will not call
- (9) Bill knows whether Sue will call or not

More precisely we have the following property:

- Prop. 1: "O that *P*" implies "O whether *P* or not"
 "O that not-*P*" implies "O whether *P* or not"

The second property of *wh*-constructions we need says that the opacity preserving negation of *that*-construction implies the negation of the corresponding *wh*-construction; the opacity preserving negation of (7) and of (8) implies the opacity preserving negation of (9). For instance (10) implies (11):

- (10) Bill does not know that Sue will call
- (11) Bill does not know whether Sue will call or not

This property can be stated in general in the following way:

- Prop. 2: If "*O*(*P*)" implies "*O'*(*P*)" and "*O*" and "*O'*" are operators of the same opacity strength, then "not-*O*(*P*)" implies "not-*O'*(*P*)"

Two operators have the same opacity strength if any pair of equivalent sentences which can be used to detect the opacity of the first operator can also be used to detect the opacity of the second one. For instance the operator formed from *to regret that* is not of the same strength as the operator formed from *to know that* because the pair (12) and (13) of equivalent sentences does not behave in the same way with respect to those two operators:

- (12) The bottle is half full
- (13) The bottle is half empty

Indeed, only (15a) and (15b) can have different truth values; (14a) and (14b) have the same truth values:

- (14a) Bill knows that the bottle is half full
- (14b) Bill knows that the bottle is half empty
- (15a) Bill regrets that the bottle is half full
- (15b) Bill regrets that the bottle is half empty

Thus the pair (12) and (13) shows the opacity of *to regret* and not the opacity of *to know that* and consequently these two operators are not of the same strength. On the other hand, as one can easily verify, *to know that* and *to know whether* are of the same opacity strength.

Now we can demonstrate the property Prop. 2. Suppose that " $O(P)$ " implies $O'(P)$ but that " $\text{not-}O(P)$ " does not imply " $\text{not-}O'(P)$ ". There then exists a situation s in which " $\text{not-}O(P)$ " is true and " $\text{not-}O'(P)$ " is false. But since *not* is an opacity preserving negation, *not-O* and *not-O'* are new opaque operators of the same strength. Consequently there exists a sentence P' with the same truth value as P at s such that " $\text{not-}O(P')$ " is false and " $\text{not-}O'(P')$ " is true at s . But then, because of the usual semantics of the negation, " $O(P')$ " is true and " $O'(P')$ " is false. But this cannot be possible since " $O(P')$ " implies " $O'(P')$ ". So the property Prop 2 must be true.

The property Prop 2 in conjunction with the definition of the *neg-transportation* verb entails the impossibility of having *wh-complementizers* with these verbs. To show that suppose that " O " is a *neg-transportation* verb. Then, according to the definition of such a verb we have (16):

- (16) " $\text{not-}O$ that P " implies " O that ($\text{not-}P$)"

Furthermore, if we had O *whether* form, then according to the property *Prop 1* (17) would be true:

- (17) " O that $\text{not-}P$ " implies " O whether P or not"

The conjunction of (16) and (17) gives (18):

- (18) " $\text{not-}O$ that P " implies " O whether P or not"

On the other hand the Prop 2 expresses what is given in (19):

- (19) " $\text{not-}O$ that P " implies " $\text{not-}O$ whether P or not"

But (18) and (19) are in contradiction. So our supposition that there exists a " O whether" form, where " O " is a *neg-transportation* verb, leads to a contradiction.

Similar reasoning holds for other *wh-complementizers*. Instead of using the Prop 1 and Prop 2 which holds for *whether-complementizer*, one should generalise these properties using the well-known decomposition of other *wh-*quantifiers (such as *what*, *who*, *where*, etc.)

It seems important to point out in conclusion that what I have shown is not that *whether-complementizers* are absolutely impossible with *neg-transportation* verbs. I have shown that they are impossible only with their "main" meaning, the one given by the definition of *neg-transportation* verb and by the properties *Prop 1* and *Prop 2*. In particular they must be related to the *that-complementizer* in the way shown above. Consequently it may happen that in some languages one can find, in some construction, **to think whether* or its equivalent. My claim is however that in this case the meaning of the verb will be different from the one stipulated by the properties here discussed.

The Role of Word Formation Rules in the Causative Constructions of Non-Configurational Languages

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One of the consequences of the recent classification of human languages into configurational and non-configurational has been an expansion of the role of the lexical component as a means of capturing more of the generalizations for which transformations were used before. For Japanese, for instance, Miyagawa (1980), Farmer (1980) have claimed that causatives are to be viewed as the output of word formation rules (WFRs), as claimed also by Hasegawa (1981), in Bresnan's lexicalist model. Kuno (1980) and (Kuroda (1981) have countered that regular Japanese causatives must be viewed not as lexical but as syntactic.

While Old French differs from Japanese with respect to word structure and to word order, in the sense that OF is not a verb final but a mixed word order language, like Japanese, it is non-configurational. Consequently, in the Government Binding framework, as in Bresnan's lexicalist framework, the formation of Old French causative sentences could not be accounted for transformationally. The study of OF causative sentences provides us with an opportunity to investigate the possible role of the lexicon in a non-configurational language having analytic rather than agglutinative properties.

Examples (1) and (2) show some of the forms of OF causative sentences:

- (1) *Les chevaliers a fait monter.* (Guingamor, 12th-13th C.)
'He had the knights mount.'
- (2) *Deu fist l'imasgene por soue amour parler.* (Alexis, 11th C.)
'God out of his love made the statue speak.'

The causative verb and its dependent infinitive may thus be separated by a variety of phrases. In addition, the two verbs, as can be seen from comparing (1) and (2) with (3) below, may be freely ordered in relation to each other. In Japanese, by contrast, the causative formative (s)ase and the dependent verb stem may not be freely ordered, nor can they be separated by words. Sentence (4) is a typical example:

- (3) *Passer le ferai mer sans nul terme prenant.* ((Gaufrey, 13th C.)
'I will make him cross the sea without any delay.'
- (4) *Taroo ga Hanako o hatarak-ase-ta.*
'Taro made Hanako plow the field.'

For Japanese, the formation of causative repredicates may be accounted for by means of the WFR in (5)

$$(5) \left[\begin{array}{c} [x] \\ +V \\ [(NP)^n \quad _] \\ X \end{array} \right] \longrightarrow \left[\begin{array}{c} [[x](s)ase] \\ +V \\ [(NP)^{n+1} \quad _] \\ CAUSE X \end{array} \right] \quad \text{Where } X \text{ and CAUSE } X \text{ represent} \\ \text{the meaning of the verb stem and} \\ \text{of the related complex predicate,} \\ \text{respectively}$$

One side effect of the rule in (5) is that by specifying that $[V+(s)ase]$ has the property $[+V]$, the WFR predicts what the position of that complex predicate will be in the sentence, so that for Japanese causative sentences, phrase-structure rules of the type proposed in Chomsky (1965) may be said to provide information which is redundant with respect to what is specified by the lexical component.

Interestingly, proponents both of the lexicalist and of the syntactic approach have referred to Chomsky (1970) as one of the criteria upon which their analysis is based. Miyagawa, e.g., pointed to the existence of irregular causative forms and instances of semantic drift, and Kuroda responded that the majority of the $V+(s)ase$ forms are regular and thus should be considered syntactic. Hasegawa's version of Bresnan's model may also be the consequence of the same preoccupation with reducing the power of the transformational component. As Bedell (1974) noted with respect to Chomsky (1970), however, it is not obvious why one should assume that eliminating minor transformations to the benefit of lexical or interpretive rules would result in a less powerful grammar, and the same applies to grammatical relation changing transformations.

Where the major analyses proposed for Japanese generally agree upon, though, and can argue for on the basis of empirical evidence is with respect to the assumption that on the surface $V+(s)ase$ does form a lexical unit.

As we saw, for OF, by contrast, the distributional evidence suggests that *faire* and its dependent infinitive should not be considered a word in the traditional sense. So, while irregular such sequences might be entered in the lexicon in agreement with Chomsky (1970)'s proposal for idiosyncrasies, it is unclear what role WFR's might play in the formation of regular causative sentences, aside from the rules necessary, of course, to form all infinitive as in (6):

$$(6) \begin{array}{l} \text{finir:} \\ \text{to finish'} \end{array} \left[\begin{array}{c} /x/ \\ +V \\ +2nd \text{ Conj} \\ W \end{array} \right] \longrightarrow \left[\begin{array}{c} /x+r/ \\ +V \\ +Inf \\ W' \end{array} \right] \quad \text{Where } W \text{ and } W' \text{ represent the} \\ \text{meaning of the verb stem and} \\ \text{of the corresponding infinitive,} \\ \text{respectively.}$$

Given that it is a mixed, and not a totally free word order language, it may be assumed that in any framework some output constraints will be necessary to filter out unacceptable sequences generated by the phrase-structure rules, for causative and non-causative sentences alike.

More specifically related to causative sentences and pertaining to the domain of the lexicon will be the entries necessary for the causative verbs and for those prepositions found in causative sentences. Thus, the OF lexicon will have to include the following specifications:

- (7) *faire*: $\left[\begin{array}{l} +V \\ +\text{Inf Complement} \\ (+\text{RAISING}) \\ \text{FAIRE} \end{array} \right]$ Where FAIRE represents the meaning of the causative verb.
- (8) a_1 : $\left[\begin{array}{l} +\text{Prep} \\ + \left[\begin{array}{l} \text{NP} \\ \text{IO} \end{array} \right] \end{array} \right]$ a_2 : $\left[\begin{array}{l} +\text{Prep} \\ + \left[\begin{array}{l} \text{NP} \\ \text{Ob} \end{array} \right] \end{array} \right]$ par : $\left[\begin{array}{l} +\text{Prep} \\ + \left[\begin{array}{l} \text{NP} \\ \text{Obl} \end{array} \right] \end{array} \right]$

This should ensure that the prepositions will be occurring only with the appropriate grammatical relations. Nothing in all this information which we may assume must be provided by the lexicon allows us, however to predict the form of the causee for a particular syntactic class of verbs, as was achieved in transformational analyses for Modern French. In Bailard (1982), I have argued that the form of French causative sentences may to a large extent be predicted, as suggested by e.g. Comrie (1976), by Keenan and Comrie's Relational Accessibility Hierarchy. Thus, this is the case here for sentences (1) and (2), which show the causee as a direct object. Sentence (3), with the accusative pronoun on the other hand constitutes a marked case for which a language-specific rule must be appealed to.

The point here is, that while there is clearly a role, albeit a reduced one in comparison with Japanese, for lexical rules in the description of OF causative sentences, there exist other important generalizations which must be captured, and which pertain to the interaction of words within sentences, which has traditionally been taken to be the role of the syntactic component of the grammar.

Thus, assuming that all sentences are produced by phrase-structure rules, one must ask how the language user arrives at the generalization embodied in the RAH. One possible answer is that he does so on the basis of the oppositions which he observes between related causative and non-causative sentences. For OF, these may be described by means of the following syntactic redundancy rules:

(9) FAIRE+Infinitive Redundancy Rule I

$$\left[\begin{array}{l} x, \\ S \left[\begin{array}{l} \text{NP} \\ -\text{Pron} \end{array} \right] \left[\begin{array}{l} y \\ \text{NP} \\ \text{DO} \end{array} \right], V \end{array} \right] \Rightarrow \left[\begin{array}{l} w, \langle a \rangle -x \\ S \left[\begin{array}{l} \text{NP} \\ \text{Su} \end{array} \right] \left[\begin{array}{l} \text{NP} \\ -\text{Pron} \\ \text{DO} \\ \langle \text{IO} \rangle \end{array} \right] \left[\begin{array}{l} \text{NP} \\ \text{NO} \end{array} \right], y, \text{FAIRE}, V' \end{array} \right]$$

Where V and V' represent a finite verb form and the corresponding infinitive, respectively, and FAIRE some verb taking an infinitive complement and allowing Subject to Object Raising

(10) FAIRE+Infinitive Redundancy Rule II

$$\left[\begin{array}{l} x, \\ S \left[\begin{array}{l} \text{NP} \\ +\text{Pron} \end{array} \right] \left[\begin{array}{l} y \\ \text{NP} \\ \text{DO} \end{array} \right], V \end{array} \right] \Rightarrow \left[\begin{array}{l} w, y, -x' \text{FAIRE} -, V' \\ S \left[\begin{array}{l} \text{NP} \\ \text{Su} \end{array} \right] \left[\begin{array}{l} \text{NP} \\ \text{DO} \end{array} \right] \left[\begin{array}{l} \text{NP} \\ +\text{Pron} \\ \text{DO} \\ \text{IO} \end{array} \right] \end{array} \right]$$

(11) FAIRE+Infinitive Redundancy Rule III

$$\left[\begin{array}{c} x \\ S \left[\begin{array}{c} NP \\ +Pron \end{array} \right] \end{array} \right], \left\{ \begin{array}{c} a \quad z \\ \left[\begin{array}{c} NP \\ IO \end{array} \right] \end{array} \right\}, V \Rightarrow \left[\begin{array}{c} w' \\ S \left[\begin{array}{c} NP \\ Su \end{array} \right] \end{array} \right], \left\{ \begin{array}{c} a \quad z \\ \left[\begin{array}{c} NP \\ IO \end{array} \right] \end{array} \right\}, \left[\begin{array}{c} -x' \text{ FAIRE } - \\ \left[\begin{array}{c} NP \\ +Pron \\ IO \end{array} \right] \end{array} \right], V']$$

It is from the comparison of the correspondences shown in such rules that the cross-linguistic generalizations noted by Keesen and Comrie can be abduced by the language user.

We may thus conceive of the RAH as a language universal interpretive rule, or well-formedness condition, as in (12):

(12) INTERPRETIVE RULE I

Unless otherwise specified, in a causative sentence with a [+RAISING] causative operator, interpret an argument as the agent of the dependent predicate IF AND ONLY IF

The form of the other arguments in the sentence is such that the syntactic subcategorization requirements of that dependent predicate are met by them, and the first argument in question is of a form in agreement with the Relational Accessibility Hierarchy and the 'no doubling on relational terms' constraint.

In these condition, marked sentences such as the one in (3) and others given in Bailard (1982) can be accounted for by some additional, OF-specific constraint, as in (13)

(13) FAIRE+Infinitive INTERPRETIVE RULE (Old French)

In a FAIRE+Infinitive sentence, interpret an Indirect pronoun as the Agent of the infinitive if the infinitive is intransitive, pseudo-intransitive, or transit in indirect, and a Direct pronoun if the infinitive is transitive direct and [+Action].

What is important here is that, while OF is a non-configurational language, its typological characteristics with respect to word structure are such that one would seem justified in concluding that for OF causative sentences, WFR's have but a trivial role in capturing the generalizations which transformational Grammar and Relational Grammar endeavoured to describe, so that we may posit that while transformations may not be appropriate as they are currently characterized to do the task, it is nevertheless the case that there exist some facts pertaining to syntax which for OF, as for configurational languages, must be described, and this may be accomplished by means of syntactic redundancy rules.

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The Choice of a Grammar for Standard and Nonstandard German

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System and norm

If the expression "the German language" is to be used in its most usual sense—i.e. to include dialects, sociolects, style etc.—then there is no justification for describing for example dialects as different language systems or for viewing dialectically coloured utterances as interference between systems. The man in the street views these merely as dialect elements or regional phenomena. Scientifically they are merely variants of a single system¹⁾. In my study "Non-standard: syntactic variants in German"²⁾, based on Coseriu's distinction between system and norm³⁾, I attempt to describe the different syntactic features of (codified) standard language, of dialect, of sociolect etc. as different slot-fillers in one identical language system.⁴⁾

Let me give you two illustrations using a postulated dependency model:

1. Certain adverbs (*herein, herauf, hinunter*) can be modified in German by dependent adverbs of place or by accusative noun phrases: i. *Er kommt dort*

1) The term "Sprachvarietät" is generally used in this sense for regional and "technical" languages. Cf. e.g. Mihm: Zur Entstehung neuer Sprachvarietäten. In: Zeitschrift für Germanistische Linguistik, 1982 in print. Here interferences between "varieties" are demonstrated.

2) In this study the following primary sources were used: Lautbibliothek der deutschen Mundarten. Ed. deutsches Spracharchiv. H. 3-35, Göttingen 1958-1964 and 9 monographs in the series: Phonai. Lautbibliothek der europäischen Sprachen und Mundarten. Deutsche Reihe, Tübingen 1970-1981. Secondary sources were all available syntactic descriptions of German dialects. "Standard language" is understood as the so-called "Dudennorm", i.e. corresponds to the information provided by: Duden—Grammatik der deutschen Gegenwartssprache. Mannheim 1973 and Duden—Das große Wörterbuch der deutschen Sprache. Mannheim 1976-1981. The project was supported by the Deutsche Forschungsgemeinschaft (German Science Foundation) under Grant HE 1244/1-2.

3) E. Coseriu: System, Norm und Rede. In: E. Coseriu: Sprachtheorie. München 1973, S. 11-101.

4) The tendency, documented inter alia by H. Eggers: Deutsche Sprachgeschichte IV. Reinbeck 1977, to adopt colloquial elements in standard text types is therefore merely a change in the norm. However Eggers fails to see the whole phenomenon when he assumes (like many other authors dealing with the spoken language) that these changes are predominantly statistic. Recent recommendations that German language teaching in schools should accept regional colloquialisms which diverge from the codified norm (cf. H. Löffler: Dialekt und Standardsprache in der Schule. In: Lehren und Lernen 8, 1982, 3-13) also aim at changing the norm. However up to now there has been no satisfactory analysis of regional language, sociolect etc. within Coserius theoretical framework.

herein/die Treppe herauf, ii. *er geht die Treppe hinunter* etc. In almost all German dialects (but not in Standard German) the analogous sentence iii. *er kommt die Tür herein* is possible. This suggests the following interpretation: A rule "Adverbs of this type can be modified by adverbs of place or by accusative noun phrases" is part of the system.⁵⁾ The extent to which its use is limited is a question of the norm.

2. A syntax system of German contains the possibility: noun plus dependent noun in the genitive (iv. *Müllers Garten*) and noun plus dependent prepositional phrase (v. *Vater von fünf Kindern*). This pattern is found in all varieties. However if we look at the use of the system we see that in the standard variety vi. *der Garten von seinem Vater* is not available while in the dialects vii. *seines Vaters Garten* is not possible. The fact that vii. is a variant means that this expression is found in at least one variety but not in all, i.e. vii. belongs to the system but not to every norm.⁶⁾ If Standard and Nonstandard⁷⁾ are described as varieties, problems arise where particular patterns/features are completely excluded by a specific norm: one is tempted, for instance, to speak of a 2-case "system" as against a 4-case "system" or to view the extension to the possessive article *sein/ihr* (viii. *dem Kind sein Spielzeug*), possible only in the Nonstandard, as a difference in the system.

The question is therefore whether arguments can be found to justify a distinction on the syntax level between norm rules and system rules. Is such a distinction dependent on the choice of a specific syntax model?

Syntax models and the possibilities of distinguishing between system and norm

The majority of grammars⁸⁾ turn out to be descriptions of the system—insofar as they are sufficiently detailed and consistent to be of use. In other words the rules are formulated so broadly that divergencies between Standard and Nonstandard such as those found with the accusative adverbial phrases are not covered. Rules for patterns which appear only in the Nonstandard are missing. The same grammars describe in other sections regularities of the norm, for instance they list nouns and the appropriate plural morphemes⁹⁾ (here there is great variation) instead of the only relevant system opposition singular vs.

5) Assuming for the moment that a satisfactory syntactical description is available.

6) Coseriu shows op. cit. pp. 65 ff. that this distinction is equivalent to that made in phonology between broad and narrow transcription or between phoneme and allophone.

7) In the following I shall only state whether an expression belongs to the codified norm of the standard language or to a nonstandard norm, however not whether the nonstandard norm is regional, social or stylistic etc., particularly since they often overlap. (In the project described in Note 2 only the regional source of the quotations is noted).

8) e.g. U. Engel: *Syntax der deutschen Gegenwartssprache*. Berlin 1977; G. Helbig/J. Buscha: *Deutsche Grammatik. Ein Handbuch für den Ausländerunterricht*. Leipzig, 1979; J. Kunze: *Abhängigkeitsgrammatik*. Berlin 1975. (*Studia grammatica* XI); H.-J. Heringer: *Deutsche Syntax*. Berlin 1972.

9) Helbig/Buscha, op. cit.; Heringer, op. cit. For a grammar of German as a foreign language a thorough description of the norm is highly desirable.

plural.

The most detailed rules for the Standard language are given by the Duden Grammar.¹⁰⁾ In a similar fashion the few dialect syntax descriptions available¹¹⁾ often list large numbers of disparate facts. However, here the distinction between system and norm is of little help since these grammars offer only thematically ordered contrasts, illustrated by many examples, and ad-hoc descriptions. Kufner's attempt to produce a transformational grammar of the Munich dialect¹²⁾ thus results merely in a description of the system: all the rules—at least in the syntax section—are so broad that they fit the standard variety, while additional patterns such as viii. are not covered.

If a consistent (structural) syntax model were available then it would be sensible to ask which rules belong to the norm and which to the system, or which additional regularities must be formulated for the norm. The answer to this question thus becomes an empirical matter which can only be solved by the comparison of different subnorms.

Two further illustrations: categorial grammars differentiate between subsetting modifiers with nouns: ix. *der blonde Liebhaber* and intensional-context-producing modifiers: x. *der mutmaßliche Liebhaber*. In the Nonstandard an intensional-context-producing modifier cannot be used in the pre-noun position. This limitation of the rule concerning modifiers thus belongs to the description of the norm.¹³⁾ In a transformational grammar one would assume a nominalising transformation whose usage is limited for the Nonstandard.

These examples have shown that in virtually every syntax model it is necessary to distinguish between rules describing the system and rules describing the norm.

Syntax models and specific problems in the analysis of the Nonstandard

The specific problems arising in the analysis of the Nonstandard severely limit the choice of syntax models. In categorial grammars it is only possible to formulate divergencies as syntactical norm deviations which are described lexically/semantically in other types of grammars, as has been shown. Nevertheless they offer a descriptive calculus which enabled a mathematical determination of the well-formedness of sentences¹⁴⁾ and this is, however, not so important for the present project for the following reasons:

1. Not only sentences are to be described but also smaller units. Here the calculus of the categorial grammars fails to help since they are based on the assumption of complete predication.

10) See Note 2.

11) J. Schiepek: *Der Satzbau der Egerländer Mundart. Teil I. Prag 1899, Teil II, Prag 1908; W. Hodler: Berndeutsche Syntax. Bern 1969* are with over 600 pages each the most extensive.

12) H. L. Kufner: *Strukturelle Grammatik der Münchner Stadtmundart, München 1961.*

13) With categorial grammar it would probably only be possible to include a description of the norm by adding additional rules.

14) R. V. B. Hill: *Language and Information. Reading, Mass., Jerusalem 1964.*

2. The corpora include only those utterances which are acceptable—either in the Standard or the Nonstandard—as determined by native speakers.¹⁵⁾
3. The corpus is not limited to sentences which correspond to available formal descriptions.

The concept of the well-formed utterance is probably in general too narrow for natural languages, particularly however for noncodified variants, if it is derived from intensional logic.¹⁶⁾

The use of transformational grammar, in whatever version, is also problematic:

1. If units smaller than sentences are to be described these have to be explained by ellipses. This means that unnecessarily broad contexts must be sought, expansions of the ellipses invented.

2. The transformational apparatus needed for descriptions covering norm regularities becomes very extensive¹⁷⁾ (incidentally also extremely confusing for the reader) and also arbitrary.

Furthermore transformational grammars are generally synthetic while research into the Nonstandard is necessarily based on corpora and involves the description of previously unanalysed utterances.

The suitable syntax model

Let me give you some further randomly selected examples: xi. *der Garten da/dort* (Standard and Nonstandard), xii. *da dieser Garten* (Standard), xiii. *da der Garten*, xiv. *der da Garten*, xv. *Peter kommt* (Standard), xvi. *der Peter kommt*, xvii. *so ein Original*, xviii. *ein solches Original* (Standard), xix. *ein so Original*, xx. *ein son Original*, xxi. *verschiedene so Originale*, xxii. *ein so ein Original*, xxiii. *ein ganz anderes Leben*, xxiv. *ein ganz ein anderes Leben*, xxv. *ein ganzes anderes Leben* (Nonstandard), xxvi. *er sagte zu ihm* (Standard), xxvii. *er sagte für ihn* (Nonstandard), xxviii. *dann heim, umgezogen, nach die Nachfeier hin*.

Categories

The most successful grammar model for a consistent description of the structures of the system and the rules of the norm turned out to be a (relatively unmodified) traditional dependency model. Divergencies from the norm are realised not at the level of sentence segments (sentence, subject, object etc.) but inside word group structures,¹⁸⁾ i.e. at the level of words and their dependency

15) See Note 2; the sources referred to contain sufficient information about the "normality" of expression.

16) The problem of adequacy, in particular of the semantic interpretation is extensively discussed by: F. v. Kutschera: *Einführung in die intensionale Semantik*. Berlin, New York 1976.

17) Development and application of individual snippets for purposes of illustration is of little genuine use.

18) Cf. U. Engel *op. cit.*: *Syntax der deutschen Gegenwartssprache*. Berlin 1977, who uses the term "Phrasen": noun and dependent elements compose the noun phrase.

relations.¹⁹⁾ The categories in the grammar are types of words (nouns, adjectives etc.) which are defined syntactically, that is according to possible word combinations they can form. The relations between the elements are dependencies and word order. Dependencies are normally determined according to Kunze's²⁰⁾ new and rather complicated configuration criterion²¹⁾ and by elimination criteria²²⁾ Transformational relationships are not used. A grammar of this type is extremely simple (although this advantage is accompanied by the semantic ambiguity of the dependency relationship).

Diagramming

Diagramming the relations turns out to be equally simple. For this we use the well-known stemmata of Ihm and Lecerf.²³⁾ In contrast to PSG and TG here partial diagrams make sense once the inventory of word categories has been completed, since the relation of "dependency" is determined according to the same criteria for all parts of the grammar.

Modification of the traditional concepts of dependency

1. We drop the configuration criterion, which is taken from IC-analysis and therefore problematical to use, and we adopt the general postulate that morphological determination always runs from head to modifier, which is well-suited to the German. Together with strict²⁴⁾ use of the elimination criterion this produces the following:

1.1 Complete sentences are no longer necessary as a starting point. No specific group has ipso facto special status. Utterances such as example xxix. can be described according to their internal dependency structure without recourse to ellipses.

1.2. Nouns/pronouns which determine verb conjugation appear as dominant nodes (in other words nouns can dominate adjectives, articles and verbs).

19) For a discussion of the basic problem cf. R. Hudson: Word Grammar. In: Preprints of the Plenary Session Papers. The XIII. International Congress of Linguists. Tokyo 1982, 77-86.

20) Cf. J. Kunze, op. cit. S. 56 ff.

21) The configuration criterion is used to determine dominant elements or to determine the top of the subtree. The basic idea is similar to the distinction made in IC-analysis between "expansion" and "model" (Kunze: "Konfiguration" and "Resultante") which should be interchangeable in all contexts. The model or "verwandte" word forms are the highest node. (cf. Kunze op. cit. S. 58 ff., and R. S. Wells: Immediate Constituents. Language 23, 1947, 81-117, particularly 83 ff.)

22) The problem of the absolute criterion for elimination (can it be omitted) is sufficiently familiar; the relative criterion for elimination functions better (Kunze, op. cit. pp. 77ff.). Here word forms are determined which have to be omitted if other word forms are deleted. The criterion is therefore useful for determining subordinate nodes in the stemma.

23) P. Ihm, Y. Lecerf: *Éléments pour une grammaire générale des langues projectives*. Bruxelles 1963.

24) More strictly than e.g. Kunze p. 83 uses this criterion!

1.3. *and*-coordinations are treated like conjunctive phrases²⁵⁾ which can be subordinated to every type of word: *kommt und geht, Peter und Paul*. This has desirable results: *Peter und Paul kommen* but *Peter kommt und Paul* (extraposition).

2. The criterion of projectivity²⁶⁾ permits the differentiation of extraposition, word order in relative clauses, specific pre- and postfield fillers etc. from other word order patterns.

3. Syntactic makers such as case, number etc. are described as paradigmatic relations parallel to the dependency structure. Convergence e.g. of case markers is treated as neutralisation, similar to the use of the term in phonology.

4. System rules and norm rules are discoverable and easy to formulate in this theoretical framework. Examples xi. to xxv., for instance, show the construction of a noun group from a noun with directly dependent article, indirectly dependent adjectives and adverbs according to the rules of the system, while the different word orders, the different choices of word classes (xxiii., xxv.; xviii., xix) and the modification of a subtree (xxiii, xxiv) etc. are controlled by the norm.²⁷⁾

25) This is a result of the strict use of relative elimination. The dependency relation is so ambiguous that semantic counter-arguments have no weight.

26) Cf. Ihm/Lecerf, op. cit.

27) The limitations of this descriptive model are obvious: pronominalisation phenomena, the use of tempus and modus and other regularities which require the use of textual categories are not described.

ECP and the Distribution of Empty Categories

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0. This paper deals with some features of empty categories and the Empty Category Principle (ECP), which Chomsky proposed in the recent literature. Chomsky posits that the Universal Grammar (UG) consists of fairly restrictive systems of principles with limited numbers of parameters that have to be fixed by experience. It follows that the diversity over language could be attributed to the choice of modules in systems of UG and parameters. I will take the theoretical position of the Revised Extended Standard Theory, whose major features I will presuppose.

1. Let us assume the ECP below as formulated in Chomsky (1981), which will be redefined as we proceed.

- (1) Generalized ECP: If α is an empty category, then
- (i) α is PRO and only if it is ungoverned
 - (ii) α is trace if and only if it is properly governed
 - (iii) α is a variable only if it is Case-marked.

I will also assume the definitions of "government" and "proper government" as in (2) and (3) respectively.¹⁾

- (2) Proper Government: α properly governs β if and only if α governs β and α is lexical. ($\alpha \neq \text{AGR}$)
- (3) Government: α governs γ in the sentence
- $[\dots \gamma \dots \alpha \dots \gamma \dots]$
- where (a) $\alpha = X^0$ or is coindexed with γ
 - (b) where ϕ is a maximal projection, if ϕ dominates γ then ϕ dominates α .
 - (c) α c-commands γ .

To illustrate how this works, let us observe the following sentences:

- (4) a. who_i do you think [_S [_C e_i^1] [_S e_i^2 saw Mary]]
- b. *who_i do you think [_S [_C e_i^1 that] [_S e_i^2 saw Mary]]

In (4a), the trace e^2 is properly governed, whereas in (4b), e^2 is not, since e^1 in

• This is a truncated version of the paper presented at the 13th International Congress of Linguists, Tokyo, August 31, 1982.

1) Note that there are a number of different notions of government. See Aoun and Sportiche (1981) for further details on this topic.

COMP does not govern the nominative trace e^2 in the embedded sentence, in other words, it does not c-command e^2 in the embedded sentence, due to the presence of the *that*-complementizer.

2. Let us shift our attention to the *that*-relatives.

- (5) a. the book [_{S'} [_C which_i that] [_S e_i is no sale now]]
 b. the book [_{S'} [_C that_i] [_S e_i is no sale now]]

(5a) is ruled out by the ECP since the presence of complementizer *that* prevents e_i from being properly governed. If *wh* is deleted in (7a) then we get (7b), which violates the ECP. Pesetsky (1982) assumes that if the binding index can be transferred from the *wh*-phrase in COMP to a sister complementizer, then the trace in the embedded sentence is properly governed by the complementizer *that*, taking on the index of the *wh*-phrase. Consider the following rule.

- (6) COMP contraction (Pesetsky 1982: 306(25))

$$[{}_{\text{COMP}} \text{WH}_i \text{ Complementizer}] \dots \rightarrow [{}_{\text{COMP}} \text{Complementizer}_i]$$

Chomsky (1981) mentions the possibility of this move in that the complementizer *that*, which is assumed to have quasi-pronominal properties, tends to take an inanimate antecedent.²⁾

Following Chomsky (1982), revising the ECP as in (7), the LF representation for (5b), which is an S-structure, is illustrated in (8).

- (7) The Extended ECP (Chomsky 1982)

An *ec* is trace if and only if it is properly governed and is PRO if and only if it is ungoverned.

- (8) a. the book [_{S'} [_C O_i] [_S e_i is on sale now]]
 b. the book [_{S'} [_C] [_S O is on sale now]]

Chomsky (1982) introduces the vacuous operator, O , which he assumes is also an *ec*. It follows that at LF (8a) is derived from (8b) by the rule "Move α ." Thus, in (8a) *ec* must be \bar{A} -bound by an operator O . This follows from the binding theory. It is assumed that the operator O is semantically empty, apart from a *wh*-phrase. Therefore, *ec* is a free variable, assigned no range by its operator. Yet this fact violates the general principle barring vacuous operators. Chomsky introduces a principle, the Bijection Principle which states that each LF variable must be either assigned a range by its operator or assigned a value by an antecedent that \bar{A} -binds it. (Cf. Koopman and Sportiche (1981)). It follows that *ec* in (8a) must bound by *the book*, yet it is not locally bound by *the book*, but by O .³⁾

2) However, it is not always true. See Imai (1981), (1982) for further discussion.

3) See also Koopman and Sportiche (1981).

4) In order to accommodate this, Chomsky (1982) modifies one of the principles of the Binding Theory as in (i).

3. Chomsky (1982) classifies overt and empty categories by features of categorial expressions as in (9).

- (9) i. [+anaphor, -pronominal]
 ii. [-anaphor, +pronominal]
 iii. [+anaphor, +pronominal]
 iv. [-anaphor, -pronominal]

Note that these four categories of expressions are identified by the principles of the binding theory. In the present paper we will only observe *ec*'s. (9i) is an instance of NP-trace. (9iii) is an example of PRO and (9iv) is the case of variables. (9ii) is a new element, which Chomsky introduces, called *pro*. The small *pro* is the element governed by INFL at D-structure as well as S-structure. Chomsky considers *pro* to be a pure pronominal analogous to its overt counterpart.⁵⁾ Thus the ECP as we have seen in (7) will be reformulated as in (10).

(10) An *ec* is properly governed unless it has features [+anaphor, +pronominal].

4. In summary, it appears that the different types of *ec*'s exist and that one language may have all of the four items, while the other may have three except (9ii), etc. It is assumed that the choice of the four *ec*'s is a parameter which contributes to explain the diversity across languages in a uniform way.

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(i) An R-expression must be A-free in the domain of the operator that \bar{A} -binds it. But principle (i) is not necessary as Chomsky puts it. See Chomsky (1982) for further details.

5) Note that *pro* is only realizable in the subject position governed by AGR in pro-drop languages.

Conditional Constructions in Korean

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The conditional construction '...-mjən (roughly 'if')...' reveals certain constraints on semantic relations between the antecedent and the consequent clauses. When this construction is compared with the conditional statement in logic, material implication cannot adequately be applied to the description of conditional in Korean. Necessity and relevance are required between the antecedent -mjən clause and the consequent clause. By having self-evident truth in the antecedent, however, the effect of expressing the speaker's attitude to the consequent assertion by way of *modus ponens* can be obtained as in (1).

- (1) hæ -ka ton - ŋ'ok - esə t'i - (n - ta) - mjən
 sun SM East side from rise Pres Dec if
 čən - s'i - nin tət'əŋŋən - i-ta
 Chun Mr. Top President Cop Dec

'If the sun rises in the East, Mr. Chun is the President.' Such case as (2), in which the antecedent is false and the consequent is true and therefore the statement is true, represents the typical disparity between conditional in natural language and material implication in propositional logic. It is counter-intuitive to accept as true any statement whose consequent is true no matter whatever false antecedent it may take.

- (2) ???so-ka mulkoki-i-mjən, New York-in mikuk-e is' - ta
 cow SM fish Cop if Top U.S. in exist Dec
 'If a cow is a fish, New York is in the U.S.'

A 'true' statement in which both antecedent and consequent are false also has the problem of relevance. But this type is used to bring about the effect of asserting the negation of the antecedent by putting a clearly false proposition in the consequent as in

- (3) næ-ka kiri-ha-jəs'-(i)mjən, ne atil - i - ta
 I SM so do Past if your son Cop Dec
 'If I did so, I am your son.'

The clear falsity of the consequent is employed to implicate the falsity of the antecedent by way of *modus tollens* ($S_1 \supset S_2, \therefore \sim S_2 \supset \sim S_1$; 'If I am not your son, (just like that) I didn't do so'.)

The conditional construction can be related to the generic Topic construction and can be compared with the Topic in general. 'inkan-in čuk-nin-ta'

(Humans die.) has roughly the meaning of 'inkan-i-mjən čuk-nin-ta' (If someone is a human, he dies.). Cf. $\forall x (Hx \rightarrow Mx)$. However, the Topic of a sentence is normally an NP whereas the conditional is a clause, and a sentence can contain both a conditional and a Topic, whereas two Topics are normally impossible in one sentence. Furthermore, there is a difference in the degree of 'givenness' between the two constructions: the givenness of the conditional is weaker than that of the Topic. In these respects, Haiman (1978)'s claim that conditionals are Topics on the basis of morphological identity between conditional and Topic in languages such as Hua and the semantic similarity of showing 'givenness' should be given up in its initial form.

The conditional *mjən* makes us select and accept internally a possible world where the antecedent proposition becomes true, and when the consequent becomes true in the same world the whole conditional statement becomes true. But another conditional form *-ta-mjən* (*ta* being a declarative sentence ending originally) represents supposing externally a possible world where the antecedent is satisfied. The anomaly of the following sentence arises because the real and immediate condition *mjən* forces you to expect some sequentially following event and that expectation is broken by the consequent clause. If *mjən* is replaced by the hypothetical *ta-mjən* the sentence becomes fine. Also in Japanese (sin-) *dara* creates anomaly in (4), whereas (sinu-) *to-si-tara* does not.

- (4) ?*ne-ka næil nac jəltu-si-e čuk-(i)mjən, kɪ čən-e muəs-ɪl haltʰenja?

'If you die at noon tomorrow, what will you do before that?'

The form *ta-mjən* is often used for counterfactuals (e.g., 'næ-ka nalkæ-ka is'-*ta-mjən* nara ka-kes'-ta' (If I had feather I would fly.). But the clear contrary of the past fact is expressed by *-təra-mjən* immediately preceded by the past tense marker. It is used only when the contrary of the past fact has been confirmed. *-ta-mjən* does not need such confirmation.

The conditional and the concessive in Korean are not compatible in surface structure. The interesting interdefinable relations between '...*mjən* ('if') an('not') *tə-n-ta*('all right')' [forbidding], '...*to*('even if') *tə-n-ta*('all right')' [permission] and '...*jaman*('only if') *tə-n-ta*('all right')' [obligation] can be analyzed in connection with the conditional.

- (5) pom-i o- (a)*to* k'och-i pʰi-či ani-ha-n-ta (*... o-*mjən-to*...)
'Even if the spring comes, no flowers bloom.'

Behind (5) there is a presupposition, 'If spring comes, flowers bloom.' Even if the antecedent condition 'spring comes' is satisfied, still the expected consequent, 'flowers bloom' does not follow. Hence the sense of concession. The concession of *to* sometimes contrasts not with the negation of the main clause but with some other element as follows.

- (6) ne-ka o- *mjən* næ-ka ka-kes'-ko, suni-ka o-(a)*to* næ-ka ka-kes'-ta
'If you come, I will go, and even if Sooni comes, I will go.'

Compare (6), which has the *sufficient* condition marker *mjan*, with the following sentence, which has the *necessary* condition marker '*jaman*' (roughly 'only if').

- (7) ?* ne-ka o- (a)ja(man) næ-ka ka-kes'-ko suni-ka o-(a)to næ-ka ka-kes'-ta
'Only if you come, I will go, and even if Sooni comes, I will go.'

Let me explore the process of representing permission, forbidding, and obligation with following utterances.

- (8) ka-to t̥ə-ə. 'Even if you go, it's all right.' [Permission]
(9) ka-mjan ani t̥ə-ə. 'If you go, it won't do.' [Forbidding]
(10) ka-ja(man) t̥ə-ə. 'Only if you go, it's all right.' [Obligation]

Sentence (8) has sentence (9) behind as a pragmatic presupposition and both (8) and (9) presuppose the speaker's authority to perform forbidding. Originally, 'If you go it's not okay' as in (9), but concession occurs so that the negation of 'ani t̥ə-ə' (it won't do), i.e. 't̥ə-ə' (it will do) can hold 'if you go, even in case you go.' It is a logical consequence that if the one who has the authority for forbidding makes concession and does the act contrary to forbidding, it becomes permission. In (10), for 't̥ə-ə, (it will do) to hold, 'your going' is a necessary condition '*ja(man)*'. Therefore it becomes an utterance imposing an obligation to the other party. (10) logically entails (11).

- (11) ani ka-mjan ani t̥ə-ə. 'If you don't go, it won't do.'
not go if not do

Cf. 'You must go.'—'You are not allowed not to go.'

Similarly, in English 'I must go' entails 'You are not allowed not to go.' When we consider the examples (8) through (10), we can realize that the utterances of permission, forbidding, and imposing obligation in Korean are more analytically interwoven than those (may, must) in English.

The '*katin*' conditional can be used exclusively for the sentences of imperative category (including the sentences that contain the speaker's mind-binding modality). Observe.

- (12) ki ai-ka o-katin, ka-ra / ka- ca / ka-kes'-ta.
the child SM come go Imp go Propositional go will Dec
'If the child comes, you go! / let's go / I will go.'
(13) *ki ai-ka o-katin əmani-ka ka- si- kes'-ta
mother SM go Honor Fut Dec
'If the child comes, mother will go.'

The *mjan* construction can also be used for these imperative contexts. Therefore, there arises subtle distinction in meaning between these two different forms when they are used for imperative sentences as shown in

- (14) næ-ka ŷuk-katin anæ kjat̥ -e mutə -ta -o.
I SM die if wife side at bury give Imp
'If I die, bury me beside my wife.'

- (15) *næ-ka* *čuk-ĩmjən* *anæ kjət^h* -e *mutə* -ta -o.
 I SM die if wife side at bury give Imp
 'If I die, bury me beside my wife.'

Since the *katĩn* form is used only for the sentences of order, suggestion, and the speaker's mind-binding modality, the antecedent event precedes the action or intention of the main clause. Therefore, *katĩn* gives the feeling of having us anticipate the following action more positively than *mjən*. The *mjən* form emphasizes the condition.

It is clear from the above observation and analysis that we need a cross-linguistic empirical investigation of apparently self-evident logic-related expressions such as the conditional.

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Bridging

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It has been claimed that Japanese is a language with relatively free surface word order. There have been two types of theories developed to account for this phenomenon, the Standard Generative Theories advanced by Muraki (1974, 1979) and Harada (1977), and the Free Word Order Theories developed by Whitman (1979), Hale (1980), and their followers.

It is not the case, however, that all the sentences that are generated in these two approaches are acceptable. As will be shown below there are numerous sentences which are acceptable in a so called "base" word order but which are awkward, marginal, or unacceptable with "scrambled" word order. In spite of this problem, little research has been done to explain why some scrambled sentences are acceptable and why others are less than perfect. The objective of this paper is to spell out exact discourse conditions to this word order switch phenomenon.

Now consider the awkward, marginal, or unacceptable sentences (1) through (6).

- (1) ?/? Hitori no onnanohito o Taro ga nagutta.
 one woman acc sub hit
 (Taro hit a woman.)
- (2) ?/? Hitori no onnanoko ni Taro ga eki de atta.
 one girl dat sub station at met
 (Taro met a girl at the/a station.)
- (3) ?/? Issatu no hon o Taro ga katta.
 one book acc sub bought
 (Taro bought a book.)
- (4) ??? 1960 - nen ni Taro wa Tokyo de umareta.
 year in theme in was born
 (Taro was born in Tokyo in 1960.)
- (5) ??? Tokyo de Taro wa 1960 - nen ni umareta.
 in theme year in was born
 (Taro was born in Tokyo in 1960.)
- (6) ?/? Naifu de Taro wa ude o sasita
 knife with theme arm acc stabbed
 (Taro stabbed himself in the arm with a knife.)

These sentences are clearly generatable both in Muraki's and Harada's Generative framework and in Whitman's and Hale's framework. No filtering rules

have been proposed by these scholars for marking these sentences as awkward, marginal, or unacceptable.

Now compare sentences (1)–(3) with sentences (7)–(9).

- (7) Sono onnanohito o Taro ga nagutta.
 the woman acc sub hit
 (Taro hit the woman.)
- (8) Sono onnanohito ni Taro ga eki de atta.
 the women dat sub station at met
 (Taro met the woman at the/a station.)
- (9) Sono hon o Taro ga katta.
 the book acc sub bought
 (Taro bought the book.)

We notice that the imperfect sentences have indefinite NP's in sentence initial position whereas the acceptable sentences have definite NP's in the first position. Not only are the NP's in the first position of the acceptable sentences definite, they have actually been mentioned in the preceeding discourse as well. It seems that sentences (10)–(11) cannot be used unless "Hanako" is mentioned in the preceeding discourse.

- (10) Hanako o Taro ga nagutta.
 acc sub hit
 (Taro hit Hanako.)
- (11) Hanako ni Taro ga eki de atta.
 dat sub station at met
 (Taro met Hanako at a/the station.)

Based on these observations I make the following claim—that when the object in an SOV sentence is preposed to give OSV word order the object must perform a "bridging function". We define the bridging function as the function of providing a "bridge" between the preceeding discourse and the rest of the present sentence. Moreover, by preposing the object, the topic of the present sentence is established.

This bridging function is not limited to direct and indirect objects. In fact it seems that any element which is preposed from its normal position must have a bridging function. Compare sentences (4) through (6) with sentences (12) through (14).

- (12) Sono tosi ni Taro wa Tokyo de umareta.
 the year in theme in was born
 (Taro was born in Tokyo that year.)
- (13) Sono mati de Taro wa 1960-nen ni umareta.
 the city at theme year in was born
 (Taro was born in that city in 1960.)
- (14) Sono naifu de Taro wa ude o sasita.
 the knife with theme arm acc stabbed

because it is new information and therefore cannot function as a "bridge" between the preceding discourse and the present sentence. Therefore, it might appear that (17) below is a counterexample to the bridging hypothesis. In fact it is not. (17) is not an existential sentence with its subject preposed, but a sentence which specifies the location of its subject. It is an answer to the question "what is on the table?"

- (17) Hon ga teiburu no ue ni arimasu
 book sub table on exist
 (A book is on the table.)

Next look at sentences (18) and (19).

- (18a) Koen ni nanimo oiteimasendesita
 park in anything not found
 (There was nothing on the ground in the park.)
 (18b) Nanimo koen ni oiteimasendesita
 anything park in not found
 (Nothing was found in the park.)
 (19a) Koen ni daremo imasendesita
 park in anybody exist not past
 (There was nobody in the park.)
 (19b) Daremo koen ni imasendesita
 anybody park in exist not past
 (Nobody was found in the park.)

These sentences all appear to be existential sentences. One might think that the two sentences in (18) and (19) are synonymous. This, however, is not true. The (b) sentences are not existential sentences. To utter the (b) sentences we need a presupposition "X exists" and the (b) sentences are the negation of such a presupposition. Sentence (18b) is used, for example, when the hearer is looking for his lost belongings, for example, a handbag. It relates to a nonexistential locative sentence which implies that the hearer's handbag was not found on the ground of the park.

The second set of apparent counterexamples involves wh-questions. In Japanese, unlike English, wh-words in a question do not need to be fronted to sentence initial position. This is illustrated in (20) and (21).

- (20) Hanako wa Taro ni nani o watasimasita ka
 theme dat what acc hand Q
 (What did Hanako hand to Taro?)
 (21) Hanako wa hon o dare ni watasimasita ka
 theme book acc whom dat hand Q
 (Who did Hanako hand the book to?)

However there are questions such as (22) and (23) where the wh-word is fronted.

- (22) Nani o Taro wa Hanako ni watasimasita ka
 what acc theme dat hand Q
 (What did Taro hand to Hanako?)
- (23) Dare ni Hanako wa hon o watasimasita ka
 who dat theme book acc hand Q
 (Who did Hanako hand a/the book to?)

According to my bridging hypothesis, questions such as (22) and (23) should be unacceptable unless the fronted wh-words are interpretable as performing a bridging function. In fact these questions seem to require a special context which would facilitate the bridging interpretation of the wh-expression. Unlike (20) and (21) which are neutral questions for information, (22) and (23) are marked questions. They are only used under special circumstances, for example in court when lawyers try to induce an answer which they already know.

I have shown in the above that the sentence-initial elements in Japanese have to perform the function of bridging the preceding discourse to the present sentence. However there is a productive class of counterexamples to this hypothesis. Only in the case where the sentence initial element is the subject of the sentence does it not have to perform a bridging function. This is shown in (24) and (25).

- (24) Otokonoko ga hon o mottekita
 boy sub book acc brought
 (A boy brought a book.)
- (25) Senotakai otokonoko ga Hanako ni tikazuitekita
 tall boy sub dat approached
 (A tall boy approached Hanako.)

It does not seem that these sentences require their subject NPs to be mentioned in the preceding discourse. It seems that this phenomenon can be accounted for only by assuming that Japanese has an underlying word order, SOV. Under this assumption, the word order of (24) and (25) is not an intentionally produced order, but is the word order that obtains automatically if the basic word order is followed. Therefore the violation of the bridging function involved in (24) and (25) is nonintentional. According to Kuno (1979)'s hypothesis on the active-inactive discourse rules, when the violation is intentionally committed, it gets a penalty, hence unacceptability of the sentence results. However when the violation is committed nonintentionally, it does not get a penalty. From observations based on (24) and (25) I claim that Japanese has an underlying basic word order, SOV.

In this paper I have given the discourse conditions of the word order switch phenomenon, namely, I have explained why some scrambled sentences are acceptable and why others are less than perfect. I have hypothesized that Japanese has an underlying fixed word order, SOV, and only when an element performs a "bridging" function, that is, the function of providing a bridge between

the preceding discourse (or the nonlinguistic context at the scene of the speech) and the rest of the present sentence, can the element be moved to the first position of the sentence. I have also hypothesized that by preposing an element, the topic of the present sentence is established. I have not been able to discuss the distinction between this preposing process and thematization with the thematic particle *wa* attached. I have some evidence to show that the two processes are subject to different conditions, but I do not have time to discuss it here.

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Mari (Cheremis) "Pseudo-Relatives"

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Mari (Cheremis) is a Volga-Finnic language of the Finno-Ugric family and is the native language of some 540 thousand people living in scattered areas between the Middle Volga and the Urals. It is a postpositional SOV language in which the modifier precedes the modified. Like Turkic languages, Mari does not usually use finite subordinate clauses. Instead Mari subordinate clauses are formed by non-finite forms of the verb called "participles" in the standard grammar. Among them by far the most frequently and widely used is the *mE*-participle, which is the main topic of the present paper.

In general, a subordinate clause formed by the *mE*-participle differs from a finite sentence in that its subject occurs in the genitive case (*gen*) and that a possessive suffix (*px*) agreeing with the subject in number and person is attached to the head noun of a complex NP or to the participle in other cases. Examples (*pl*=plural, *PART*=participle):

- (1) tunemše-vlak tengeče Tartu ola-ške mijenət
pupil-*pl* yesterday Tartu town-to they went
'the pupils went to the town of Tartu yesterday'
- (2a) [tunemše-vlak-ən tengeče Ø mijə-me] ola-št
pupil-*pl-gen* yesterday go-*PART* town-*px*
'the town where the pupils went yesterday'
- (2b) [tunemše-vlak-ən tengeče Tartu ola-ške mijə-mə-št]
pupil-*pl-gen* yesterday Tartu town-to go-*PART-px*
'the pupils' having gone (that the pupils went) to the town of
Tartu yesterday'

As is shown in (2a) no element corresponding to the relative pronoun is used in Mari and the relativized NP is deleted. The subordinate clause of the type (2b) occurs as a verb complement (subject, object, or adverbial clause) or a postpositional complement. The present paper exclusively deals with cases like (2a).

A widely accepted assumption concerning complex noun phrases is that a complex NP is either a relative clause or an appositive (noun complement) clause. It is a *relative clause* if the embedded sentence contains one more noun phrase in underlying structure than it does at the surface—one which is identical with the head NP and is, in languages including Mari, deleted in the course of syntactic derivation. Any complex NP which does not conform to this characterization is to be regarded as an appositive clause (e.g., *the claim*

that you made a claim, an opportunity for you to read the book, cf. the claim that you made, a book for you to read). This dichotomy seems to work quite well with languages like English, where the class of nouns capable of taking an appositive clause seems to be semantically homogeneous (thus the term "content clause" used occasionally to denote appositive clauses).

In Mari, however, the construction of the structure [...V-*me*]_s N-*px* is used in such cases as well in which major European languages would use neither a relative nor an appositive clause. A noteworthy fact is that such complex NPs in Mari have almost always their literal counterparts in Japanese. Examples (*acc*=accusative):

- (3a) [ala-kō-n omsa-m čot peraltə-me] jük-eš-əže poməžaltəm
 some-who-*gen* door-*acc* hard knock-PART sound-into-*px* I awoke
- (3b) [dareka no doa o hagesiku tataku] oto ni megasameta
 someone *gen* door *acc* hard knock sound to awoke
 'I was waked by the sound of someone knocking hard on the door'
- (4a) [avam-ən kol žaritlə-me] puš-əžə-m šizən, ...
 my mother-*gen* fish grill-PART smell-*px-acc* noticing
- (4b) [haha no sakana o yaku] nioi ni kigatuine, ...
 my mother *gen* fish *acc* grill smell to noticing
 'having perceived the smell of my mother grilling fish, ...

Semantically, (3) and (4) are more like relative clauses than appositive or "content" clauses. (4b), for instance, denotes such a smell as is "involved in" my mother's grilling of fish in just the same way as the relative clause *the book you are reading* denotes a book which is involved in your reading. The appositive clause interpretation would be strange here, since an appositive clause, which is supposed to express the "content" of what is denoted by the head noun, presupposes an abstract noun as its head, whereas the head noun of (4b) denotes an object of physical perception. In Japanese any noun denoting an object of physical perception can, as a rule, take a clausal modifier of the type (3b) and (4b): *oto* 'sound', *koe* 'voice', *nioi* 'smell', *azi* 'taste', *tezawari* 'touch', etc. As to Mari, *jük* 'sound, voice' and *puš* 'smell' are the only instances I have of nouns denoting an object of physical perception. Considering the relatively small size of my data, however, there is little reason not to expect other instances to turn up in the course of further study. The construction of this type will be referred to as "pseudo-relatives."

There is one circumstance which needs special consideration. Mari as well as Japanese uses one and the same form of the verb where English would use finite or non-finite forms or even a deverbal noun depending on the case:

- (5a) tunem-me ij
 study-PART year 'academic (or school) year'
- (5b) [ačam-ən Tartu olase universitet-əšte tunem-me] ij-la-štə-že
 my father-*gen* Tartu town's university-at study-PART year-*pl-in-px*

'in the years when my father studied at Tartu University'

Mari dictionaries consider *tunem-me* used in (5a) to be an equivalent of the Russian adjective *učebnyj* 'pertaining to studying' (*učebnyj god* 'academic year'). Though this treatment may be quite all right from a practical point of view, I doubt that such *mE*-forms as *tunem-me* in (5a) are to be considered different from those *mE*-forms used in constructions like (5b). Rather, I assume that there is only one *mE*-form for each verb and that it is the environment in which it occurs that makes the *mE*-form in (5a) appear less verb-like (more adjective-like) than that in (5b). I further assume that the "degree of verb-likeness" of a *mE*-form is determined by the "degree of sentence-likeness" of the modifying clause (phrase) in which it occurs. The notion of "sentence-likeness," which is to be understood intuitively here, is probably of kin to the notion of "nouniness" proposed by John Robert Ross: it seems that the nounier the construction, the less sentence-like it is (cf. I expect *he will arrive happily*, I expect *him to arrive happily*, I expect his happy arrival). (5a) and (5b) are thus to be regarded as representing one and the same construction at the opposite extremities of sentence-likeness. In other words, (5a) is actually a relative clause and is to be interpreted as meaning something like "a year during which to study." The same line of a gument also applies to Japanese: *ugoku hodoo* (move footpath) 'a moving footpath,' *sinu kakugo* (die preparedness) 'the preparedness for one's own death.' Mari pseudo-relatives show different degrees of sentence-likeness too: *lūjə-mö jük* (shoot-PART sound) 'the report of a gun,' *šort-mo jük* (cry-PART sound) 'a tearful voice.'

There is another class of Mari constructions of the structure [...V-*mE*]_s N-*px* which fall outside the categories of relative clause and appositive clause. Japanese again has literal counterparts for them:

- (6a) [poškudo kalək-vlak dene kelšen ilə-me] kuməl (jüla)
neighbor folk-*pl* with agreeing live-PART mood (custom)
- (6b) [kinrin no syominzoku to nakayoku kurasu] sein (syuukan)
neighbor *gen* nations with peacefully live spirit (custom)
'the spirit (custom) of living peacefully with neighboring nations'
- (7a) [lūm mut gəč pale mut-əm əštə-me] jön
name word from sign word-*acc* make-PART means
- (7b) [meisi kara keiyoosi o tukuru] hoohoo
noun from adjective *acc* make method
'a means of deriving adjectives from nouns'

As is shown in (6) and (7), the embedded sentence is most likely to be subjectless with this class of nouns, but there are cases in which a subject does occur. In such cases, the construction looks like a relative clause or a pseudo-relative (*subj*=subject marker):

- (8a) [Evajn-ən počelamut vozə-mo] jön-zö
Evinc-*gen* poem write-PART method-*px*

- (8b) [Evain ga si o kaku] hooohoo
 Evine *subj* poem *acc* write method
 'Evine's way of writing poems'

The speaker of English is likely to associate this type with a construction like *his habit of getting up early* rather than with one like *the prospect of his getting up early*. The latter can be regarded as a non-finite variant of the appositive clause: compare *the fact that he gets up early* and *the fact of his getting up early*. The constructions of the type (6)–(8) will be referred to as "pseudo-appositives." Japanese nouns capable of taking a pseudo-appositive are *kuse* 'habit,' *tati* 'disposition,' *kakugo* 'preparedness,' *unmei* 'fate,' etc. Mari has a large inventory of nouns taking a pseudo-appositive: *kuməl* 'mood,' *jüla* 'custom,' *paša* 'work, task,' *jön* 'means, method,' *negəz* 'basis,' etc. In addition, many recent borrowings from Russian belong to this category: *metodika* 'methods,' *praktika* 'practice,' *nauka* 'science,' *obrazec* 'model,' *opət* 'experience,' *politika* 'policy,' etc.

I have shown that there are in Mari as well as in Japanese at least two types of relative clause-like constructions, pseudo-relatives and pseudo-appositives, for which English (and other major European languages) would not use a relative clause. It is worthy of noting that pseudo-relatives and pseudo-appositives occupy a significant part of the overall system of complex NPs in Mari as well as in Japanese. A conclusion that can be drawn from this is that pseudo-relatives and pseudo-appositives are to be regarded as being on a par with relative clauses, not as something secondary to the latter. A stronger claim would be to regard the three types as showing "different shades" of one and the same construction. This claim seems to gain support from the fact that the native speaker of Japanese does not feel any syntactic difference between *sakana o yaku ami* (fish *acc* grill gridiron) 'agridiron on which to grill fish,' *sakana o yaku nioi* (fish *acc* grill smell) 'the smell of (someone) grilling fish,' and *sakana o yaku kakugo* (fish *acc* grill preparedness) '(one's) preparedness for fish grilling.' What subtle difference there is between them is felt by the native speaker only when he or she is asked to translate them into English. It should be noted that Japanese transformationalist grammarians have tended to maximize the difference between the relative clause and the pseudo-relative/pseudo-appositive in terms of tree structure. Such an analysis seems all the less appropriate because in his recent book on English syntax Andrew Radford has brought English relative and appositive clauses so close to each other that they differ from each other only in the presence or absence of one N-bar node. What is to be hoped is that some system of describing noun modifying clauses, probably more lexicon-oriented, will be devised which can differentiate the subtleties of Mari and Japanese complex NPs without having recourse to inappropriate "structural" differentiation in terms of tree structure at the underlying level of representation.

Reordering in Japanese

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Within the framework of a generalized categorial grammar that is developed along the line of Bach and Partee (1980), a formal analysis of reordering, especially scrambling, is presented for Japanese. This framework, in principle, utilizes a simple operation, concatenation. It allows no transformations such as deletion, raising, and substitution, but only permutation and movement. I argue that scrambling in Japanese is predicted on the basis of a general structural configuration and then I propose a general convention which makes crucial use of this structural configuration.

I assume that there are two types of reordering in Japanese, one a bounded rule, scrambling, and the other an unbounded movement rule, and that they should be sharply distinguished from each other in terms of the difference in the domain and the manner of rule application. For example, in (1), the verb *meizi-ta* 'ordered' has three arguments, such as *John ga* (Nominative), *musuko ni* (Dative), and [*zookin de yuka o huk-u koto*] *o* (Accusative), and they are freely scrambled, yielding five scrambled variants of (1).

- (1) John ga musuko ni [_{VP} *zookin de yuka o huk-u koto*]
 NOM son DAT cloth INS floor ACC wipe-Pres COMP
o meizi-ta
 ACC order-Past

'John ordered his son to wipe the floor with a wet cloth.'

In addition, within the complement of (1), the two constituents, *zookin de* (Instrument) and *yuka o* (Accusative), allow scrambling and this will double the number of 'scramblable' variants. What is crucial here is that in Japanese, scrambling takes place only among clausemate constituents and it does not apply between the constituents in the VP-complement and those in the higher clause; a rule of scrambling is thus bounded in its application.

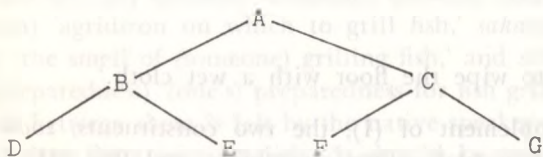
In fact, there are still other possible variants of (1), which would be derived by a different rule, an unbounded leftward movement rule, that Haig (1976) called "Emphatic Fronting", as shown in (2). (3) shows that this rule differs from scrambling in that, in principle, it permits only the single application and it does not apply to adverbs in embedded clauses.

- (2) a. *zookin de* John ga musuko ni [_{VP} *yuka o huk-u*]
 cloth INS NOM son DAT floor ACC wipe

- koto] o meizi-ta
COMP ACC order-Past
- b. yuka o John ga musuko ni [_{VP} zookin de huk-u
floor ACC NOM son DAT cloth INS wipe
- (3) a. ??yuka o zookin de John ga [_{VP} huk-u koto]
floor ACC cloth INS NOM wipe COMP
o musuko ni meizi-ta
ACC son DAT order-Past
- b. *gakkoo de Fred wa [_S Bill ga Mary ni kisusi-ta
school LOC Topic NOM DAT kiss -Past
koto o Jane ni osie-ta
COMP ACC DAT tell -Past
'Fred told Jane that Bill had kissed Mary at school'
- (3b) only means 'At school, Fred told Jane that Bill kissed Mary.'

As a general structural relation to predict reordering in Japanese, in Miyara (1982), I argued that if two constituents are in c(onstituent)-subjacency relation within a right branching structure, they can be scrambled. To clarify the notion, the 'right or left branching' structure, in (4) below, node E is c-subjacent to node C in the left branch, while node F is c-subjacent to node B in the right branch.

(4)



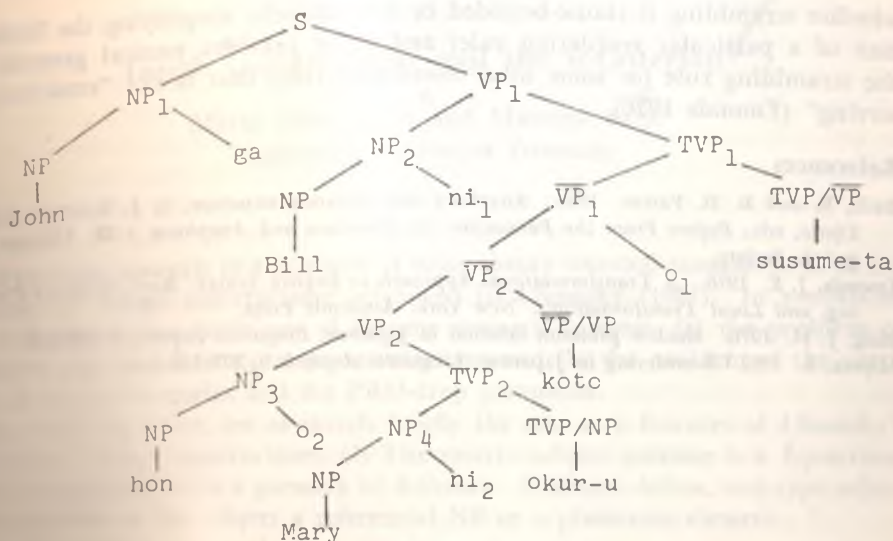
In Miyara 1982, I claimed that nodes F and B satisfy the necessary condition on scrambling in Japanese.

As an illustration, let us take up another example of scrambling. In (5), the subject NP, the dative NP, and the complement of category \overline{VP} can be scrambled and within the \overline{VP} -complement, the dative NP and the direct object NP can be reordered. (6) is the categorial structure of (5).

- (5) John ga Bill ni [_{VP} Mary ni hon o okur-u koto] o
NOM DAT DAT book ACC send-Pres COMP ACC
susume-ta
advise -Past
'John advised Bill to send Mary the book.'

S. Miyara

(6)



(The subscripts here are added merely for ease of identification.)

In the categorial structure (6), NP_2 is c-subjacent to NP_1 in the right branch and \bar{VP}_1 is c-subjacent to NP_2 in the right branch, hence the three constituents can be scrambled. The same thing is true in the case of NP_3 and NP_4 , which also satisfy the condition on c-subjacency and right branching structure. For this reason, NP_3 and NP_4 can be scrambled. Other pairs of constituents that satisfy the condition on c-subjacency in (6) exhibit a left branching relationship and thus cannot be scrambled. For example, though the syncategorematically introduced *ga* is c-subjacent to VP_1 and they are in the left branch, the two constituents do not satisfy the condition on the right branching structure; hence, they cannot be reordered. This same restriction on reordering holds between ni_1 and TVP_1 , between o_1 and TVP/VP , between \bar{VP}/VP and o_1 , between TVP_2 and \bar{VP}/VP , between o_2 and TVP_2 and between ni_2 and TVP/NP . Accordingly, in Japanese, any two constituents that satisfy both the conditions on c-subjacency and the right branching structure can be scrambled.

I propose a general convention for structural reduction, the 'tree-flattening convention' (TFC), which makes crucial use of the two configurational notions, c-subjacency and either left or right branching structure. This convention has the following characteristics: (i) the TFC provides uniform conditions on various reordering rules by making use of structural configurations; (ii) whether left or right branching structure is relevant for reordering is determined by the word order type of the language; (iii) it makes an interesting claim on reordering since only the sister constituents reduced by this convention are to be reordered if the structural description of a language-particular reordering rule is met; (iv) it naturally determines the domain of the application of possible reordering rules (that is, it is unnecessary to state the condition as to

whether scrambling is clause-bounded or not), thereby simplifying the formulation of a particular reordering rule; and (v) it provides natural grounds for the scrambling rule (or some other reordering rule) that is not "structure-preserving" (Emonds 1976).

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12-2

Tough Constructions and the θ -Criterion*

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The main concern of this paper is some theory internal consequences of the analysis of Tough constructions advanced in Chomsky (1981). In particular, we will discuss two problems of current syntactic interest: (a) the problem of parasitic gaps and S-structure representations; and (b) the problem of the partition of empty categories and the PRO-drop parameter.

As a starting point, let us sketch briefly the two core features of Chomsky's analysis of Tough constructions. (I) The matrix subject position is a θ -position. This assumption settles a paradox of θ -theory. As shown below, *easy*-type adjectives can take as the subject a referential NP or a pleonastic element.

(1) John_i is easy [_S O_i [_S PRO to please e_i]]

(2) It is easy [_S [_S PRO to please John]]

Instead of assuming a dual lexical characterization for this class of adjectives, we can simply state that *easy*-type adjectives do not assign a θ -role to the subject position. Now, (I) implies that Tough sentences do not have a lexical subject at D-structure. And this, in turn, implies that the matrix subject is inserted at some point after D-structure. Thus, a question remains as to how the inserted subject gets a θ -role, and this is where the second assumption comes in. (II) *Easy*-type adjectives trigger 'restructuring.' Due to restructuring, (I), for example, has the S-structure representation shown below.

(3) John is [_{AP} [_A easy to please] e]

Here, if *John* is coindexed with *e* under free indexation at S-structure, the latter is construed as an anaphor bound by the former. Thus, *John* and *e* form a θ -chain, and consequently, *John* gets a θ -role by virtue of being part of a θ -chain.

We have assumed above that prior to restructuring, there is an operator binding a variable in Tough construction. This hypothesis is confirmed by the fact that a parasitic gap can appear in Tough sentences.

(4) The book is hard to buy *t* without reading *e*
Following Chomsky (1982), let us assume that the core properties of parasitic gap constructions are, (a) an operator c-commands the 'real gap' and the 'parasitic gap,' and (b) neither of the gaps c-commands the other. Then, it follows that there must be an operator in Tough construction.

However, as pointed out in Chomsky (1982) and Montalbetti, et al. (to appear), (4) leads us to an even stronger conclusion. A parasitic gap, though it

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is a pronominal at D-structure, must be a variable at S-structure, as required by the Binding Theory and the θ -criterion. Thus, it follows that Tough sentences must have an operator at S-structure. But at the same time, the matrix subject in Tough construction must be part of a θ -chain at this level, as required by the θ -criterion and the Projection Principle. And this is possible only after restructuring. Thus, it follows that Tough sentences must have two representations, the restructured one and the unrestructured one, simultaneously at S-structure. This consequence is particularly interesting in the light of Chomsky's (1981) hypothesis about Japanese that each sentence has a dual representation at S-structure. If we assume that the so called "predicate raising" in Japanese is an instance of restructuring, and that restructuring results in dual representation, then we should expect that causative sentences in Japanese, for example, have a dual representation.¹⁾

Now, let us consider the following Spanish sentences:

- (5) Juan es fácil de convencer (John is easy to convince)
- (6) Juan es fácil de ser convencido (*John is easy to be convinced)
- (7) *Juan es fácil de ir al colegio (*John is easy to go to school)

As shown in (5) and (6), the preposition *de* appears in Spanish Tough sentences, and some dialects of Spanish allow the so called 'Tough passive construction.' (7) shows that the extraction from the subject position is impossible even in these dialects. On the other hand, the matrix subject position seems to be a θ -position, and a parasitic gap can appear in a Tough sentence.

- (8) Es fácil (de) convencer a Juan (It is easy to convince John)
- (9) Esta teoría es difícil de explicar *t* sin conocer *e*
(Lit. This theory is hard to explain without knowing)
- (10) Esta teoría es difícil de ser explicada *t* sin conocer *e*
(Lit. This theory is hard to be explained without knowing)

Given these sentences, we must say that Spanish Tough sentences are derived basically in the way English Tough sentences are, that is, they contain an operator and have a dual representation at S-structure due to restructuring. Thus, the crucial question at this point is why Spanish Tough construction has some properties that cannot be found in its English counterpart.

As for the preposition *de*, (5) and (6) are totally out without this preposition. Thus, although this can be an idiosyncratic fact about Spanish Tough construction, it seems likely that *de* is playing some crucial role in these sentences. In Montalbetti, et al. (to appear), we proposed that *de* is assigning Case to the following infinitival. This hypothesis predicts that when the infinitival is assigned Case in some other way, *de* is not required. And this prediction is indeed borne out by (8). In this sentence, the infinitival forms a chain with the expletive *pro* in the matrix subject position, and thus, inherits the nominative Case assigned by the matrix INFL. Consequently, this sentence is perfectly acceptable without *de*.

1) The idea that "predicate raising" may be an instance of restructuring was, as far as we know, first suggested by Kazuko Inoue.

The more important difference between Spanish and English is that only the former allows the 'Tough passive construction' exemplified in (6). (7) shows that (6) is not an instance of raising, and (10) shows that the trace in the complement object position of 'Tough passive construction' must be a variable. It should be noticed that in (10), if the trace in the complement object position is an NP-trace and the 'real gap' is in the complement subject position, then the 'real gap' c-commands the 'parasitic gap' and (10) should be out. Now, given that the trace in the complement object position of (6) is a variable, we immediately face two questions. How does this trace get Case? And, what is the empty category in the complement subject position?

As for the first question, we cannot say that *de* is assigning Case to the complement object position, since *de* is not adjacent to this position and also, it is already assigning Case to the infinitival as a whole. Furthermore, if *de* can assign Case to the complement object position, then it is not clear at all why it cannot assign Case to the complement subject position, that is, why (7) should be out. However, when we compare Spanish and English Tough constructions, it seems likely that *de* is playing some role in the Case-assignment to the complement object position in sentences such as (6). Also, given the adjacency condition on Case-assignment, we must say that in (6), it is the passive participle *convencido* that assigns Case to the complement object position. Thus, we are naturally led to the hypothesis that *de* somehow restores the Case-assigning ability of the passive participle *convencido*.

This hypothesis, we believe, is not as strange as it may sound, if we assume the interpretation suggested to us by Neil Elliott. That is, a passive participle, being a neutralized category between verb and adjective, lacks the value for the feature N. And the verbal complex *ser convencido* in (6), for example, being the head of the complement sentence in some sense, agrees with *de*. Now, due to this agreement, the verbal complex acquires the Case-assigning [-N] feature of *de*. If this interpretation is correct, then we should expect that if the complement verbal already has a value for the feature N, it cannot acquire the Case-assigning [-N] feature of *de*. And this prediction is borne out by the following data:

- (11) a. *Juan es fácil de llegar (*John is easy to arrive)
 b. *Juan es fácil de ser orgulloso (*John is easy to be proud)

Llegar, being a **ergative** verb, has the feature [-N], and *orgulloso*, being an adjective, has the feature [+N].

Now, let us move on to the second question. What can be the empty category in the complement subject position of (6)? Note that regardless of whether our answer to the first question is correct or not, we must say that the trace in the complement object position is a variable for the reasons already stated. Thus, this trace must be A-free due to the Binding Theory, and cannot be coindexed with the empty category in the complement subject position. Since the trace in the complement object position is coindexed with the matrix subject as well as the operator, this implies that the empty category in the com-

plement subject position is free. Hence, it must be PRO or *pro*. But this empty element does not form a chain with any other element in the sentence, and its position is not assigned a θ -role. Thus, it follows that it must be an expletive *pro*.

This conclusion, however, leads us to some new problems. First of all, if *pro* can appear in the complement subject position of (6), it is not clear why it cannot appear also in the complement subject position of (5). The complement subject of (5) gets the arbitrary interpretation, and hence, must be a PRO. This problem, however, may be resolvable, at least technically. For example, we may claim that *de* goes down to the COMP position and governs the complement subject position only when the verbal complex in the complement acquires the Case-assigning [-N] feature from *de*. Then, if *pro* has to be governed for some reason, then it follows that the complement subject of (5) must be a PRO.²⁾ But the second problem seems to be more serious. That is, in (6), there is no AGR licensing a *pro* in the complement subject position. In fact, the appearance of *pro* without a licensing AGR does not seem to be limited to the context of 'Tough passive construction.' Let us consider the following sentences:

- (12) a. De *e* comer una manzana, (él) tendrá dolor de barriga
(If he eats an apple, he will have a belly-ache)
- b. De *e* llamar por teléfono, parece que no lloverá
(If he calls, it seems that it won't rain)

The empty element in these sentences cannot have the arbitrary interpretation, and hence, seems to be *pro*. And here again, the features of this element are not recoverable from the INFL. (6) and (12) seem to indicate that *pro* can appear in the subject position even when its content is not recoverable from the governing INFL, and a proper analysis of these sentences may give us some clue as to why *pro* can appear all over the place in languages like Japanese, which does not have any kind of agreement phenomenon.

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2) If *de* in the COMP as well as AGR licenses a *pro*, this may be strong evidence for Safir's (1982) hypothesis that *pro* can appear in a position that is governed but not properly governed.

From Non-linear Realistic Grammar to Linear Formal Grammar

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0. Introduction

Grammar, when defined as a real object in the sense of Chomskyan linguistics, has two representations: realistic representation and formal representation. The former constitutes, at the mental and ultimately at the physical level, a realistic grammar (RG), which is an ontological description of the mental entities and mechanisms in the language faculty; the latter constitutes a formal grammar (FG), which is a formal description of a speaker's competence at the syntactic intuition. The present study attempts to suggest a new approach for the study of human language and cognition, with focus on (A) the substantial properties of RG; (B) the properties of FG; (C) the relation between RG and FG.

1. Substantial Properties of RG

Consider the sequence in Russian below:

- (1) *nashar rojina* (our mother-land)

Syntactically the two words are permuted in a temporal order in which *nashar* precedes *rojina*. But semantically, the "thinking order" is logically inferred to be adverse to the syntactic order since the choice of the pronoun *nashar* in its female-gender form must be made upon the semantic information of female-gender in *rojina* which is provided previous to that of the pronoun. This interesting phenomenon can be observed in any language and reveals an important property of RG, perhaps a substantial one, which helps to distinguish mental events from physical events, intelligence from non-intelligence and syntax from semantics in a loose sense. To account for this dual nature of a sequence we'd like to use the capitalized S for a syntactic sentence and the small s for a psychological or semantic sentence which is defined as (2).

- (2) An s is an equivalent of a syntactically-sufficed S

Then the substantial property of RG can be accounted for in terms of sequential non-linearity displayed in the presentation of s's. The basic idea about the sequential non-linearity is that an s does not recognize an intrinsic order of time as it surfaces in the form of intuitively syntactic structure. This non-linearity of RG exhibits the following characteristics:

- (3) An s is a *synchronism*, which means that an s is never generated but as a whole, or there is no temporal but only spatial process in the integration of the s.

- (4) A previous bit of semantic information will last till the finis of an S within the limits of memory-span.
- (5) A is a part of B which is at the same time a part of A, where A and B are two formally distinguishable elements of an s, roughly two lexical items.
- (4) together with (5) might lead to (6).
- (6)
 - i. semantic primitives are binary.
 - ii. the status of s's can be realized through the "from-bottom-to-top" device in Montague grammar.

2. The Properties of FG

In contrast to the non-linear properties of RG, FG is by virtue linear. Most of the grammars so far postulated are FG varying in degree of precision.

3. The Relation between RG and FG

The relation between RG and FG is assumed to be "extradition" whose generalization is as follows:

- (7) For every S there is a corresponding s and vice versa.
- (8) For an s there must be two and only two elements.
- (9) If s_1 contains s_2 and S_1, S_2 correspond to s_1, s_2 respectively, then S_1 entails S_2 , and there is a language-specific device to realize the relation between S_1 and S_2 .

The consequence of the sketchy proposal is hard to predicate, but a tentative study along this line has shown some promise of success, in the following aspects:

- I. the realistic understanding of mind
- II. an explanatory theory of language as a mental process
- III. a realistic interpretation of Chomsky's Logical Form or S-structures
- IV. a challenge to the century-old controversies of body-soul, language-mind, physical-mental, form-content, and syntax-semantics, etc.
- V. providing some new notions necessary for a grammarian's theorization in selection restrictions, co-indexing, entailment, presupposition, anaphoric relation, etc.

Anacoluthon and "Double Subject" Sentences

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Li & Thompson (1976), describing topic-comment sentences in topic-prominent languages refer to "double subject" constructions as "the clearest cases of topic-comment structures". They say that "all Tp languages have sentences of this type, while no pure Sp languages do as far as we know" (p. 468).

An example of Japanese was:

- 1) Gakkoo-wa boku-ga isogasi-kat-ta "School, I was busy".

I want to show, in this paper, that what is called "double subject" sentences in so-called Tp languages is also found in Portuguese, under the label of "anacoluthon". Consider the following example from oral Portuguese:

- 2) "E o almoço, eu volto mais cedo".

And the lunch, I'll come back earlier.

As in other languages, this construction is formed by an initial NP- the topic- followed by a comment S containing subject and predicate. It is not possible to say that the initial NP was moved from any point of the comment S, since it is complete. There is no element missing in it, as in topicalized Ss, nor any copy pronoun left behind, as in left dislocated Ss. Anacoluthons are perfect examples of Ss which depart from syntax. They are discourse-dependent and consequently, an interesting case for pragmatic study.

In order to interpret a construction like (2), we need to know what was said before in the discourse as well as the context of situation. S (2) was said in the following context: the housekeeper was giving instructions to her maid. She said:

- 3) Tina, pode botar a louça na máquina. *E o almoço, eu volto mais cedo.*

Tina, you may put the china in the dishwasher. And the lunch. I'll come back earlier.

She meant that, as for the lunch, she would come back earlier from work and she would prepare it. Out of context, S(2) could be understood as if the speaker was coming back to have her lunch earlier. But, in the context of situation this does not make sense, and the maid understood it as it should be. This shows how a S has to be understood in the context of situation as well as of the discourse.

I think S(2) illustrates well Grice's (1975) maxim: "be brief (avoid unnecessary prolixity)". In a very economic way, speaker and hearer understand them-

selves, not saying what can be supplied by the whole of the discourse and the context of situation.

From the syntactic point of view, these constructions are different from SP sentences. There is an intonational break between the initial NP and the following S, which is complete. The relation between the initial NP and the following S is a discourse relation, since the following S is always a comment about the initial NP which is the topic. It is the juxtaposition of the NP with the S which creates the semantic link between them.

Keenan-Schieffelin (1976) studied constructions similar to (2) in English under the label of left-dislocation. They describe them as having "the following format: Referent+Proposition. That is, some referent is specified initially and then followed by a proposition relevant in some way to this referent" (p. 240). Although many linguists, following Ross (1967), define left-dislocation as constructions which contain a coreferential pronoun, for Keenan-Schieffelin such constructions may not have a coreferential pronoun, as can be seen in the following example (p. 240):

- 4) "The mo- the modern art the twentieth century art, there's about eight books".

I am reserving here the label "anacoluthon" to those constructions which are similar to LD constructions, but do not have a coreferential pronoun, like (3) and (4).

Keenan-Schieffelin (1976) and others (e.g. Prince, 1980) state that LD constructions are typical of spontaneous, or informal, or unplanned discourse. This may be true of English or even Italian, but surely is not true of French, Portuguese, or Classical Greek as attested by traditional grammarians, who study LD under the label of "Pleonasm" and Anacoluthon (see Pontes, 1981, for more information on LD in written Portuguese, French, Spanish). Anacoluthon is described by Portuguese Grammarians as "putting in the beginning of a clause, without a grammatical link to the rest of the sentence the object's name, after which an assertion follows".

Portuguese grammarians give examples of anacoluthon found in the best writers of our language, from archaic to contemporary Portuguese. I will give here only one example, quoted by Câmara Jr. (1968), from a Brazilian modern poet:

- 5) "Estas estradas, quando novo Eliseu as percorria/as crianças lançaram-me These roads, when as another Eliseu I crossed them/the children threw pedradas".
stones at me.

As for the function of Anacoluthon in discourse, Keenan-Schieffelin say that constructions like that introduce a new topic or reintroduce topics. They say that LD constructions "appear most often in (...) an environment in which the referent does not appear in the immediately prior discourse". Its function

is to introduce discourse-new referents. They state further, that "Typically, the initial referent is some entity known to or knowable by the hearer from the non-verbal context of the utterance from some prior background experience" (p. 242).

Examples (2-3) tell us that Keenan-Schieffelin are right in one respect: the word "almoço" 'lunch' is introduced by this construction. But I don't think it constitutes a new topic. I think there is, in the conversation, a main topic, which is "kitchen work". The householder and the maid were talking about the work to be done in the kitchen. One was to wash the china, the other was to prepare lunch. Washing the china and preparing lunch are both sub-topics related to the main topic, which is "kitchen work". If one speaks of a 'new' topic in discourse one might be giving the impression that speakers are changing topics entirely as the conversation goes on.

It might be more accurate to speak of a main topic, the discourse-topic, or text-topic and of sub-topics, or sub-text topics. Prince (1980) seems to be thinking along these lines, when she says, about LD, that "Upon hearing a LD sentence, with NP_i in the leftmost position, infer that the speaker is about to begin a (sub-) text in which some entity is salient and which is judged to be of a certain "bigness" (p. 21).

It is interesting to notice that, although the Anacoluthon is also found in written language, it is not well accepted in formal written language. In this register, what corresponds to Anacoluthon is a construction exactly equal to (2), but beginning with an expression as "quanto a", "in relation to", "as far as X is concerned", etc. If we want to transform (2) in a construction well accepted in written formal language, it is sufficient to add "quanto a", before 2:

- 6) "Quanto ao almoço, eu volto mais cedo".
As for the lunch, I'll come back earlier.

This expression, "quanto a", is used, in written formal language, to begin paragraphs, and has the same function, I think, that "anacoluthons" have in other registers. We use "quanto a" in order to call attention to another sub-topic. This subtopic, usually, is related to some antecedent in the text as a sub-part of a main topic, the text-topic.

Prince (1980) noted that 23% of LD occurrences in a corpus were transcribed as paragraph-initial. Based in McKeon (1980) she observes that a "text-piece meets two criteria: (A) it must be (expected to be) a coherent (sub) text, and (B) it must be big" (p. 17). These two criteria are used by Prince to define LD structures, as can be seen from the quote I mentioned earlier on. I suspect that there is a relation between Anacoluthons and paragraphs. This suspicion is corroborated by an early research a student of mine is doing on connectives. We are finding a correlation between some connectives which we suspect are paragraph-introducers and the occurrence of LDs.

Anacoluthons occur in Portuguese as topic-reintroducers:

- 7) A. Não, realmente, João, acho que eu te falei, eu pretendo fazer
 Not, really, John, I think that I told you, I intend to do
 acupuntura em Odontologia. É só aparecer e eu vou... enfiar a cara
 acupuncture in Odontology. When it comes up, I'm going to... work
 prá ver se a gente faz um curso diferente. Um curso de *especialização*
 hard so that we may have a different course. A specialization course
 né? A gente clini... fazer clínica geral é muito bom, a gente... (a)
 ok? We clini... to do general practice is very good, we... learn
 prende muito ganha muito, né? O conhecimento não fica muito limi-
 tado... a lot earn a lot, uh? Knowledge isn't very limited...
 B. Humm-hum.
 A. enquanto que *especialidade*, a gente limita o conhecimento.
 while in specialization, we narrow down knowledge too much.

In this example, we see that the speaker began speaking of "specialization", then changed to "general clinic", contrasting the two fields of work in Odontology. After that, he returned to the first sub-topic: "specialization".

We see a similarity between this example and (2): there is a main topic, more general (kitchen work, odontology) and two sub-topics, two "alternatives", following Keenan-Schieffelin.

To sum up, Anacoluthon differs syntactically from LD and Topicalization because it does not have a resumptive pronoun, neither is it possible to say that some part of the comment-sentence was transformationally transposed to the beginning of the sentence.

However, it is similar to topicalized and LD sentences in the fact that it begins with a referent followed by a comment-sentence. The comment-sentence, as it happens with the so called "double subject sentences" in Chinese or Japanese, is complete, with subject and predicate. The relation between the referent-topic and the comment-sentence, which are juxtaposed, is one of discourse: we establish a link between them based on what Grice describes as maxims of conversation: "Be relevant" and "Be brief" (p. 46).

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On Mixing Configurational and Nonconfigurational Properties*

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0. Introduction

In this article I will address an issue which arises from the discussion of the configurationality parameter in recent work by Hale and Chomsky (Hale (1981, 1982), Chomsky (1981)). The discussion itself stems from the contention in Chomsky (1965) that grammatical functions are definable in terms of the configuration in which the constituent bearing them appears (the definition of the *SU*(bject) of the sentence as the NP immediately dominated by *S*, [*NP*, *S*] or the *D*(irect) *O*(bject) as the NP immediately dominated by *VP*, [*NP*, *VP*]). This is related to (although certainly distinct from) the question to what extent the order of constituents is fixed in the base. If constituent order is fixed in the base, and grammatical functions are assigned configurationally, it follows that syntactic processes in which an NP receives a thematic role (θ -role) associated with one function and some morphological (e.g. Case) marking associated with another, involve syntactic movement. Standard examples are passivization, in which an NP receives the θ -role of the *DO* and the structural Case of the *SU*, and other types of raising. It has been argued by Hale and others (eg. Farmer (1980), Nash (1980)) that there are languages in which the order of arguments is essentially free, and in which these arguments are directly assigned to slots in the thematic grid of their functors, not mediated by configurationally defined functions. There is no (or only limited) syntactic movement in such languages. It has been suggested in the literature (eg. Chomsky (1981), Hale (1982)) that differences of this kind constitute a major syntactic parameter: languages can be *+configurational* (eg. English), or *-configurational* (eg. Warlpiri, Japanese). In configurational languages an argument can become associated with different grammatical functions only by undergoing the syntactic movement rule *Move α* : the various grammatical functions of an argument are represented in its function chain. Hence, in these languages the function chain represents the different positions occupied by an argument in the course of the derivation of a sentence. For instance, in the case of a simple passive, the subject NP will have the function chain [*SU*, *DO*]. In nonconfigurational languages the principle of random *G*(rammatical) *F*(unction) assignment is employed instead. The output of *GF* assignment is restricted by similar principles as the output of (also random) movement in configurational languages. *GF* chains are formed directly, without movement being an intermediary. Thus, configurational languages use *Move α* , nonconfigurational ones use *Assume GF* instead. In the case of a passive-like structure in a nonconfigurational language, a chain [*SU*, *DO*] will be formed

when an argument on the basis of *Assume GF* first is assigned the DO function, thereby acquiring the associated θ -role, and then assumes the SU function receiving the concomitant Case. The most interesting question concerns now the nature of the parameter. Is it indeed the case that there is a dichotomy in the set of natural languages along these lines; does the parameter \pm configurational represent an independent principle, or it reducible to other principles and their interaction? A first step towards an analysis of the true nature of the parameter is to determine whether it does indeed induce a dichotomy on the set of grammars of natural languages in the sense that grammars either only employ configurational principles or only nonconfigurational ones. In other words can *Move α* and *Assume GF* appear together in a grammar or are they mutually exclusive? In Chomsky (1981) it is shown that there is a sense in which *Assume GF* cannot do the job of *Mov α* when applied to a configurational structure; one cannot take the surface structure of *John is considered stupid*, simultaneously using it as D-structure, and derive the correct L(ogical) F(orm) by applying *Assume GF*, in a manner which is consistent with other theoretical principles. For instance, such a derivation would violate the projection principle and the θ -criterion since in its surface position *John* is not governed by the predicate *stupid*, hence could not be θ -marked by the latter. On the other hand, accepting the argument presented by Chomsky, nothing prevents the grammar of English containing both *Move α* and *Assume GF*, only, in grammatical derivations the latter cannot but apply vacuously. That is, assuming a derivation of the example under consideration from the representation [_{S₁} e is considered [_{S₂} John stupid]] by moving *John* from the subject position of S₂ to that of S₁, and hence assigning it the function chain [SU_{S₁}, SU_{S₂}], all derivations in which *Assume GF* would assign a function different from that fixed by the configurations would be ruled out for independent reasons. This is important, though, in that it shows that there is no inconsistency in a view of universal grammar in which configurational and non-configurational principles coexist in general, and in fact coexist in the same grammar, while other properties of the grammar determine which of them may apply nonvacuously. If this is correct, we may go one step further and entertain the possibility that it is not so much the general properties of a grammar which determine which set of principles may apply nonvacuously, but rather specific properties of particular constructions. I.e. it would appear to be possible for there to be languages in which configurational principles apply in some constructions and non-configurational ones in others. I will argue that this case is indeed realized, namely in certain cases in Dutch where complement clauses are restructured with their matrix clause.

1. Restructuring in Dutch

The limitations on the size of this contribution prevent me from presenting a justification of the theoretical principles underlying my analysis of the relevant infinitival complements in Dutch; in fact I can hope to do no more than convey an impression of the analysis itself and the facts it explains. For details I have

to refer to Reuland (1981, 1982 and to appear). Dutch is an SOV language, the base position of complement clauses is on the left of the verb of the matrix clause. For independent reasons clauses are often subject to extraposition. Infinitival complements of (mainly) perception verbs cannot be extraposed; instead, their verb is adjoined to the right of the verb of the matrix clause by 'verb-raising' (see Evers (1975)). The process may be iterated and result in a string of verbs in an order which mirrors the base order. If the structure contains an auxiliary which normally requires its complement verb to assume participial form (eg. *hebben* 'have'), in this construction, the verb must appear in infinitival form instead. It follows that something more than simple movement is going on. The facts are illustrated in (1) with the relevant structure indicated (using subordinate sentence forms).

- (1) a. *dat [_S A1 [_{VP} [_{S₂} Pat [_{VP₂} de ratten vang-] [_{INFL} -en]] gezien] [_{INFL} heeft]]
 that A1 Pat the rats catch Ø seen has
- b. dat [_S A1 [_{VP} [_{S₂} Pat [_{VP₂} de ratten e_i] [_{INFL} e_j]] e_h] [_S heeft zien_h
 vang-en_j]] *gezien_h]

The phenomenon of infinitive for participle (IPP) shows that the formation of the verbal cluster labelled α (indicating a merger of the categories INFL and V) involves restructuring: the occurrence of the infinitive is explained, since under the conditions on restructuring given in Kayne (1981) restructuring between the verbs involved would be blocked by a participle in this position. The reason for restructuring is that otherwise the subject of the complement, *Pat*, could not receive Case. In (1a) *Pat* is only governed by the infinitival ending /-en/ which is not a Case assigner. The basic pattern of government is that the c-command domain of the head of a construction (INFL in the case of S) is inaccessible to government from a source outside that domain. Thus the presence of /-en/ blocks government of *Pat* by *gezien*. I will propose that this inaccessibility does not hold if the head and the outside governor involved are coindexed. So, in (2a) Z in the domain of X^0 is an opaque position wrt. Y^0 , but in (2b) it is not wrt. X^0 .

- (2) a. ... [\bar{x} Z X^0 ...] Y^0 ... b. ... [\bar{x} Z X^0 ...] Y^0 ...

The standard case of (2b) obtains when X^0 is the trace of Y^0 ; other reasons for coindexing are not ruled out. Thus, in (1b) *Pat* can be governed by and receive Case from a verb, just in Case this verb is coindexed with e_j (null heads create opacity just like overt heads). The result of restructuring is that a complex category V/INFL is formed. The indices of its constituents may percolate up. Thus α as a whole in (1b) may acquire the index set {i, j, h}. As a consequence, α and its constituents govern into the domains of e_h , e_j , and e_i . This entails that at S-structure θ -roles must be assigned according to nonconfigurational principles since each of the elements of α governs all the arguments involved: the configuration does not tell that *de ratten* receives its θ -role from *vang-* instead

of from *zie*-. Yet, this manner of θ -role assignment at S-structure cannot but mimick what was determined by the D-structure configuration. Any other assignment leads to a violation of the θ -criterion. There are, however cases in which free θ -role assignment on the basis of *Assume GF* is crucial. The construction can be used with the SU argument missing, as in *dat de jongens elkaar zagen neersteken* 'litt. that the boys each other saw stab'. This sentence only has a passive-like interpretation: 'that the boys saw each other being stabbed (by some person)'. It can be shown that *elkaar* must be the syntactic subject of the complement. Yet it is the thematic object of *neersteken*. Since there is no passive morphology on *neersteken* simply moving *elkaar* from the DO position to the SU position violates the θ -criterion: since the SU position is not dethematized *elkaar* receives a θ -role from both members of the chain [SU, DO] (see the discussion in Chomsky (1981): [NP, S] lacks a θ -role if [NP, VP] has no Case.) Under the assumption that the percolation of the indices of the components of a verb cluster is optional (as are most syntactic processes) the following analysis is available (in (1) taking any other option but the one discussed leads to ungrammaticality for obvious reasons). Consider the structures given in (3), where α as one of the options picked up the index set {h, j}.

- (3) a. $\text{dat } [_S \text{ de jongens } [_{VP} [_{S_2} e [_{VP_2} \text{ elkaar } e_1] e_j e_h] [_\alpha \text{ zagen}_h \text{ neerstek}_1\text{-en}_j]_{\{h, j\}}]]$
 b. $\text{dat } [_S \text{ de jongens } [_{VP} [_{S_2} \text{ elkaar } [_{VP_2} t_k e_1] e_j] e_h] [_\alpha \text{ zagen}_h \text{ neerstek}_1\text{-en}_j]_{\{h, j\}}]]$

Since α does not bear the index i, at S-structure *elkaar* is not governed by *neersteken* in (3a). Hence it has no Case and the passive rule applies, dethematizing the SU position. Now it can move, yielding (3b). As a SU it can receive Case (α bears the index j). It cannot inherit its θ -role from t_k , since t_k is not governed by *neersteken*. However, the structure is saved, since one of the outcomes of random θ -role assignment is that under which *elkaar* is assigned the θ -role of the DO of *neersteken*, in conformity with the θ -role it received at D-structure by configurational principles, before V-raising took place. This is possible since as the SU of S_2 *elkaar* is governed by α and hence by each of its constituent verbs. So, as required by the projection principle *elkaar* is governed by *neersteken* both at D- and at S-structure. The discussion of (3) shows that the nonconfigurational assignment of θ -roles at S-structure makes an essential contribution to its derivation. The hypothesis that the grammar of Dutch employs both configurational and nonconfigurational principles thus receives theoretical and empirical justification.

2. Consequences

This result has some consequences for the theory of nonconfigurationality. In the attractive view presented in Hale (1982) the parameter resides in the categorial component. Free word order follows from the minimal categorial specification of the nodes; the necessity to employ nonconfigurational principles follows from the phrase markers being relatively 'flat'. Hale proposes that non-configurational languages only employ the endocentric PS rule schema (i) $\bar{X} \rightarrow$

... \bar{X} ... instead of both (i) and (ii) $\bar{X} \rightarrow \dots \bar{X} \dots$ as configurational languages do. The "lack of hierarchical depth" in languages using only (i) causes the greater "looseness" of grammatical organization. He then suggests that the availability of a universal principle such as *government* for a particular grammar may depend on the presence of sufficient hierarchical structure, i.e. it is available to two-bar languages, but not to one-bar languages. Hale observes that there are two ways to relate lack of hierarchical structure to the absence of government. One might say that in "flat" structures "government... cannot serve to partition a structure into distinct sub-phrasal domains of government." As an alternative, he proposes that in flat structures government simply does not operate. This is the alternative for which Hale opts. It is not entirely clear to me whether there are immediate empirical differences between these two views of the relation between government and configurationality. There is a considerable conceptual difference, however, which is manifest in the issue we started out with; namely, whether languages (or perhaps rather grammars) are completely of one of these types, or may exhibit mixed properties. In the view that regards *government* as a principle that either "clicks on" or "shuts down", languages with mixed properties would not be expected to exist. In a view which says that *government* may become irrelevant under certain conditions, namely when we simply have government all over the place in some domain (but, crucially, government as such remains operative), one would expect mixed language to be realized: nonconfigurational strategies become relevant whenever government fails to distinguish the relevant domains. In the end, the theoretical import of the results obtained may relate to questions like the following: How typical of natural language in Dutch? (As an aside, I once heard an exasperated linguist doubt that Dutch is a possible language.) Anyhow, (if it is) the typological result of this investigation is that the complete absence of hierarchical structure is not a necessary condition for nonconfigurational strategies to apply, and that languages of a mixed type do exist.

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On the Syntax of Free Word Order Languages: Evidence from Basque

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0. Introduction

In a classic article on word order typology Joseph Greenberg observes that the vast majority of languages have several variant orders but a single dominant one. Given the possible permutations of Subject (S), Verb (V), and Object (O), the most common dominant orders are: VSO, SVO, and SOV. These observations, which were made over thirty languages and classify Basque as SOV, led to Greenberg's first universal:

- (1) *Universal 1.* In declarative sentences with nominal subject and object, the dominant order is almost always one in which the subject precedes the object. (Greenberg 1963:77)

As indicated by the title, this paper will show that, on closer inspection, Basque exceeds Greenberg's basic order typology as a true 'free' word order language. Furthermore, this paper will argue that free word order phenomena are derivative concepts of general modes of grammar and need no special statement in the grammar of the language in question.

In section 1 evidence will be presented to the effect that the concept of 'dominant' or 'basic' word order is theoretically excluded in Basque. In fact, in this language all major constituents appearing in declarative sentences may occupy the focus-like position (known as the *galdegaia*) immediately before the main verb. Consequently, contrary to the basic order typology, in Basque all permutations of Subject, Object, and Verb (as well as oblique complements) are basic, since each permutation is by definition a syntactically distinct construct receiving a unique semantic interpretation. (Authors 1981)

In section 2 further evidence is presented which correlates the free word order phenomenon of Basque, now reinterpreted as an extended case of Inversion, with other phenomena in the syntax of this ergative language. It will be shown that the three argument/thematic positions (Subject, Direct Object, Indirect Object) can not only be freely inverted but also omitted (pro-dropped). Moreover the same three positions are obligatorily 'doubled' (cf. Jaeggli 1980) onto the verb morphology. This scenario of free Inversion and Pro-drop along with obligatory doubling provides us with an extended case of the so-called pro-drop parameter observed in languages like Italian and Spanish with 'rich' thematic specification on the verb. (Authors 1982)

Section 3 presents a characterization of Basque within Chomsky's (1982)

Government and Binding framework. The concluding remarks deal with free word order, configurationality and grammatical relations.

1. Free Word Order

The phenomenon of free word order in Basque is illustrated in (2). Given the affirmative form of the English declarative sentence 'John has read the book to Mary' all permutations of the three arguments with the verb phrase actually occur as well-formed surface structures. However, not all theoretically possible

- (2) (a) (i) Jon-ek Miren-i *liburu-a* irakur-ri dio
 John-E Mary-D book-A read-1prt 3sA-[prs-Ax2]-3sD-[3sE]
 'John has read the book to Mary'
 (ii) {Jonek, Mireni, liburu-a irakurri dio}
 *(iii) ... liburu-a X irakurri Y dio ...
 (b) (i) Jonek liburu-a *Mireni* irakurri dio
 (ii) {Jonek, liburu-a, Mireni irakurri dio}
 *(iii) ... Mireni X irakurri Y dio ...
 (c) (i) liburu-a Mireni *Jonek* irakurri dio
 (ii) {liburu-a, Mireni, Jonek irakurri dio}
 *(iii) ... Jonek X irakurri Y dio ...
 (d) (i) *irakurri* dio Jonek liburu-a Mireni
 (ii) {Jonek, liburu-a, Mireni}
 *(iii) X irakurri Y dio ...

permutations of each of the three arguments and the verb phrase are logically well-formed. This is due to the logically significant concept of *galdegaia*. Basque requires that every sentence with overt arguments must have one in *galdegaia* or focus. Generally, the syntactic *galdegaia* position is the one immediately before the verb. If no argument precedes the verb, then the verb itself is interpreted as the *galdegaia*. In (2) (a-d) the permutations given in (i) are well formed structures both syntactically and logically. The full set of logically well-formed permutations for each case (a-d) are defined in (ii). In (iii), one finds for each case (a-d) the logically impossible permutations, corresponding to those in which a variable X intervenes between the stated *galdegaia* and the main verb (or a variable Y breaks the strict sequential order obtaining between the main verb and its auxiliary in affirmative sentences). Note, in particular, the permutation (2) (d) in which the verb itself is the *galdegaia*. In this case the particle *ba* is usually preposed with synthetic verbs and not one of the arguments may precede the verb phrase, although permutations among the arguments themselves will yield logically well-formed constructs.

As presented in (2) free word order in Basque is limited to the major constituents in a sentence, thus effecting the rule expanding S in the Base in languages like English. No reordering rule, be it transformational or part of the Base, can offer a satisfactory solution since it would posit a basic word order

which in Basque is not supported. A logical form option is perhaps the most appealing solution but it will not be considered at this moment. A syntactic solution will be presented in section 3, after we look a little further into the syntax of Basque.

2. Inversion, Pro-drop, and Inflectional Case Marking

The data presented in (2) show that any argument/thematic category in a sentence may appear before its verb (in a fixed *galdegaia* position) or may be inverted. In (3) one can observe that a parallel phenomenon occurs in the language whereby any or all argument/thematic categories in a sentence may be omitted (or 'pro-dropped'). Thus, *zuk* 'you' is omitted in (3) (b), *zuk* and *Mireni*

- (3) (a) *zu-k Miren-i liburu-a irakur-ri d-io-zu*
 'you-E Mary-D book-A read-lprt 3sA-[prs-Ax2]-3sD-2sE
 'You have read the book to Mary'
 (b) *Mireni liburua irakurri diozu*
 (c) *liburua irakurri diozu*
 (d) *irakurri diozu*

'to Mary' in (3) (c), and *zuk*, *Mireni*, and *liburua* 'the book' in (3) (d) leaving only the verb as a perfectly well-formed construction.

Further insights into the structure of Basque come from the binding relation existing between the morphological case marking system of nouns and pronouns and the inflectional system of the verb. In addition to tense and aspect, the verb, in this ergative language, agrees with its arguments via a case bound system of pronoun-like morphemes in portmanteau with the auxiliary verb, or with the main verb in synthetic constructions, as one can see at a glance in (3) (a). In this example, for each argument in the sentence there is a corresponding marker which is part of the inflectional make up of the auxiliary verb. One can observe the antecedent-anaphor pair *zuk/zu* for the ergative case marked argument 'you'. For the dative case marked argument 'to Mary' the pair is *Mireni/io*, and *liburua/d* is the antecedent-anaphor for the absolutive argument 'the book'. (Cf. Authors 1982 for a more detailed treatment of this topic.)

Consolidating the phenomena of Inversion and Pro-drop in Basque we recognize an extended version of the so-called 'pro-drop' parameter (Chomsky 1982) distinguishing accusative languages like Italian and Spanish from French and English. It must be noted, however that whereas in the accusative languages Inversion and Pro-drop obtain for subjects, but not for direct and indirect objects, in this ergative language the phenomena apply regardless of the subject/object asymmetry. Moreover, from a cross-linguistic perspective the argument-anaphor doubling of Basque reminds us of Clitic Doubling in Porteno Spanish (Jaeggli 1980). Basque and Romance are similar in that in both the anaphor is obligatory while the bounding argument may appear as an empty category. Morphologically the anaphors are clitics in Romance but inflectional

suffixes in Basque. The typological statement which seems to arise from these comparative remarks is the negative correlation which obtains between free Inversion and Pro-drop in contrast with obligatory argument-anaphor doubling. The analysis of Basque which is presented in the next section relies mainly on this observation.

3. Government and Binding

The extended pro-drop parameter just exemplified for Basque exceeds the characterization of pro-drop vs. non-pro-drop language proposed in view of languages like Italian vs. English, as the incorporation of an optional rule of syntax assigning INFL to VP. Any adequate government and binding theory of Basque must be rich enough to provide at S-structure a scenario in which all major categories in S (a) appear in all possible permutations and (b) are ungoverned. Any hypothesis characterizing such a theory must insure that INFL (including AGR) is in \bar{V} and, at the same time, that not one of the major categories is \bar{V} . One such viable theory for Basque would incorporate a (partially) unordered base roughly of the form (4).

- (4) (a) $S : (X^{max})^* \bar{V}$
 (b) $\bar{V} \rightarrow e \quad \bar{V} \quad e = galdegaia$
 (c) $\bar{V} \rightarrow V \quad INFL$
 (d) $INFL \rightarrow (Aux) \quad AGR$

The first base rule (4) (a) expands S as a 'mobile' set, following a discussion in Bach (1975) and, perhaps, along the lines of a proposal by Lapointe (1981). The mobile-S hypothesis allows the grammar to provide at D-structure a set of ordered phrase markers defining the possible permutations of the verb with its arguments. Such a rule, as it marks no particular order, is a general convention and not part of the linear order restrictions of Basque (4) (b-d). In the base (4) all major categories in S are generated in ungoverned positions. In this theory of Basque, the cyclic nodes are S and \bar{V} . \bar{V} is the binding node. INFL is not a governor. There are two general rules of syntax (5). The rule Move- α

- (5) (a) Move α (into *galdegaia*)
 (b) Move WH (from *galdegaia* to *galdegaia*)

moves any major category into the empty structural *galdegaia* position *e*. The *galdegaia* must be filled at S-structure and at logical form. Operators like WH-words and negatives must be in *galdegaia*, the focus position. Extraction of WH-words is possible from *galdegaia* position through the rule of Move-WH which applies cyclically in Basque from *galdegaia* to *galdegaia* (cf. Authors 1981). In (4) (d) it is proposed that the inflectional anaphors A(bsolutive), D(ative) and E(regative) are generated in the base under \bar{V} as part of the AGR(eement) node. Consequently they are governed by V. These anaphors, which bear Case are cosuperscripted at D-structure with any major category in S, which also bear case. Case

is checked at S-structure. In Basque, there is one governor V (and, if it can be demonstrated, P(reposition)). There seems to be no need for the COMP(lement) position, though complementizers are suffixed to the verb.

4. Conclusion

This paper has shown that free word order in Basque (section 1.) is not an isolated phenomenon requiring a special statement in the grammar of the language. Rather, it is only one aspect of an extended version of the pro-drop parameter evidenced in the syntax of the language. In a theory of Government and Binding, inflectional anaphor doubling, Inversion (free word order) and Pro-drop (the possibility of null categories) observed in Basque fall under an organizational principle subsumed under the concept of governed/ungoverned category. In this ergative language all major lexical categories are ungoverned but in a binding relation with their anaphors which are all governed by the the verb. In this syntactic scenario, then, lexical categories may be freely inverted/moved as well as omitted owing to their binding relation with a system of inflectional anaphors, whose order is fixed and cannot be null. Furthermore the phrase structures generated by the base rules, as in (4) (a), have no hierarchical configurations such as NP(S), NP(VP), or NP(PP). Thus, all arguments in Basque are the same from the point of view of the grammatical/syntactic function that each may display in the syntax of the language. Evidence for the lack of asymmetry between Subject and Object has been seen in the application of Inversion, Pro-drop, and Case Doubling. In the Romance languages these phenomena are clearly restricted to specific grammatical/configurational functions. In Basque no such restriction exists. The projection of thematic roles in languages like English is insured by the fact that each lexical category is governed and in a configurationally defined position. In Basque thematic roles are projected through the inflectional anaphors, which are governed, occupy fixed structural positions, cannot be omitted, and are in a binding relation (marked by Case) with their lexical categories.

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Patient Centrality and English Verbal Derivation

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Lexicase is a non-transformational generative lexicalist framework of grammatical description. Unlike the other frameworks which have recently cloven to the lexicon and embraced 'thematic relations' or 'functional structure', lexicase has accumulated a long track record of successful full-scale applications to the description of a number of Asian and Pacific languages.

One of the fundamental claims that it is now possible to make within the lexicase framework is what may be referred to as 'Patient centrality'. According to this hypothesis, the Patient case relation is obligatorily present in the case frames of all verbs (cf. Gruber 1965). Semantically, the Patient marks the perceptual center of a predication, and syntactically, a number of relationships among verbal clauses can be accounted for explicitly, economically, and insightfully in terms of verbal derivation rules involving promotion or downgrading of Patients, without resorting to ad hoc and empirically vacuous transformational rules. By stating such rules in terms of the limited set of Case Relations available in a lexicase grammar (cf. Starosta 1982), we place an automatic empirical constraint on the kinds of Derivation Rules which are possible: all derived verbs must fit into the set of possible verb classes as characterized by the set of possible case frames that can be constructed with these available Case Relations. A corollary of this process is that almost without exception, all new derived verbs will enter a class which already contains some underived members. That is, the set of basic syntactically definable verb classes remains essentially constant, and each of the basic classes contains some underived charter members and some additional members derived from items in other classes.

As an example, instead of deriving a sentence such as 2) from 1) transformationally, the verb of sentence 2) can be derived from the verb of 1) in the lexicon:

- 1) John hit₁ his cane [PAT] against the fence [LOC]
- 2) John hit₂ the fence [PAT] with his cane [MNS]

The lexical derivation process which produces hit₂ from hit₁ associates the root {hit} with a new case frame. In effect, it 'promotes' the Locus to Patient, making it the perceptual center of the sentence, and the former Patient is 'demoted' to the more peripheral Means relation. This process of derivation then imposes a new perspective on the event of hitting by reinterpreting the situational goal of the action, coded in the first construction by the Locus case role, as the grammatical Patient of the derived verb, and thus obligatorily down-

grading the previous Patient, which encoded the undergoer of the action in the first construction, reinterpreting it in this instance as a manifestation of the Means case relation. Thus *cane* is perceived as the thing being moved through space in sentence 1) but the means of hitting in sentence 2), while *fence*, which acted as the goal of the motion of the cane in sentence 1), is viewed as the entity affected by hitting in sentence 2).

Verbal derivational rules such as this formalize the tendency in natural languages for verbs of one class to be reinterpreted analogically as verbs of some other class. Every verb root corresponds to a conceptualization of a particular external situation, and each case frame represents a perspective on a situation. Thus by deriving a root from one case frame class to another, we are putting the situation represented by the verb root into a new perspective. For example, the verb root {walk} refers to the use of legs to move an object through space. This root occurs as a charter member *walk*₁ of the intransitive location verb class, characterized by the case frame shown as 4), and this case frame imposes the perspective of a Patient located in or moving to or from an inner Locus (cf. Gruber 1965). When this verb root is derived into a transitive class characterized by the case frame shown as 5),

4) [+PAT], +[+LOC], -[+AGT]]

5) [+PAT], +[+LOC], +[+AGT]]

a new perspective is imposed on the situation characterized by the root, a perspective in which some external agency is causally involved in the motion of the Patient, as in (6) and 7):

6) The beast [PAT] walks₁ in the swamp [LOC] on dark nights.

7) Zargo walks₂ the beast [PAT] in the swamp [LOC] on dark nights.

This derivational approach can be applied in a formally simple and conceptually revealing way to the description of many other inter-sentence relationships involving distinctions of case-marking and transitivity, and in fact provides the basis for a neat typology of such relationships. All of the basic derivational processes involve changes in the case frame of a verb: case relations may be added, subtracted, and/or reinterpreted; and all the processes crucially involve the Patient case relation.

It would also be possible to look at the various derivational processes in terms of how they affect transitivity, but this would miss the point, since transitivity as such is not a crucial variable in any of the derivation rules, and whether or not transitivity is affected is a rather indirect consequence of what happens elsewhere. Thus what is essentially the same process may happen within either the transitive or intransitive verb class, as when Locus is reinterpreted as Patient and Patient as Means in examples 1) and 2). That is, the same Derivation Rule that accounts for the relationship between transitive verbs in 1) and 2) also accounts for the relationship between intransitive verbs as in 8) and 9):

8) Bees [PAT] were swarming₁ in the garden [LOC]

- 9) The garden [PAT] was swarming₂ with bees [MNS]

Looked at from the other side, derived transitive verbs can result from several very different processes, including case relation addition, as in 7) versus 6), or 'recentralization', the reinterpretation of some other role as Patient with a consequent 'demotion' of Patient to some other role, as in 11) versus 10):

- 10) The horse [PAT] jumped₁ over the log [LOC]
 11) The horse [AGT] jumped₂ the log [PAT]

Similarly, intransitivization is also not a unified process, and may result from the loss of different case relations, as in 12) versus 13), where the loss of an Agent allows the Patient to occur in subject position, and 12) versus 14), where the loss of the original Patient requires the reinterpretation of the old Agent as Patient in order to maintain the obligatory-Patient requirement:

- 12) Waldemar [AGT] cooked₁ the bigos [PAT] furiously.
 13) The bigos [PAT] cooked₂ furiously.
 14) Waldemar [PAT] cooked₃ furiously.

Passivization also fits into the category of intransitivization. The Agent is reinterpreted as Means, so the Patient automatically takes over the subject slot, as in 15) versus 11) and 16) versus 12):

- 15) The log [PAT] was jumped₃ (by the horse [MNS]).
 16) The bigos [PAT] was cooked₄ furiously (by Waldemar [MNS]).

Intransitive verbs can also result from partitivization, which involves the decentralization of Patient and the consequent reinterpretation of the old Agent as Patient, as in 17) and 18):

- 17) Rafael [AGT] chewed₁ the sugar cane [PAT] thoughtfully.
 18) Rafael [PAT] chewed₂ thoughtfully on the sugar cane [LOC].

Anti-passivization also fits into this pattern.

This lexical analysis has the following advantages over previous transformational-type analyses, in which an entire clause is derived from another clause:

i. It requires no ad hoc rule features to account for lexical gaps. Instead, the semi-productivity of the rules involved is shown to be merely a normal and universal characteristic of lexical derivational processes.

ii. This analysis automatically imposes a strong constraint on the class of possible verbs, and consequently on the class of verbal clauses.

iii. The lexibase analysis accounts for semantic differences between pairs such as 10) and 11) in terms of differences in case relations, whereas a pure relational account has nothing to say about such differences.

iv. It constrains the choice of grammatical relations available to downgraded Patient 'chômeurs' in terms of a fixed and limited case relation inventory, and thus,

v. It automatically explains their choice of case marker as an automatic consequence of general rules of case form—case relation mapping.

vi. Finally, the structure of lexical entries is radically simplified. Each verb has its own fairly simple characteristic distribution which maps onto a single semantic representation in an entirely straightforward way. This means of course that there will then be more separate but homophonous entries in the lexicon, but this merely makes explicit a property of the lexicon that is camouflaged in other approaches. This property accords with recent neurolinguistic studies showing that the human brain processes information in terms of a large number of simple configurations rather than a smaller number of more complex packages, and points up the empirical fallacy of the belief in 'the unity of the word'.

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Unmarked Values in Languages: the Example of Malecite

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Malecite is one of the Algonquian Indian languages. It is spoken in New Brunswick, Canada. When I began working on this language (in 1969), it was poorly recorded and hardly described in the linguistic literature. I noted down eleven volumes of texts in this language, published the first Malecite dictionary, and published several articles on questions of Malecite grammar.

The present study on "Unmarked Values..." is a result of many years of research on obviation, verbal enclitics, the use of the locative ending, the use of the inverse theme sign etc. So far, I presented five conference papers and published them as articles on Malecite syntax.

The ways to express first, second, third person, to mark or not mark tenses, plurality, comparative or superlative degree, subject-object relation, location etc., these and other phenomena are marked in many languages of the world, but not in all of them. While using the term "unmarked values", I am speaking relatively, contrasting Malecite to other, non-related languages.

In one of the nicely logical languages of the world, in Japanese, the plurality of nouns as well as verbs, the differences between first, second and third person are not at all or not necessarily distinguished by special forms. The comparative degree of adjectivals is marked, but not on the adjectivals themselves.

In English, only the third person singular is distinguished from the forms of the first or the second person, and this happens only in the paradigm of the present tense of the verbs. Even this small difference between forms is not there in the paradigm of the past tense, where the forms of all persons and numbers are the same. For some verbs, the English language does not discriminate between present or past (e.g. *put*, *cut*). Only the word order carries the difference between subject and object, and still English is clear and perfectly understandable.

Equal forms for nominative and accusative are common in many other major languages of the world; to mention a few examples: neutral noun and adjective forms in Latin, all nouns in French, the feminines and neutrals in German, the inanimate masculines and neutrals in Russian etc.

In Hungarian, possessed nouns can keep or drop the *t* ending of the accusative. In Finnish, there are several ways to mark the object: partitive, as well as marked and unmarked accusative.

As far as Malecite is concerned, the situation is basically the same: certain grammatical values are marked, others are not. The grammatical structure of Malecite (as well as that of other Algonquian languages) is very different

from those well known major languages of the world. Not the same things are marked or unmarked as in the well known languages, and if something is marked, it is marked on a different part of the sentence, not on the same part where one might expect it on the basis of other languages.

There are no cases in the inflection of Malecite nouns. How can they distinguish between subject and object, for instance? The context, mostly the form of the verb helps to understand who is acting on whom. If they say *n-tiyaa) nət skiitap* 'I said to this man', the *n* is the prefix of the first person; *tiyaa* is a transitive animate verb in the form of the non third person singular of the independent indicative: and the words *nət skiitap* 'this man' do not have any ending, they are uninflected forms of a pronoun and a noun. There is no formal indication here, that *nət skiitap* is the object of the sentence. In the same form, these two words could be the subject of another sentence.

There is a little device in the Malecite language, the inverse theme sign *ək*, which helps to clarify in many sentences who is acting on whom.¹⁾ If they say *kətamakitahaamək w cel* 'He (or she) even felt sorry for me', there is no separate word either for the subject or for the object in this sentence. But the sign *ək* in the verb *kətamakitahaamək w* indicates that the action happened the other way around. First person is the grammatical subject, but it means 'I felt sorry for him the other way around', in other words: 'he felt sorry for me'.

If two animate third persons are involved in the action, the Malecite language uses the obviative ending (*əl, l* for the singular).²⁾ This ending joins sometimes the verb, sometimes a noun functioning as the object, sometimes both the verb and the object, and sometimes several other words in the sentence. The obviative is used mostly with transitive animate or animate intransitive verbs, or if a third person possesses a third person. This is a very common ending in Malecite. There are sentences of five to six words, all of them with obviation. Strict rules can be worked out for the sentences with transitive animate verbs. In the sentences with animate intransitive verbs, it depends on psychological factors whether the obviation is marked in the sentences or not. If an Indian girl had a baby from a white man, they can say when they are seeing the newborn child: *eci-wapeeyit* 'He is so white.' There is no *l* ending in this version of the sentence. But if they say: *eci-wapeeyiilit* 'He is so white (as the other third person, i.e. his white father)', we have something in this sentence which is very unusual from the viewpoint of the well known languages of the world. The obviative ending refers here to the little boy's father. In the Malecite language, it is not necessary to refer to that person by using a noun or a pronoun, as most languages would do. It is enough to use an *l* ending somewhere in the ending complex of the verb, and it is clear to every speaker of the language what this ending indicates.

Obviation seems to be more important for the Malecite speaker, than to specify the subject of the sentence; all three words, namely: a pronoun, a noun and a transitive animate verb have the obviative ending, but the subject is not formally expressed by a separate word: *yoohtəl skitapeehsiisəl ali-amniihkwaacil*

'Around this little man, he went around him'.

In other sentences, neither the subject, nor the object is there in the sentence as a separate noun or pronoun. A single obviative ending on a transitive animate verb is enough to indicate that one animate third person is acting on another animate third person. E.g. *'tiyal-yakw* 'he said to him'.

The ending of the locative is *k* in Malecite. This is the only ending in this language, bringing out place, location. But even this is not necessarily there in all sentences, referring to place. If the location is expressed by a place name, the Malecite speaker would feel it to be redundancy to use any ending. He uses just the place name, without any *k* of the locative. I am quoting an example from my article "Les noms de lieux dans les phrases malécites"³⁾: *nit etotāpit/sowonhkāhsisk* 'Il vivait là, à Swan Creek'. 'He lived there, at Swan Creek'.

There are no tenses in the inflection of the Malecite verb. How do they express, anyway, that something is happening right now, happened in the past or will happen in the future?

In sentences where the verb does not show any indication of past, the subject can indicate, anyway, that the action happened in the past. The subject might be the name of a dead person. E.g. *elhkiilāk-yakw naaka/laaliw* 'He was a very big man, the late Lola'. In other cases, only the larger context, for instance the whole story indicates that something happened in the past. E.g. *yoohtel wālekiyil/teehpo peskowaacil* 'This, his breechclout, only (this) he wore'.

However, if the Malecite speaker wants to stress past, future, conditional etc., he might use a verbal enclitic.⁴⁾ This term of Algonquian linguistics wants to discriminate between enclitics and endings. The enclitic (*h*)*pān* is not the same as the suffix of past in other languages. It is more or less a stylistic device in Malecite. The enclitics do not necessarily join the verbs in Malecite sentences. However, the past enclitic can join only verbs, but its use is not compulsory to express past. It stresses the past, giving to it almost the sense of past perfect: *ma-na ceska itāmoowi maaceheehpān/kāsnaa peeciyeehpān* 'He did not even say whether she had gone away or whether she had come'.

Nobody can claim that one language is significantly better, more expressive, more economical than other languages. In all languages, there are economical and redundant expressions. In this paper, I showed a few phenomena where the Malecite language appeared to be less redundant than many of the well known languages of the world. A number of short and productive endings, such as the locative *k*, the obviative (*ə*)*l* and particularly the inverse theme sign (*ə*)*k* enable this language to use less words and express more than other languages are able to do.

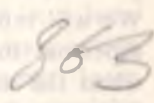
Even though several grammatical phenomena seem to be unmarked in this beautiful and flexible language, due to a few little devices the chances of misunderstanding are minimal in Malecite.

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2. László Szabó: "Obviation with AI Verbs in the Malecite Sentences", *Papers from the Third Annual Meeting of the Atlantic Provinces Linguistic Association* (Church Point: Université Ste. Anne, 1979), pp. 185-190.
3. László Szabó: "Les noms de lieux dans les phrases malécites", *Papers of the Tenth Algonquian Conference* (Ottawa: Carleton University, 1979), pp. 118-121.
4. László Szabó: "The Use of the Verbal Enclitics in Malecite", *Papers of the Twelfth Algonquian Conference* (Ottawa: Carleton University, 1981), pp. 106-113.

Multiple Argument Noun Phrases and Case in Japanese and English

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The controversy over the case markings of the arguments of Japanese verbals (Kuno 1973, 1978; Shibatani 1977; Tonoike 1975, 1979) concerns what grammatical function to assign to different NPs in certain pairs of sentences. Hypotheses made to explain recent psycho- and neurolinguistic findings (Bisazza 1980) regarding the processing complexity of derived nominals and related verbals in English and Japanese suggests unsuspected parallels between the two languages capable of providing evidence for the controversy over case in Japanese.

Consider the English sentence (1) in the list at the end of the paper. First, the position of the three arguments is fixed. Second, all of them are obligatory. In (2), containing an "ing" form, the same conditions obtain. We will refer to (2) as aspectual perfective gerunds, since they contain aspect. Note that in (2) the subject, which is obligatory, may be expressed as a possessive determiner. Next, note that both (3) and (4) exist. We will refer to (3) as aspectual non-perfective gerunds, and to (4)—containing "of"—as a nonaspectual gerund, since it can not contain "have." In (3) and (4) the subject is optional. Note that "of" can not appear in (2), or in (3) by definition. Also with nonaspectual gerunds, the subject can be placed after the verb and marked by "of" when the verb is intransitive, but only by "by" when the verb is transitive. Regarding order, the fixed positions do not seem to have altered, as seen in (5).

So far, we have introduced three main factors: order, "genitivization" (to include both English "'s" and "of") and optionality of arguments.

Next, consider the derived nominal expression (6) parallel to (4). First, the order of the constituents with the derived nominal is freer, as (7) and (8) show. Regarding genitivization, note that two things happen. First, the object can occur as a possessive determiner, which is *not* possible with any of the verbal forms. Second, if the object follows the derived nominal it *must* take "of." Finally, some of the arguments are optional (i.e., can be interpreted generically) with "placement," as (9)–(12) show.

There is a clear break between verbal forms in "ing" and derived nominals in that only the latter permit something other than the subject as a possessive determiner. Apparent counterexamples such as (13) are actually cases of derived nominals in "ing."

Finally, with derived nominals something other than the subject and the direct object can be a possessive determiner, as in (14).

Moving on to Japanese, consider (15). Notice that two arguments are

obligatory, although they may be (phonologically null) "zero pronouns." However, the relative order of arguments is free, in contrast to English. Next, consider the Japanese *koto/no* expressions in (16)–(17). As in English aspectual gerunds, the subject can be either nominative or genitive, and the verb is tensed. We will refer to (16)–(17) as aspectual gerunds since they can also contain the aspectual morpheme (*te-i*). Here again two arguments are obligatory, except when the aspectual gerund is nonperfective, in which case the subject can be interpreted generically. Next, consider (18), containing *kata* and *yoo* (roughly "way"), used with verbs, and *sa* (perhaps "degree"), used with adjectives and (less productively) with nominal adjectives. Notice that all arguments are now genitively marked and that the subject and object take *no* without intervening postpositions (=unmarked genitive). Note also that (18) are tenseless. Thus, these expressions are structurally quite parallel to the English nonaspectual gerund if we regard English "'s" and "of" as both corresponding to the single genitive marker *no* in Japanese. Therefore, we will also refer to (18) as nonaspectual gerunds since they can not contain the aspectual morpheme. The subject is optional in Japanese nonaspectual gerunds, as seen in (19)–(20).

Finally, in (15)–(18) *ut/ur* can be replaced by the Sino-Japanese verb *hanbai-si* with the same meaning. *Hanbai* is by itself an abstract noun ("selling") and is similar to derived nominals in English. This noun has optional arguments, as the grammaticality of (21) indicates. With adjective-based derived nominals—see (22)—the optionality of arguments is also striking. These independent nominals often incorporate the subject argument and mean "what is..." Their meaning is also often restricted to one particular usage of the corresponding adjective, as shown by the ungrammaticality of (23b). Of course, English derived nominals often show similar idiosyncratic qualities.

Let us summarize the parallels between the English and Japanese data.

First, although the order of arguments in Japanese sentences is free it becomes fixed with respect to the relative order of the subject and the object in nonaspectual gerunds, as case marking is lost due to genitivization. In English, the order of arguments is equally fixed in all three types of verbal constructions.

Second, in both languages genitivization can apply only to a subject in an aspectual gerund and must apply to both the subject and the object in a nonaspectual gerund. Japanese has one genitive marker: *no*. In English gerunds only the subject can ever be marked with "'s"; in nonaspectual gerunds both the subject and object can take "of," although not in the same phrase. The correspondence between English and Japanese (unmarked) genitive subjects and objects in nonaspectual gerunds is as follows.

Consider (24a). Of course, in English transitive gerunds the position of the arguments to the left or right of the verb signals their function as subject or object, respectively. Since Japanese sentences are verb final, the positions for the subject and object in nonaspectual gerunds (and nominal expressions with unmarked genitive arguments) are right and left (L and R in (24)) relative to

each other; when both subject and object are present, the former must precede the latter. In Japanese, the subject or object argument in a nonaspectual gerund may be a phonologically null, zero pronoun (with an antecedent in context) or may be absent but interpretable generically. In either case, the function (subject or object) of a single unmarked genitive argument in a Japanese transitive nonaspectual gerund can be determined by what its position relative to the absent argument would be if the latter were present. Interestingly, in the SVO order of English, the subject and object of a transitive gerund must occur in their L and R positions, respectively, even if one or the other of them is deleted or interpretable generically. One such case—with the subject absent—for English and Japanese is diagrammed in (24c), the empty squares standing for the only positions where a subject argument could be inserted.

The net effect of the facts described above for English and Japanese is to preserve an SO order—which, in the case of Japanese, is not obligatory when full case marking is present. The case of intransitive nonaspectual gerunds is shown in (24b) and (24d). In Japanese intransitive nonaspectual gerunds the L and R positions are unmarked, since there is only one unmarked genitive argument—the subject. Here, the interesting thing is that despite the fact that English is SVO the L and R positions are also unmarked in it for intransitive nonaspectual gerunds—as shown by the fact that the subject can occur with “of” after the gerund (see (24d)).

Finally, we have also noted parallels between English and Japanese with regard to optionality of arguments. We noted that in both English and Japanese aspectual nonperfective and nonaspectual gerunds subjects are optional, and that derived nominals can often be used without one or more of the obligatory arguments of their related verbs.

We turn now to some of the implications of the parallels discussed above.

Alongside (15), we have the paradigm of (25)–(26). The adjective *hosi-i* (“desirous”) is transitive at its deepest level of representation, as is indicated by the possible accusative marking of *hon* in (25a). The nominative marking of *hon* in (15b) can be ascribed to the generalization that the object of a stative adjective is so marked. This near obligatory nominative marking of the object of *hosi-i* suggests that at a fairly deep level the construction has undergone a restructuring where *hon* has become the subject of the sentential predicate and thus behaves like a nonderived subject. This explains the grammaticality of *hon no hosi-i koto/no*, an apparent counterexample to our claim that only a subject will genitivize with *koto/no*. The same line of reasoning explains the possible double genitive with *-ta-i* in *koto/no* complements, as in (28). However, in (26) with *omosiro-i* the accusative marking of *sono hon* is not possible, and *Mary* can be datively marked, which is not possible with *hosi-i*.

Kuno (1973, 1978) and Shibatani (1977) have proposed transitive analyses for (26). Tonoike (1975, 1979) treats *omosiro-i* as an intransitive adjective which can take an optional dative, as can English *interesting*. In this analysis, (26b)

is basic and (26a) is derived by a generalization of the rule of Subjectivization already available in Kuno (1973). The difference between the transitive analyses and the Dative Intransitive Analysis is as follows. In the former, *Mary* and *sono hon* are the subject and direct object, respectively; but in the latter *sono hon* is the subject and *Mary* is a dative argument.

According to our claims, if *Mary* were the *nonderived* subject in (26a) it should genitivize in both aspectual and nonaspectual gerunds, on the basis of the parallels discussed above. However, (27) are ungrammatical with the intended readings, in sharp contrast to the grammaticality of (18b). The ungrammaticality of (27) shows that *Mary* is not a *nonderived* subject. The grammaticality of (29), on the other hand, shows that *sono hon* is the *nonderived* subject.

A rejection of the Dative Intransitive Analysis (Tonoike 1975, 1979) would leave the facts regarding genitivization unexplained and would destroy a major part of the parallels between English and Japanese.

Note that (30b) is ungrammatical, since in a nonaspectual gerund only the subject and the direct object are genitivized and *tanosi-i* is an intransitive adjective like *omosi-ro-i* in Tonoike's analysis. (31a) with *tanosi-mi* is grammatical, since it is a derived nominal where arguments other than the subject and direct object can be genitivized (cf. 14)). *Tanosi-mi* has optional arguments as shown in (31b) and (31c).

Finally, the parallels between English and Japanese discussed above support Bisazza's (1980) claim—based on normal and aphasic data—that the processing complexity of lexical items in English and Japanese is a result of the minimum number of obligatory arguments required when they are used in sentences, nouns having fewer obligatory arguments than related verbs. These parallels show that, despite free word order in Japanese sentences and zero pronominalization, extensive parallels do exist between gerunds and derived nominals in the two languages—with a clear break between verbals (both sentence functors and gerunds) and nominals in both languages in terms of optionality of arguments and other factors. Thus, the psycho- and neurolinguistic data and the formal data in this regard are mutually corroborative.

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List of Examples

1. John placed the book on the shelf.
2. John/John's having placed the book on the shelf

3. John/John's placing the book on the shelf
4. John's placing of the book on the shelf
5. *John's placing on the shelf the book
6. John's placement of the book on the shelf
7. The book's placement on the shelf by John
8. The book's placement by John on the shelf
9. John's placement of the Go-stones was amateurish.
10. With the U. S. flag, the stars' placement is a matter of aesthetics.
11. The tall building's placement makes it vulnerable to earthquakes.
12. Placement/Arrangement is a major factor in good photography.
13. The poem's impassioned reading by its author stunned us.
14. Mary's refusal of a car by John
(ga=NOM; o=ACC; ni=DAT; e='to'; nitotte 'to/for')
15. a. John-ga (Mary-ni) hon-o ut-ta. "John sold a book (to Mary)"
b. Mary-ga hon-ga hosi-i. "Mary is desirous of a book"
16. John-ga/no (Mary-ni) hon-o ut-ta koto/no "John/John's having sold a book (to Mary)"
17. Mary-ga/no hon-ga hosi-i koto/no "Mary/Mary's being desirous of a book"
18. a. John-no (Mary-e-no) hon-no ur-i-kata/yoo "John's way of selling a book (to Mary)"
b. Mary-no hon-no hosi-sa "Mary's (degree-of) being desirous of a book"
19. Hon-no ur-i-kata/yoo "A way of selling books"
20. Hon-no hosi-sa "Desire for books"
21. Hanbai-no hiketu-wa aruk-u koto desu. "The secret of selling is walking"
22. a. Sono yama-ga taka-i. "The mountain is high"
b. Sono yama-no taka-mi "The mountain's high place(s)"
23. a. Sono nedan-ga taka-i. "The price is high"
b. *Sono nedan-no taka-mi "The price's high place(s)"
24. a)

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J. <div style="display: inline-block; vertical-align: middle;">NP-no Subj</div> ... <div style="display: inline-block; vertical-align: middle;">NP-no Obj</div> ... gerund	b) <table border="0" style="display: inline-table; vertical-align: top;"> <tr> <td style="text-align: center;">L</td> <td style="text-align: center;">R</td> </tr> <tr> <td><div style="display: inline-block; vertical-align: middle;">NP-no Subj</div> ... gerund</td> <td><div style="display: inline-block; vertical-align: middle;">NP-no Obj</div> ... gerund</td> </tr> <tr> <td style="text-align: center;">(Vt)</td> <td style="text-align: center;">(Vi)</td> </tr> </table>	L	R	<div style="display: inline-block; vertical-align: middle;">NP-no Subj</div> ... gerund	<div style="display: inline-block; vertical-align: middle;">NP-no Obj</div> ... gerund	(Vt)	(Vi)
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E. <div style="display: inline-block; vertical-align: middle;">NP's gerund</div> of NP ...	<div style="display: inline-block; vertical-align: middle;">NP's gerund</div> ...						
(Vt)	(Vi)						

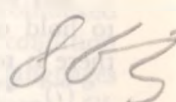
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J. <div style="display: inline-block; vertical-align: middle;">NP-no Obj</div> ... gerund	d) <table border="0" style="display: inline-table; vertical-align: top;"> <tr> <td style="text-align: center;">L</td> <td style="text-align: center;">R</td> </tr> <tr> <td><div style="display: inline-block; vertical-align: middle;">NP-no Subj</div> ... gerund</td> <td><div style="display: inline-block; vertical-align: middle;">NP-no Obj</div> ... gerund</td> </tr> <tr> <td style="text-align: center;">(Vt)</td> <td style="text-align: center;">(Vi)</td> </tr> </table>	L	R	<div style="display: inline-block; vertical-align: middle;">NP-no Subj</div> ... gerund	<div style="display: inline-block; vertical-align: middle;">NP-no Obj</div> ... gerund	(Vt)	(Vi)
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(Vt)	(Vi)						
25. a. Mary-ga hon-o hosi-i. "Mary is desirous of a book"
26. a. Mary-ga hon-ga hosi-i. "Mary is desirous of a book"
b. Mary-ga sono hon-ga omoiro-i. "Mary finds the book interesting"
27. a. *Mary-ga sono hon-ga omoiro-i. "The book is interesting (to Mary)"
b. *Mary-no sono hon-ga omoiro-i koto/no "The book's being interesting to Mary"
28. a. Mary-no sake-no nomi-ta-i koto/no "Mary's wanting to drink sake"
29. a. (Mary-ni) sono hon-no omoiro-i koto/no "The book's benign interesting (to Mary)"
b. (Mary-e-no/Mary-nitotte-no) sono hon-no omoiro-sa "The book's being interesting (to Mary)"

30. a. (Mary-nitotte-no) eega-no tanosi-sa "Movies' being enjoyable (to Mary)"
 b. *Mary-no tanosi-sa "•Being enjoyable to Mary"
31. a. Mary-no eega-no tanosi-mi "The thing that Mary enjoys about movies"
 b. Eega-no tanosi-mi wa iroiro ar-u. "Movies contain various enjoyable aspects"
 c. Zinsee-ni-wa tanosi-mi-ga iroiro ar-u. "Life contains many pleasures"

The Open Path Condition

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This paper is a brief presentation of the Open Path Condition (OPC; c.f. Yadurajan (1980)), an alternative to Subjacency. In section 1, we formulate the OPC as a condition on trace-binding, more generally, on *e*-binding. In section 2, we examine how it can be incorporated into the grammar, assuming the general framework of Chomsky (1981).

1. In what follows we make use of the notion "argument". We will say that in (1) the element extracted is an argument but in (2) it is the sub-part of an argument:

- (1) a. What did John buy
- b. Where did you see him
- (2) *What did they deplore the damage to

As can be seen, arguments are not restricted to base-generated NPs; nor are they limited to elements subcategorized by the Verb. (For a formal characterization of the notion *argument*, cf. Yadurajan, 1980.)

Consider now a case of long-distance extraction.

- (3) [\bar{S}_1 Who do you think [\bar{S}_2 [that John suspects
[that Sheila will marry t]]]]

The element extracted here is an argument of the verb *marry*; the maximal structure containing the trace ($=\bar{S}_3$) is an argument of the verb in \bar{S}_2 and \bar{S}_2 in turn is an argument of the verb in \bar{S}_1 . The relation obtaining between the \bar{S} s in (3) is a Successive Argument Relation (SAR), defined at (4):

- (4) The Successive Argument Relation (SAR) holds
of $\bar{S}_1 \bar{S}_2 \dots \bar{S}_n$ if, for \bar{S}_j ($j > 1$)
 \bar{S}_j is an argument of the Verb in \bar{S}_{j-1} .

A preliminary condition on the output of *wh*-extraction can be:

- (5) The element extracted must be an argument and if A is the set of \bar{S} s such that \bar{S}_j immediately dominates *wh* and for \bar{S}_k ($k \geq j$) if \bar{S}_k dominates t (t , the trace of *wh*) \bar{S}_k is in A , then SAR holds of A .

But this will immediately run into trouble with a case like (6):

- (6) [\bar{S} What do you wonder [\bar{S} who [t said t]]]]

We shall refer to (10) as the Open Path Condition (OPC).

We have been talking till now in terms of movement and the trace left under movement. One can as well think of a base-generated NP [*e*], coindexed by a rule of construal with its binder. We may then think of OPC as a condition on *e*-binding. Such an approach has a straight-forward application in languages where rules like Topicalization and Relativization involve no movement. Cf. Yadurajan (1980).

2. Recall now that at LF (and by the projection principle, at other levels, too) each R-expression is assigned a θ -role checked at LF. Necessarily the θ -roles in question are θ -roles assigned by a verb (in the case of V-complements) and by VP in the case of subjects (where required). We will assume that in the case of complements of adjectives, it is *be+adjective* which assigns a θ -role. The θ -roles assigned in this way, we shall designate $V.\theta$, to distinguish them from the θ -roles assigned by other categories (N, P).

θ -role assignment is supposed to depend on case assignment. But clausal complements, although they don't come under the case filter, must yet receive a θ -role since they are, presumably, R-expressions. Let's assume that a V assigns a θ -role to the \bar{S} (or S) it governs. When a θ -role is assigned to an S (e.g. *John believes [Mary to be honest]*) there is no problem. The interesting case is where a θ -role is assigned to an \bar{S} . Now we want the θ -role to percolate to S. That is, we want the θ -role assigned to \bar{S}_2 in (11) to percolate to S_1 .

- (11) [Who do you think [that [she will marry t]]]
 \bar{S}_1 \bar{S}_2 S_1

Further, we want this percolation to be blocked in a case like (12):

- (12) What do you know [who [t saw t]]
 \bar{S} S

and in cases like (13):

- (13) Who have you considered [the proposal [that [we should hire t]]]
 NP \bar{S} S

We can achieve all this if we assume that a θ -role assigned to X percolates to the lowest node dominating the lexical material under X. On the entirely natural assumption that the complementizer in (11) is not lexical, S will receive a $V.\theta$ -role. But in (12) and (13) the $V.\theta$ -role assigned to \bar{S} and NP respectively, will not percolate to the lower Ss.

Under the proposal informally sketched above, the element extracted (or, equivalently, the base-generated *e*) will receive a $V.\theta$ -role if it is an argument; and every S in the interval between the binder and *e* which dominates *e* will also receive a $V.\theta$ -role if there is an open path. That is, if the OPC is met there will be a chain of Ss in the interval between *e* and its binder all of which have been assigned a θ -role by their respective matrix verbs. We can now think of the Ss in the interval between the binder and *e*, dominating *e*, as a chain, whose

first member is the first S under the highest VP and the last member is *e* itself. Call this the *e*-chain, the chain which 'connects' *e* to its binder. We may now re-state the OPC as at (14):

- (14) Each link of the *e*-chain must be assigned a V.θ-role.
Viewed in this way, the OPC is a θ-criterion on \bar{A} -binding.

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Intentional Semantic Adaptations

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0.0. This paper will deal with intentional semantic adaptations in everyday situations; literary usages and figures of speech are, therefore, outside its scope. Intentional semantic adaptation is viewed as an active process that evaluates lexical items and grammar in terms of their communicative function. Pragmatically, language may be used for accuracy, simplification, confusion, or evasion (cf. Anwar 1981), in addition to other uses. Intentional semantic change, in this respect, may be an abstraction from reality. Although it is natural to expect the statement of an intentional mental state to specify its intentional object, in many cases these assertions may be about potential situations. I would like, therefore, to argue that any speech act with a propositional content contains a representation of some object or state of affairs. These representations are usually filtered through the impression one likes to leave on the addressee. This is so because sentences about intentional states are at least in part about representations. In light of this, their truth conditions will sometimes depend on features of—or even the existence of—the objects represented. We may expect, then, that the reference of a word may interact with its inferences where features from either area (i.e., reference or inference) may be shifted around, leading to an overloading or impoverishment of a lexical item.

The grammatical model that will be suggested in this paper is based on the following assumptions:

- a. Intentional change may occur in the basic or redundant features of a word.
- b. Since semantic features in a language form a relatively closed system, once a word undergoes semantic change, other words also undergo change to maintain semantic "balance" in the language.
- c. Semantic change affects not only individual words but also related words of one paradigm or related paradigms.

1.0. *The Nature and Aims of Intentional Semantic Change*

Intentional semantic change may have pragmatic parameters that can be accounted for in light of the mismatch between lexemes and their referents and/or inferences. There may be social situations under which referents—whether material or abstract—may change (cf. Anwar 1981). Examples are the use of gentlemen of the road for highwaymen, playing casinos for gambling casinos, etc. Bureaucratic uses of language are also another example. Under the influence of these uses words acquire certain meanings, so playing (in playing

casinos), in addition to innocent hobbies may also include business-like enterprises like gambling. This creates a certain expansion in the meaning of a word with the concomitant implications that go with it. So, a gambling casino is not licensed by the local municipality, but only if the name changes, it can be licensed although its activities remain the same. Under some circumstances, a new situation or object may come into existence and may require a name to label it. In this case, there may be the following possibilities:

a. Adapting a form in an analytic way, e.g., the use of *tele+vision* to identify televisions.

b. The use of arbitrary names such as *Kodak*.

c. Adapting an old form that may share certain features with the new object. For example, in Arabic when automobiles were used, the Arabic word used to designate them was *sayyāra*. This word originally meant a caravan. The feature of travelling common between these two objects, cars and caravans, allowed the word to be used in this new situation.

d. Characterizing a situation by some of its salient features. The 1974 San Francisco killings of blacks and whites were termed by the police "zebra killings."

The above phenomena raise the following issues: 1) Do extensions determine meanings, 2) Are the concepts associated with a term changeable depending on the facts true of the extension, and 3) Which parts of the semantic component are affected by intentional semantic change?

1.1. The first question is whether extensions determine meanings and how situational changes that affect our responses and understandings lead to lexical semantic changes. The fact is that the inferences of a certain form may combine with this form adding new meanings to it or making it lose others. The associations that go with words like *king*, *leader*, *socialist*, *guerrillas* may be intentionally twisted by the user under pragmatic considerations. This is why French socialism and Russian socialism are considered different. In view of this Dahlgren (1978) is wrong in claiming that "extensions determine meanings and not the other way around. If concepts (intensions) were more important in semantics than extensions, then we would expect that when the concepts associated with a term no longer applied to the members of its extension, then that term would be replaced by another to refer to the extension" (p. 62). In many cases when the associations that go with a term no longer apply to its extension, a new term is used. An example is the use of *selective service* instead of *drafting* or *conscription*. Another example is why certain names become popular or unpopular and may be used more or less than others depending on historical events. Such changes may be due to the lack of uniformity in understanding terms and to the effect of social circumstances. So, some countries talk about giving *facilities* to a superpower and not *bases* to avoid the anger of the masses. People also highlight certain connotations in different ways when talking about the same concepts: nationalist fighters may be called *freedom fighters*, *liberators*, *guerrillas*, or

terrorists depending on the prejudices of the speaker.

1.2. The second issue is whether the concepts associated with a term are changeable depending on the facts true of the extension. This is what Dahlgren (1978, p. 62) believes. However, the one-to-one correspondence between intension and extension may be true only when language is used to define or in cases where uniformity of understanding is required. It may not be true when language is used for implication or associative purposes. Moreover, the intensions and extensions true of one word may not be considered alone; related paradigms may be affected. When we talk about *success* or *white*, we also think of the opposite of these words: *failure* and *black*. The meaning of *cold* is also related to *cool*, *warm*, *hot*, etc.

1.3. The third question has to do with the semantic components that undergo the change. Voyles (1978, p. 110) believes that this change is mostly limited to semantic redundancy rules. This may be true of gradual change but in other cases non-redundancy rules may undergo the change (cf. the changes in *knave*, *knight*, and *meat*.)

2.0. The above three phenomena can be understood if we realize that the meanings of many words are mostly assigned to us by others rather than through direct experience. We talk about *Reaganomics*, *democracy*, *international relations*, etc., without knowing all the details that go with them. This is why change may occur in any of the semantic features of such words. Moreover, change is enhanced by the symbolic use of words to evoke in the listener certain distinctive representations of the things referred to. These representations utilize a code that highlights the properties of things and experiences that the language user deems important and can therefore, as Osgood (1978, 45) believes, be manipulated symbolically much more easily than the things themselves. Moreover, the pragmatic considerations of language use make such semantic changes possible. These considerations may be simplification, clarity (Stern, 1968, 283), confusion (as in politics), evasion (Anwar 1981), or emotive influences. Under such conditions, new uses may emerge. The word *discrimination*, which is negative in meaning, is not expected to combine with anything positive. However, in his report on racial troubles in London (*Time*, December 8, 1981), Lord Scarman recommends that the London Police Department hire more blacks on its force. He calls this *positive discrimination* (the equivalent American term is *affirmative action*). The combination of *positive* and *discrimination* is interesting in many ways. It deals with a would-be situation, i.e., its extension does not exist yet; however, its intension is at play here. It reverses the situation by implying discrimination against whites but positively in order to neutralize the negative intensions that go with discrimination.

3.0. The linguistic strategies employed to achieve the above aims may be one

of the following:

3.1. *The use of stereotypes.* Such prototypical forms do not allow for delimitation of reference. Things are done in the name of *democracy*, *national interest*, *security*, *justice*, etc., without making clear the exact reference of these words. Also under the domination of a certain atmosphere, the meaning of a word is generalized to cover many territories. When a military mood prevails in a country, the word *battle* may be used in contexts outside its military denotations, e.g., the *battle of peace*, *battle of land reform*, *battle of education*. This expansion of usage makes the word *battle* lose some of its impact on people. The word acquires extra "sense" but without clear "reference." This "multiple semantic situation," as called by Ullman (1967, 174) leads to fuzziness in understanding. The new term may "take over" and the other word in the expression may be deleted. The example given by Ullman is the use of *Burgundy* to label a certain type of wine: *Burgundy wine*. The word *wine* can be deleted and *Burgundy* can still be understood to refer to this type of wine. But deletability may not apply in every case, cf. *the capital (city)*, *capital *(fund)*, *capital *(letter)*, *capital *(punishment)*; also *Pan American (Airlines)*, *Pan American *(investments)*, *Pan American *(steel)*. It seems that deletability has something to do with the prototypical meaning of the word but not in every case. The word *drink* is used without an object to mean *wine drinking*. This is the acquired not prototypical meaning of the word.

3.2. Neutralization of features by using words with opposite features together such as *positive discrimination* (discussed earlier) or by using other words to refer to the same object or person. In some conservative countries, a person refers to his wife in the presence of strangers as *she*, *the children*, etc.

3.3. Semanticization of extra-semantic information, i.e., "the incorporation into the meaning of a lexical item of elements which before the incorporation in question, were associated with that meaning through inductive generalization" (Dik 1977, 283). Words like *Solidarity*, *Bader Meinhoff*, *P.L.O.*, have extra-semantic features as part of their meaning that were not there when these terms were first introduced. This extra-semantic information, as Dik says, is complementary in nature because when a specific meaning is applied in a situation, that meaning is singled out and complemented. For example, some countries add words such as *federal*, *democratic*, *socialist*, *united*, *popular* to their names to code extra-semantic features.

3.4. Desemanticization of extra-semantic features to exclude some of the connotations that go with a certain word. An example is the use of *chairperson* or *chair* instead of *chairman* to desemanticize any male domination. To desemanticize maleness in *boycott* one wonders whether one day we will have the words *womancott* and *girlcott*.

4.0. *Linguistic Implications of Intentional Semantic Adaptations*

This type of change manifests the relation between pragmatic and linguistic factors. The power of conviction of the new adaptations is compelling if they come from prestigious persons making assertions whose truth value cannot be readily tested. The cooperation of the listener and his readiness to believe the representation of a state of affairs that is asserted to exist contributes a great deal to the incorporation/exclusion of extra-semantic information. This is so because the physical fields of experience affected decide which words undergo the concomitant change once a word is "upset" semantically. This change may be productive in one area and not in the other. So, we may get *chair*, *chair-person* instead of *chairman* but not *womancott* or *girlcott* instead of *boycott* in a feminine context. This may happen though if the feminine context is expanded enough.

This analysis has implications for 1) learnability as the prototypical meaning of a lexeme may or may not be a prerequisite for understanding the "projected" one, 2) historical change, as it is wrong to claim, as Lass (1980) does, that linguistic change is not a consciously-purposive action, and 3) speech act theory because this teleological change makes languages maintain semantic balance once a paradigm is upset.

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Lexical Features of Linguistic Quantifiers and Well-Formedness of Logical Forms

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The approach I have assumed here is an attempt to account for all the well-formed distinct semantic readings of ambiguous quantified sentences by means of a lexical characterization of linguistic quantifiers and specifiers in terms of two formal features: *absoluteness* and *distributiveness*. Generally speaking, this approach is intended to be incorporated into the framework of a lexical grammar, along the lines of Bresnan (1978, 1981), Kaplan and Bresnan (1980) and others according to which lexical rules play an essential part of a theory of language and replace some basic transformations (e.g. NP movement) as well as several local constraints or filters (e.g. Chomsky and Lasnik (1977) and Chomsky (1978)). Bresnan herself has dealt with lexical forms, that is, predicate argument structures with specified grammatical functions, and was not concerned with quantifiers or specifiers so far. The lexical forms constitute forms analogous to open sentences, with slots for subcategorized arguments, whereas lexical forms together with linguistic quantifiers and specifiers make up schemata which can serve as proper objects for linguistic semantics (semantic interpretive rules for deriving consequences).

Robert May, in his work on the grammar of quantifiers (1977) has proposed quantifier movement rules (QR) which generate representations of Logical Forms of sentences with quantifiers and specifiers. The Condition on Quantifier Binding and the Subjacency Condition determine the well formed representation of Logical Forms, for the unmarked cases. He had, however, to invoke some additional highly idiosyncratic quantifier movement rules (QR'), in order to account for transparent readings in opaque contexts. Moreover, his theory predicts that "for any given clause S which contains n quantified noun phrases there are $n!$ (factorial) possible formally distinct well-formed logical forms which may be associated with it" (May, 1977, p. 33) (The stress is mine, for it is important to realize that there are many pragmatic and "morpho-lexical" factors which reduce the number of theoretically possible readings, but neither May nor myself take those factors into account when possible readings are discussed). It will be shown in the sequel, however, that by taking a lexical approach and by assigning the proposed features to all linguistic quantifiers, it is possible to establish certain general constraints which exclude some of those readings. That is to say, we obtain a smaller number of formally distinct well-formed logical forms which are theoretically (and practically) possible. On the other hand, in some cases the number of theoretically possible well-formed logical

forms is greater than that predicted by May's theory. In general, the number of well-formed logical forms depends solely on the number of the ambiguities characterizing only certain quantifiers with respect to the proposed two features and on the conditions that are applicable. It is only incidental that the correct number coincides with the factorial of quantified noun phrases in some examples discussed by May.

The proposed two features are called *absoluteness* and *distributiveness*. In terms of these two features, it is possible to characterize five classes of linguistic quantifiers (including specifiers):

(a)	(b)	(c)	(d)	(e)
all x-es: φ x	an x: φ x	the x: φ x	the x-es: φ x	many x: φ x
+ ab	+/- ab	+ ab	+ ab	+/- ab
+ dis			+/- dis	+/- dis

(Note that the generic use of the quantifiers in (c) and (d) is not taken into account here)

Class (a) includes: *each, every, any* and *all*

Class (b) includes: *a, one, some* (with an NP sg)

Class (c) includes: *the* (with NP sg), *this, that* and zero (with proper names)

Class (d) includes: *the* (with NP pl), *these, those, the entire group of*, etc.

Class (e) includes: *many, few, a few, several, the majority (minority) of, a group (bunch, set etc) of*, numerals etc.

Definitions. A quantifier α in a quantified noun phrase αX : φX (where the range of X is restricted by φ , the lexical content of NP) has the feature:

+*absolute* iff just one subset of φ can be referred to, the number of its elements (or relative size) being determined by α . For some quantifiers, it can be a unit set (e.g. *the, a*)

-*absolute* iff one or another subset of φ can be referred to, each having the same number of elements (or relative size) as determined by α .

+*distributive* iff the main predicate is evenly distributed, or applies in a uniform way to each value of X within the range determined by α .

-*distributive* iff the main predicate is not distributed evenly, or does not apply in a uniform way to each value of X within the range determined by α .

zero-distributive iff the main predicate is not distributed at all.

The feature +*distributive* is clearly the feature of a universal quantifier, whereas the feature +*absolute* is construed by analogy to what Copi (1977) calls the 'absolute' sense of an existential quantifier in the case when it precedes a universal quantifier (as opposed to the 'relative' sense of an existential quantifier in the case when it occurs within the scope of a universal quantifier). Notice that this feature is independent of whether a speaker has a definite referent (or referents) in mind or not nor does it depend on the context. It depends solely on the type of quantifier it can be assigned to and on the other

types of quantifiers that cooccur in the same sentence. Specificity could be described as *referential* (not descriptional!) *uniqueness*: at least one and at most one set of individuals is referred to, whenever a statement is made by the use of the corresponding sentence; only one set of individuals can make it true. *Non-absoluteness* can be described as referential non-uniqueness: any set of individuals as determined by α and restricted by φ can make the corresponding statement true. (Consider as an example: "Each man in our village respects two women", where the object phrase can be interpreted as $+$ or $-specific$.)

Most quantifiers could be two-ways ambiguous with respect to one or the other feature. The total number of combinations of features in quantified NPs in sentential structures (including the NPs in subcategorized sentential complements) is larger than the number of possible readings (and the corresponding well-formed Logical Forms). But there are some universal formal conditions which block certain impossible combinations (those would correspond to ill-formed Logical Form):¹⁾

Condition 1: For any quantifier α of the type $+/-ab$, if α is $-ab$ then α is $-dis$ or zero- dis (no quantifier is thus $-ab$; that is, the feature $+dis$ implies the feature $+ab$)

Condition 2: For any quantifier α , is $-ab$, then there is at least one quantifier $+dis$ cooccurring in the sentence (a quantifier $-ab$ must be in the scope of another quantifier $+dis$)²⁾

Condition 3: For any two quantifiers α, β cooccurring in a sentence, if α $+ab$ and β $+ab$ then β is $+ab$ $+0dis$.

Those three conditions block all the ill-formed Logical Forms and allow only those which are theoretically possible and correspond to distinct semantic readings.

I will present only one example, because of the lack of space. My claim is that given the above classification of quantifiers together with the three conditions, one can find an example for every theoretically predicted combination of features, and vice versa, for every example, there are no more distinct semantic readings than the feature method predicts.

Example: Everyone spoke to a minister on two problems

1) The number is further restricted by certain idiosyncratic properties of some verbs. Moreover, there are pragmatic factors, such as factual knowledge of the world, the context, etc. which reduce the number of readings, but these are immaterial for semantic well-formedness.

2) A quantifier $-ab$ can also occur within the scope of an opaque predicate (believe, want seek, etc). The lack of time does not allow me to discuss such cases. See. Bellert (in preparation)

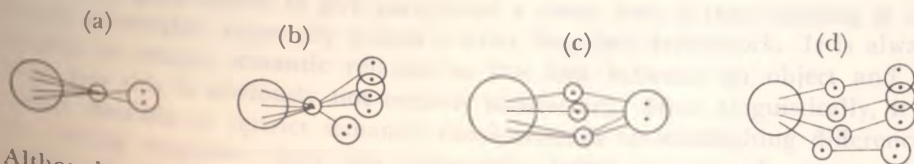
I. Bellert

$(\forall x: x \text{ is a person}) (\exists^1 y: y \text{ is a min}) (\exists^2 z: z \text{ is a pr}) z \text{ spoke to } y \text{ on } x$			
	$+ab$	$+/-ab$	$+/-ab$
	$+dis$		$+/-dis$
Four predicated readings:	(a) $+ab$	$+ab$	$+ab$
	$+dis$		$+dis$
	(everyone spoke to the same minister on the same two problems)		
	(b) $+ab$	$+$	$-ab$
	$+dis$		$-dis$
	(everyone spoke to the same minister on some or other two problems)		
(c)	$+ab$	$-ab$	$+ab$
	$+dis$		$+dis$
	(everyone spoke to one or other minister on the same two problems)		
(d)	$+ab$	$-ab$	$-ab$
	$+dis$		$-dis$
	(everyone spoke to one or other minister on some or other two problems)		

The corresponding Logical Forms with four distinct readings due to the only possible meaningful reordering of the three quantifiers would be:

- (a) $(\exists^1 y: y \text{ is a min}) (\exists^2 z: z \text{ is a pr}) (\forall x: x \text{ is a person}) x \text{ spoke to } y \text{ on } z$
 $+ab$ $+ab$ $+ab$
 $+dis$ $+dis$ $+dis$
- (b) $(\exists^1 y: y \text{ is a min}) (\forall x: x \text{ is a person}) (\exists^2 z: z \text{ is a pr}) x \text{ spoke to } y \text{ on } z$
 $+$ $+$ $-$
 $+d$ $-d$
- (c) $(\exists^2 z: z \text{ is a pr}) (\forall x: x \text{ is a person}) (\exists^1 y: y \text{ is a min}) x \text{ spoke to } y \text{ on } z$
 $+ab$ $+ab$ $-ab$
 $+dis$ $+dis$
- (d) $(\forall x: x \text{ is a person}) (\exists^1 y: y \text{ is a min}) (\exists^2 z: z \text{ is a pr}) x \text{ spoke to } y \text{ on } z$
 $+ab$ $-ab$ $-ab$
 $+dis$ $-dis$

The corresponding diagrams are:



Although the number of the possible combinations of features is eight, two combinations are excluded because of Condition 1, and two are excluded because of Condition 3. (Condition 2 is always met for the example.) The correct number of distinct readings, as predicted by the feature method, is four

(May's theory would incorrectly predict six).

The advantage of the feature method over a transformational approach with (Logical Forms derived by Movement Rules) is as follows:

- (1) Similarly to Bresnan's proposal concerning lexical forms, it makes it possible to dispense with a large number of transformational movement rules (this is especially important in the cases in which quantification is not Clause-bound, as for transparent readings in opaque contexts) so that the Core Grammar is simpler.
- (2) It correctly predicts the number of distinct semantic readings and correlates each with a well-formed Logical Form (construed by analogy to logical laws concerning the order of quantifiers)
- (3) It can be applied to distinguish between transparent/opaque readings by an extension of Condition 2 which allows the feature —*absolute* in the scope of opaque verbs (See footnote 2).
- (4) Last but not least, it constitutes a move towards a more realistic theory of language. The two features are cognitively simple (e.g. for each distinct reading in the above example there is a paraphrase in which they are explicitly expressed by the words 'each' and 'same', respectively). Thus every man in the street, without any knowledge of logic, may actually interpret each sentence with its quantified NPs along the lines suggested by the interpretive feature method.³⁾

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Formal and Non-Formal Conditions on Semantic Paraphrase

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The following is an attempt to give a workable account of semantic paraphrase and outline some of the conditions it has to satisfy.

Any semantic theory, in any sense of semantic, has to allocate some place to paraphrase, just as it has to offer some solution to the problem of idioms or make some statement regarding translation (Choul, 1980; Choul 1982)

In fact these problems are interrelated: the most plausible solution to the idiom problem is found in the use of paraphrase. An idiom does not seem to have a plausible semantic representation other than its paraphrase, except for oversophisticated devices. This does not mean that we should forego and discard all attempts at representing meaning through abstract or symbolic systems. Paraphrase does not compete with semantic representation, whatever its horizon. It nevertheless invites us to make use of the metalinguistic resources of language as a testing and control device, before venturing to construct a representation that will require a further interpretation, possibly a paraphrase, and requiring an elaborate transcoding system.

As Katz and Fodor (1963) pointed out, paraphrasing is basically an ability and skill of the speaker of a language, that is an ability to explicate what one means, with more or less success. A successful paraphrase may in that case depend on one's command of the language. But it is equally an activity of the hearer: there is no other way of proving one's understanding of an utterance, apart from performing whatever instructions it involves, than to paraphrase it. We could assume that paraphrasing is more than just a verification procedure, and is at the basis of understanding, but I'm not concerned with this speculative approach here, other than to suggest that paraphrase may be more than just a marginal aspect of language and probably requires more than ad hoc conditions.

Another good reason to give paraphrase a closer look is that meaning is not directly observable, especially within a strict linguistic framework. It is always possible to restrict semantic relation to the link between an object and its name, but this is obviously not entirely satisfactory. More linguistically, it is equally possible to restrict semantic considerations to establishing differences, but stating negatives does not necessarily bring us any closer to what meaning is.

Seeking identity or equivalence is probably a more rewarding enterprise. This will not necessarily imply that synonymy exists, but without stating the obvious, there is more in common between a pear and an orange and between

an umbrella and a parasol than between an orange and umbrella, although these might be connected in some field of experience or some situation.

The point here is that paraphrase and semantic analysis are interdependent, if paraphrase is what I am supposing: a complex semantic relation. As such it gains by being distinguished from definition as found in dictionaries, although many dictionaries make use of paraphrase in analysing certain items, especially idioms. It cannot be assimilated to encyclopedic descriptions of objects any more than semantic representation using features can. Nor can paraphrase be identified with devices such as meaning postulates or logical forms, these being confined to the metalanguage of description. Paraphrase combines a metalinguistic function with the relative autonomy of an utterance. If syntactic transforms may also combine these, they are to be isolated in view of their lexical material.

Paraphrase as a product of paraphrasing is analogous to synonyms: it can be substituted for the trigger element, whether a phrase, a clause or a sentence. But substitution is not the privilege of paraphrase and synonymy: anaphora and coreference exploit it as well.

A further step can be made in regard to the semantic nature of paraphrase if we decide to restrict transforms to an area of paraphrasing, or to a category of paraphrase. Past work on paraphrase in fact restricted it to sets of sentences reordering or superficially altering identical lexical material (Smaby 1971; Culioli 1976; Fuchs 1980).

Just as I proposed to distinguish the utterer's paraphrase from the hearer's paraphrase, syntactic manipulations on similar lexical material should be identified as formal or F-paraphrase.

To this category of paraphrase belong examples (1) to (10), borrowed from various authors:

- (1) Beavers build dams.
Dams are built by beavers.
- (2) The gardener watered the plants.
The plants were watered by the gardener.
- (3) The delegates visited the plants.
The plants were visited by the delegates.
- (4) J'aime le chocolat
C'est le chocolat que j'aime.
- (5) Il y a eu un débarquement anglais à Tripoli.
Tripoli a été le lieu d'un débarquement anglais.
Les Anglais ont débarqué à Tripoli.
Un débarquement anglais a eu lieu à Tripoli.
- (6) J'ai acheté l'auto à Paul.
Paul m'a vendu l'auto.
- (7) Pierre met du papier à fleurs sur le mur.
Pierre tapisse le mur de papier à fleurs.

- (8) Englebert collects stamps.
Englebert is a philatelist.
Englebert is a stamp collector.
- (9) The defects of the scheme were obvious.
The demerits of the plan were evident.
- (10) All the children might have been shouting at once.
The children all might have been shouting at once.
The children might all have been shouting at once.
The children might have all been shouting at once.
The children might have been all shouting at once.

These pairs or sets show various degrees of formal identity or similarity, just as the sentences in (11):

- (11) The police examined the bullet.
The bullet, the police examined.
The bullet is what the police examined.
The bullet was examined by the police.
It was the bullet that the police examined.
The police did examine the bullet.
The police did not examine the bullet.
Did the police examine the bullet?
Which bullet did the police examine?

As Smith and Wilson (1979), from whom these last two series are borrowed, indicate, relatedness does not amount to identity of meaning, and Chomsky (1977) is quite right in saying that the passive of example (1) is false, but it can still be a valid F-paraphrase under certain external conditions restricting its application (pragmatically involving "beaver activity", or with an implicit reference to "some"). Similarly, in (4) the strict semantic equivalence is conditioned by a question "qu'est-ce que tu aimes?" "What do you like?"

If relatedness is not sufficient, neither is formal identity: the plants in (2) are not the same as in (3). F-paraphrases can combine some lexical equivalence, derived or not, as in (6) and (8). (6) is usually referred to as converse, while (9) is a typical example of paraphrasing through a systematic synonymic substitution, where the formal character is confined to syntax, and (10) is strict formal reordering.

(5) is a typical paraphrastic set along the lines of Culioli's conception of paraphrase, but does not constitute valid semantic paraphrases or S-paraphrases, in the strict sense, since every formal difference is also a possible or potential semantic difference, analogous to the (11) series. They remain plausible choices with neutralisation, either for an utterer or a hearer, under certain conditions (frame of discourse).

External conditions (situation, reference, axiological or connotative purpose) will determine the extent of a paraphrastic set, as a set of sentences sharing a semantic core, with or without formal evidence. External conditions

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- (16) The head refused a raise. = The person in charge did not accept an increase in salary.

In (16) as in (9), borrowed from Leech (1974), we are dealing with the sentential type paraphrase, a type which is usually contested since it always seems a little contrived, but (17) still holds an S-paraphrase relation without this feeling:

- (17) The manager said no to his salary being upped.

The contrived appearance is then due to a one-to-one substitution on the basis of synonymy. This leads us to some operations carried out to produce an S-paraphrase. First, one has to realize that "paraphrastic synonyms" do not necessarily comply with the strict requirements of total synonymy. A basic operation then is a feature suspension (or neutralisation) which results from the application of a condition. Of course feature suspension is not restricted to word substitution in paraphrases, since syntagmation also blocks off readings, but the two phenomena are linked if MUSCULAR ORGAN cannot be assigned to *heart* in (18):

- (18) She has a kind heart.

It is connected with the fact that *nature* and *disposition* can be substituted for it. Here the verb *to have* and *kind* act as cooccurring conditions, while *to put*, *all* and *into something* would have commanded another reading.

Another operation is negation, single or correlative. (16) and (17) illustrate the single negation, and (19) the correlative (logical incompatibility):

- (19) *tiède*=*ni chaud ni froid*.

Negation of feature in the paraphrasing process does not happen alone: it obviously implies expansion, and expansion is due to feature raising, as the general paraphrasing process is: feature raising (as its opposite feature lowering) consists in a change of status—from the metalanguage plane (or analytical level) to the object language plane. This in turn implies that S-paraphrase is at least a ternary relation, but more generally a quaternary relation, since object language strings resulting from feature raising could have in turn a different reading although preserving the double redundancy. For instance, *said no* in (17) does not have the same reading as *refused* or *did not accept* in (16).

Deeper levels of analysis may show similar equivalence, but will not be translated into surface structures as readily as value-assignment which requires only a change in notation to change status.

The quaternary relation can be considered as a paraphrastic system, due to its interlocking capacity. While in a set of transforms meaning is said to be preserved, in an S-paraphrase system (part of larger sets, combining degrees of equivalence and types) the claim would be reversed: syntax is preserved through the double redundancy, and much of this is a projection of the hierarchy of features within a sememe, and between sememes.

Each system is sealed off through the application of conditions: in an S-paraphrase, reference has to be constant, as well as connotation or axiological conditions. This means that S-paraphrase sets could very well be restricted to systems made up of a small number of pairs, while an R-paraphrase set can be larger, without preserving semantic conditions, especially if proper names are involved, as in (12), (13) and (14). In line with this, axiological sets centered on pragmatics are probably the largest since the equivalence is one of purpose or theme, regulated by situational factors, with complete lack of redundancy at the reading level.

Paraphrases of world-objects, as *tree* for instance, are normally attainable through a reduction in features, where only the generic and one specific are raised. But this apparent difficulty is not confined to paraphrasing: a semantic representation of *tree* is bound to compete with a botanical description, just as one for a given emotion will borrow from psychology. If paraphrase becomes a dependable tool in semantic description, value-assignment will have an advantage over exhaustive feature listing.

To sum up, an S-paraphrase as a surface relation as illustrated in (20):

$$(20) \quad (a+b+d = e+f+g)C$$

is obtained through a quaternary interplane relation, as shown in (21):

$$(21) \quad \begin{array}{ll} a+b+d = & e+f+g \\ a:=E & e:=A \\ b:=F & f:=B \\ d:=G & g:=D \end{array}$$

displaying a double redundancy as in (22):

$$(22) \quad (a \cap b \cap d) \cap (e \cap f \cap g).$$

It would obviously have been elegant to arrive at a single formula, but the series (20), and (22) does have the merit, at least at this stage in the research, of illustrating the very complex semantic nature of the paraphrase relation.

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A Disseminational Analysis of Human Language Sentences Prolegomena to a Constitutional Theory of Language

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The difference between Eskimo and English is a minor one:

Savik nanineqāngila?

Has the knife not been found?

Hasn't the knife been found?

Eskimo is a non-disseminating language, whereas English belongs to the disseminating language type which splits up its verbals into nuclei (○) and operative verbs of different kinds (◇ (infinitives), △ (participles) and ▽ (absolutives, or gerunds)).

Our Eskimo example shows us that the polar categories "positive" and "negative" are by nature verbal or at least ad-verbal categories and that this capacity is shared by the sentence status category: the interrogation mark is strictly speaking superfluous in Eskimo, as the verb is in the interrogative mood.

English (German, French, Russian etc.) deploy(s) its (their) status and polarity notations. So a Basque sentence like

Etzuen batere erran bere anderari galdu zituela mandoak.

is matched by its English translation:

He has *not* even told his own wife *that* he has lost the mules.

in which latter sentence there is a special *word*(!) for 'not' and a special *word* for 'that' (corresponding to the verbal morph *-la* in Basque) indicating the neutrally sub-ordinated status.

These two categories, the materializations of status and the corresponding materializations of polarity, have a tendency to coalesce in human language,

thereby forming so-called *confusions*:

Minulla oli sellainen tunne, *ettemme* me itse olisi voineet joulua tehdä jollei se näin olisi saapunut... (Finnish)

("I had a feeling that we ourselves had not been able to celebrate Christmas as if he had not come like that").

Some languages differ maximally from Eskimo in that they split up their verbal hemispheres into minimal carriers of semasio-functional information. An example of this is Chinese:

我們非解放他不可

Wox-men fei chieh - fangz tha puz khex.*

we not let go let fall him not acceptable
(like that)

We have had to let him go.

Several languages tend to engender more or less complex verb corpora in the form of non-disseminating "molecules" leaving to their disseminating counterparts the only possibility of splitting up if you translate from one to the other:

iki taraf anlagamadılar.

(Turkish)

The two sides could not agree.

Bu böyle olmalıymış.

(Turkish)

Dies scheint so sein zu müssen.

Watashi wa hon ga yomemasu.

(Japanese)

[I the book is susceptible to reading]

= I can read the book.

* My Chinese transcription follows the principles proposed by Olov Bertil Anderson (Lund/Uppsala, Sweden).

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Énekelhetsz?

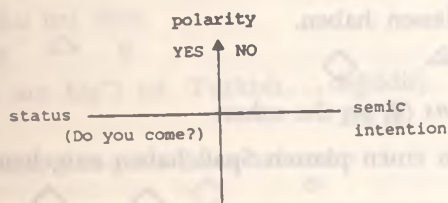
(Hungarian)

Can you sing?

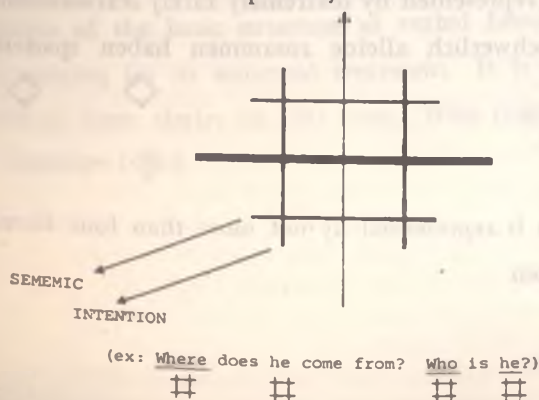
A universal constitutional linguistics operates with the following basic depiction of human language (the WORD of St. John's Gospel):



This figure allows us to define any human sentence as the sum of its status and its polarity:



or of its sememic intention and its polarity:



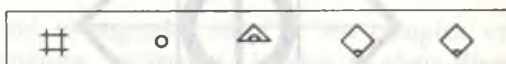
Moreover, it allows for a concise universal analysis of all linguistic categories seen as \diamond (=noun), \triangle (=adjective), ∇ (=adverb), \triangleleft (=finite adjective), \triangleright (=finite adverb), $\triangleleft\triangleright$ (=adjective, part of verbal complex), $\nabla\triangleright$ (=adverb, part of verbal complex), etc.

The central part of our figure (the \circ) gives us the key to the research of all human nuclei (traditionally they were called auxiliaries). Phonology, in this play, is reduced to its appropriate role: it is the mere printing ink.

So, if we look at a sentence like

Dies hätte untersucht werden müssen.

we see that it has the following structure:



in which sequence of simplest signs we introduce a \diamond indicating any infinitive which is a part of a periphrastic verb complex.

The verb structure just demonstrated is one of the 10 *quadrusections* existing in Modern German. They are matched on one side by *trisections* (8 in number):

Er muss sie verlassen haben.



and by *quincusections* (2) on the other:

Er soll sich kaum einen platten Spaß haben entgehen lassen können.



The maximum is represented by (extremely rare!) *sexcusections*:

Er wird sie schwerlich alleine zusammen haben spazieren gehen lassen



wollen können,

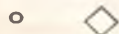


and the minimum is represented by not more than four *bisections*:

Er ist gekommen



Er hat anzurufen.



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Er wird gehen/gegangen.

Japanese is much simpler. The prevailing bisectional types:




Baka-na koto o itte-shimatta.

(= "I have said a foolish thing")

and trisectional types:

Yamada-san fusai ga kuruma de tsurete-itte-kuremasu.

(= "Mr. and Mrs. Yamada will take us by car")

differ from their German counterparts in their basic structures mainly on behalf of the frequent use of absolutives ( — ) and verbal adverbs ():

Kodomo ga asonde-imasu.

(= "the child is playing")

Tokaku hito no yo wa sumi-nikui.



(= it is not a very agreeable world to live in, this world of ours)

Kono kōjō wa ōkiku nai desu.

(= "this factory is not big") (cf. Turkish...değildir)

Gakusei de nakattara, han-gaku dewa arimasen

(= "If you are not a student, it is not half-price")

The whole problem of the basic structure of verbal hemispheres in human language is still waiting for its universal treatment. It is so enormously important to introduce more clarity on this issue: Who reigns the verb (), has conquered language ().

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Lexical and Conceptual Semantic Categories

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Two types of representation of the meaning of lexical items may be recognized. One is the predication sort, identified with the semantic representation of sentences, such as may be expressed in the forms of the predicate calculus, semantic case systems, or thematic relations. The other type is the representation of meaning of lexical items in terms of sets of usually binary valued features.

We show in this paper the formal distinctness of these two types of representation of meaning, and the relationship of their roles in a theory of semantic structure.

The two types of meaning representation relate to two levels. The one, involving predication relations, because of its direct association with the connection between the meanings of words and sentences, we call lexical. Categories elemental to this level we call lexical semantic categories. The other, involving sets of feature values, we call conceptual. These consist of matrices of conceptual features that define and distinguish the lexical categories.

Lexical and conceptual fields, utilizing a principle of disjunction, can thus be formally described and distinguished. In addition, a pervasive distinction in the character of meaning, as well as distinct syntactic effects, can be attributed to the nature of the two levels of meaning.

Verbs and adjectives tend to have lexical meaning, i.e. they are lexically complex, being defined by a predication structure. For example, a lexical field comprising the verbs *buy*, *sell*, *barter*, *borrow*, *lend*, *give*, *take* etc., is characterized by a single predication structure for each item in the field, each being distinguished from one another by substitution, addition, or interchange of categories or variables in distinctive roles within the predication. For instance, the role of Source in *sell* is identified with the same variable that plays the role of Agent and also serves as syntactic subject; whereas the role of Goal in *buy* is so identified. Both *buy* and *sell* involve a category MONEY, contrasting them with the other items in the field. The elemental lexical categories on this level, e.g., MONEY, would in turn be defined in terms of conceptual features, in a conceptual field contrasting items such as MONEY, GOODS, EFFECTS. Nouns, on the other hand, are often lexically simple, having meaning in terms of conceptual features only. For example, the conceptual field of the set of terms *cup*, *mug*, *glass*, *bowl* (i.e. lexical categories CUP, MUG, GLASS, BOWL, each term being lexically simple and corresponding to a single lexical category) is characterized by sets of values of con-

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ceptual features, such as Substance (earthen, glass, or plastic or paper), +|- Tall Shape, +|- Large Size, and +|- Having a Handle.

The conceptual features themselves may each be viewed as a predicate of a sort. In a lexical field, however, the items differ by varying the elements in a single predication, or modifying that relation. Whereas in a conceptual field the items differ by varying the values of each of the members of a set of conceptual (predicate-like) features.

An illuminating example is provided by the English temperature terms *hot*, *warm*, *cool*, *cold*, an analysis of which involves both lexical and conceptual levels. Like all polar adjectives (*big|small*, *high|low*, *good|bad*) a relational structure among various elements is needed to describe the terms and their opposition: each describes a measurement in some dimension, the same for each pair but in a positive direction in one term and in a negative direction in the other, in each case some distance from a relative norm. *Warm*, for instance, must at least be represented by the predication structure HAVING A TEMPERATURE WHICH GOES (EXTENDS|MEASURES) UPWARD FROM THE NORM, in contrast to *cool* and *cold*, which are measurements DOWNWARD. *Hot* is distinguished from *warm*, however, and *cold* from *cool*, in that *hot* and *cold* must be used to refer to temperatures only in specified ranges, viz., HOT and COLD, respectively. HOT and COLD would be elemental lexical categories; they are defined absolutely rather than relative to a norm, not involving such a predication structure. Thus we speak of a (relatively) cool star, but not a cold one (unless we mean a dead star); and we can speak of (relatively) warm liquid hydrogen, but not hot liquid hydrogen. Thus *hot* would have to be represented by a predication such as HAVING A HOT TEMPERATURE WHICH GOES UPWARD FROM THE NORM; *cold* would also involve a similar complex structure, using the categories COLD and DOWNWARD.

Schematically representing the possible permutations of the structural elements of the semantic representations here described, and mapping each possibility to one of the lexical items, constituting a lexical field, we have the following:

Lexical Field: Temperature Terms			
Semantic Representations		Lexical Items	Lexical Entries
TEMP. UP	FROM NORM, HOT	hot	TEMP, FROM NORM, HOT
TEMP.	FROM NORM, HOT		hot
TEMP.	FROM NORM	warm	TEMP, FROM NORM
TEMP. UP	FROM NORM		warm
TEMP.	FROM NORM, COLD		
TEMP. UP	FROM NORM, COLD	cool	TEMP, DOWN FROM NORM
TEMP. DOWN	FROM NORM		cool
TEMP. DOWN	FROM NORM, HOT	cold	TEMP, DOWN FROM NORM, COLD
TEMP. DOWN	FROM NORM, COLD		cold

By means of the principle of disjunction, whereby the specification of the use of one item delimits the use of a more generally specified item, the lexical

entries, which contain the minimal specifications, determine the full set of semantic representations corresponding to a given lexical item. Note that *warm* is the residual lexical item, having the least specification, but delimited in its use by that of all the others. *Cool* is also residual relative to *cold* and delimited by it.

The lexical categories HOT and COLD must now be defined in terms of conceptual features. The situation proves more complex than would allow HOT and COLD to be defined by a single feature, such as +Hot and -Hot, respectively. The range of use of HOT or COLD (and other categories, COLD' and ROOM, corresponding to *Room-Temperature*) appear to depend on additional conceptual features, such as whether referring to an object of the environment (+|- Object), and whether to an object used when hot (+|- Used Hot); the distinctions in range also seem to require three temperature features (+|- Hot, +|- Cold, and +|- Room). Thus the conceptual field, being a mapping from the conceptual space (defined by the possible permutations of conceptual feature values) to lexical categories, may be as complex as the following:

			<u>Conceptual Space</u>			<u>Lexical Categories</u>		
Object			-	+	+			
Used Hot			-	-	+			
Hot	Room	Cold						
+	-	-				_____	HOT	
+	+	-				_____	ROOM	
-	+	-				_____	COLD'	
-	+	+				_____	COLD	
-	-	+						

Such a mapping between a conceptual space and basic categories would be similar to that for basic colour terms, which themselves seem to be elemental lexical categories, defined by conceptual features.

It is also illustrative to display the conceptual field of a set of lexically simple terms, such as *cup*, *mug*, *glass*, *bowl*. This can be represented as follows (note that the feature Substance is three-valued in this analysis), where each is written above a matrix giving the feature combinations disjointly possible for it:

Conceptual Field: Drinking Vessels																							
	CUP												MUG				GLASS		BOWL				
Substance	g	g	g	e	e	e	p	p	p	p	p	p	g	g	e	e	e	g	g	g	p	e	
Tall Shape	-	-	-	-	-	-	-	-	-	+	+	+	+	+	+	+	+	+	+	-	-	-	
Handle	+	+	-	+	+	-	+	+	-	-	-	+	+	+	-	-	-	-	-	-	-	-	
Large Size	-	+	-	-	+	-	-	+	-	-	+	+	+	+	-	-	-	-	+	+	+	+	
g=glass												e=earthen						p=paper plastic					

g=glass

e=earthen

p=paper|plastic

Category Definitions

$\begin{bmatrix} + & \text{Vessel} \\ - & \text{Tall Shape} \\ - & \text{Handle} \\ + & \text{Large Size} \end{bmatrix}$	$\begin{bmatrix} + & \text{Vessel} \\ g & \text{Substance} \\ + & \text{Tall Shape} \\ - & \text{Handle} \end{bmatrix}$	$\begin{bmatrix} + & \text{Vessel} \\ g/c & \text{Substance} \\ + & \text{Tall Shape} \end{bmatrix}$	$\begin{bmatrix} + & \text{Vessel} \end{bmatrix}$
BOWL	GLASS	MUG	CUP

In the above the categories are minimally specified by category definitions using a principle of disjunctive delimitation. Thus CUP, the lexical category defining the term *cup*, is the residual category, having maximally general specification, but delimited by the specifications of all the others.

Note that although *cup* corresponds to a residual category it cannot be used as a general term in an unspecified situation. This is generally true for conceptually defined, i.e. lexically simple, terms. (For another example, note that among basic colour terms, also lexically simple, something pink, although technically a type of red, cannot be referred to as such.) This property of conceptual meaning is due to the role of conceptual features in defining lexical categories, whereas it is the latter that characterize what can be referred to, viz., distinctive areas in conceptual space. Words with lexical meaning left unspecified for some lexical category may however be used in a general unspecified situation, such as *warm* in 'How warm is it?'

The fact that it is the lexical categories of the semantic representation that characterize what can be referred to or stipulated, results in a pervasive distinction in meaning character attributable to lexical categories and conceptual features. Lexical categories are associated with an aspect of meaning that can be described as denoting a state that is actual or manifest; while conceptual features denote properties possibly not manifest, but potential or intentional and as such inherent and necessary. Thus a glass mug remains a mug, not a glass, if its handle is broken off. Also contrast *home*, with lexical meaning WHERE SOMEONE LIVES, with one of the senses of *house*, which is lexically simple with conceptual meaning characterizing something that is a building with the intention of being for someone to live in, viz., with the conceptual feature value + To Live in. Similarly, contrast *meat-eating* with *carnivorous* the one meaning one that actually does eat meat, the latter meaning one that potentially, and inherently, eats meat (presumably an animal). Similarly contrast the salient senses of *afraid* or *fearful* with *timid*, the former characterizing an actual state, possibly an habitual or generic one, the latter an inner property not necessarily manifest.

Since terms with lexical meaning directly involve the semantic representation of sentences, they are involved in sentence syntax, and aspects of the predicational relations that define them are accessible to syntax, resulting in syntactic effects, such as the presence of complements. Conceptual features, however, are not accessible to syntax. Thus contrast the fact that we can say *afraid of wolves*, but not **timid of animals*. In the same way parts of the

semantic representation of lexical meaning are accessible to modification in syntactic structures, such as in the phrase *temporary home*; however, parts of conceptual features are not so accessible, hence we cannot say *temporary house* in the same sense.

1.	Semantic Representations	Lexical Items	Lexical Entries
	SIZE, UP FROM NORM	big	SIZE, FROM NORM
	SIZE, FROM NORM		# big #
	SIZE, DOWN FROM NORM		SIZE, DOWN FROM NORM
		small	# small #
2.	Conceptual Space	Semantic Representations	Lexical Items
	HOT	[TEMP, HOT, UP FROM NORM	hot }
		[TEMP, HOT, DOWN FROM NORM	cool }
		(e.g., a star, molten metal, the Congo.)	
	NEUTRAL	[TEMP, NEUTR, UP FROM NORM	warm }
		[TEMP, NEUTR, DOWN FROM NORM	cool }
		(e.g., 70°F)	
	COLD	[TEMP, COLD, UP FROM NORM	warm }
		[TEMP, COLD, DOWN FROM NORM	cold }
		(e.g., liquid hydrogen, Siberia.)	
3.	Lexical Field: Temperature Terms		
	Semantic Representations	Lexical Items	Lexical Entries
	TEMP, HOT, UP FROM NORM	hot	TEMP, HOT, FROM NORM
	TEMP, HOT, FROM NORM		# hot #
	TEMP, FROM NORM	warm	TEMP, FROM NORM
	TEMP, UP FROM NORM		# warm #
	TEMP, COLD, FROM NORM		
	TEMP, COLD, UP FROM NORM	cool	TEMP, DOWN FROM NORM
	TEMP, DOWN FROM NORM		# cool #
	TEMP, HOT, DOWN FROM NORM	cold	TEMP, COLD, DOWN FROM NORM
	TEMP, COLD, DOWN FROM NORM		# cold #

4. Conceptual Field: Temperature Categories			Lexical Categories
Conceptual Space			
Object	—	+	
Used Hot	—	+	
Hot Room Cold	—	+	
+	—	—	HOT
+	+	—	ROOM
—	+	—	COLD'
—	+	+	
—	—	+	COLD

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Category Definitions: Temperature Categories

- 5.
- | | | | |
|--|--|---|---|
| $\begin{bmatrix} + \text{Temp} \\ + \text{Hot} \\ \{- \text{Room} \} \\ \{- \text{Object} \} \end{bmatrix}$ <p>HOT</p> | $\begin{bmatrix} + \text{Temp} \\ + \text{Cold} \\ \{- \text{Room} \} \\ \{- \text{Object} \} \end{bmatrix}$ <p>COLD</p> | $\begin{bmatrix} + \text{Temp} \\ - \text{Hot} \\ + \text{Used Hot} \end{bmatrix}$ <p>COLD'</p> | $\begin{bmatrix} + \text{Temp} \end{bmatrix}$ <p>ROOM</p> |
|--|--|---|---|

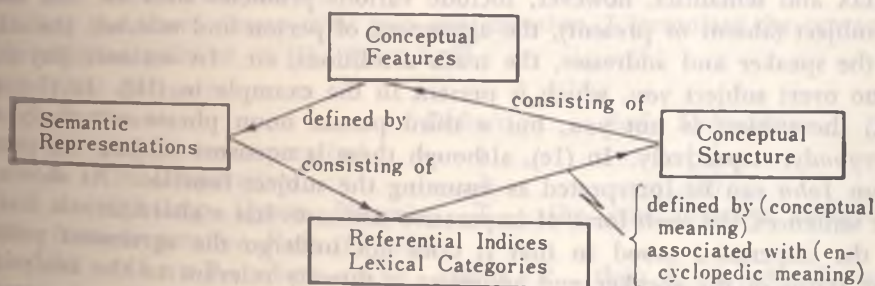
Conceptual Field: Drinking Vessels

- 5.
- | | CUP | | | | | | | | | | | | MUG | | | | | | GLASS | | BOWL | | |
|------------|-----|---|---|---|---|---|---|---|---|---|---|---|-----|---|---|---|---|---|-------|---|------|---|---|
| Substance | g | g | g | e | e | e | p | p | p | p | p | p | g | g | e | e | e | e | g | g | g | p | e |
| Tall Shape | - | - | - | - | - | - | - | - | - | + | + | + | + | + | + | + | + | + | + | + | - | - | - |
| Handle | + | + | - | + | + | - | + | + | - | - | + | + | - | - | + | + | - | - | - | - | - | - | - |
| Large Size | - | + | - | - | + | - | - | + | - | - | + | + | - | + | - | + | + | - | - | + | + | + | + |

Category Definitions: Drinking Vessels

- 6.
- | | | | |
|--|---|--|--|
| $\begin{bmatrix} + \text{Vessel} \\ - \text{Tall Shape} \\ - \text{Handle} \\ + \text{Large Size} \end{bmatrix}$ <p>BOWL</p> | $\begin{bmatrix} + \text{Vessel} \\ g \text{ Substance} \\ + \text{Tall Shape} \\ - \text{Handle} \end{bmatrix}$ <p>GLASS</p> | $\begin{bmatrix} + \text{Vessel} \\ g \text{ Substance} \\ + \text{Tall Shape} \end{bmatrix}$ <p>MUG</p> | $\begin{bmatrix} + \text{Vessel} \end{bmatrix}$ <p>CUP</p> |
|--|---|--|--|

7.



Syntax and Semantics of Imperative Sentences

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This paper attempts to provide an explicit syntax and semantics of imperative sentences in English. The category of imperative sentences includes the following:

- (1) a. Shut the door!
- b. You shut the door!
- c. Somebody open the door!
- d. Everybody shut his/their eyes!
- e. John, open the door!
- f. Anyone (of you) lend a hand and make himself/yourself useful here!

All the sentences in (1) are assumed to be used as imperatives. Their syntax and semantics, however, include various problems such as: the status of subject (absent or present), the agreement of person and number, the status of the speaker and addressee, the truth conditions, etc. In sentence (1a) there is no overt subject *you*, which is present in the example in (1b). In (1c) and (1d) the subject is not *you*, but a third person noun phrase *somebody* and *everybody*, respectively. In (1e), although there is no overt subject, the proper noun *John* can be interpreted as assuming the subject function. As shown in the sentences, the verb form of imperative sentences has a characteristic feature of the imperative mood in that it does not undergo the agreement process. The status of the speaker and addressee is directly relevant to the analysis to be presented. Finally, can we ever determine the truth value of an imperative sentence? If yes, what would be its truth conditions?

In this paper, an attempt is made to explain the sentences in (1) in a uniform way. In order to account for the observed problems and formally represent the semantic characteristics of imperative sentences, I adopt a revised and extended version of Richard Montague's (1974: "The Proper Treatment of Quantification in Ordinary English": PTQ, henceforth) model theoretic framework sketched and applied in such works as Bennett (1975), Karttunen & Peters (1975, 1979), and Lee (1977, 1979/80, & 1982). Syntactically, Montague's convenient bottom-to-top derivation and quantification rules are assumed. Semantically, I make use of Montague's translations and meaning postulates, plus the notion of (conventional) implicature. Particularly, making use of the two-phrase translation method (i.e., expressed and implicated meanings: Karttunen & Peters 1979; Lee 1977), I attempt to correctly represent the semantic aspects of imperative sentences.

As for the relationship between a syntactic form and its translation, I assume that any semantic aspects carried or created by a lexical item or by a syntactic form (i.e., structure or feature) can be represented in the translation of the relevant item or form. For example, the translation of the imperative sentence in (1a) will include the semantic aspects roughly corresponding to the structures in (2) and (3).

(2) You will shut the door.

(3) I demand you to shut the door.

In other words, the translation in my analysis includes the following points: First, it is usually observed (e.g., A. Ross 1944:55-57) that an imperative sentence denotes the comparable meaning of a declarative sentence. It will be convenient to regard this meaning of an imperative sentence as comparable to Lewis's 'sentence radical' (Lewis 1976:38) or to Bierwisch's 'propositional content' (Bierwisch 1980:22). Secondly, logicians and linguists observe that imperatives include the speaker's will or intention. (Leonard 1959:184; Bierwisch 1980:21 f.). I regard this aspect of an imperative as a part of its implicated meaning, which arises from the significant syntactic structure of the imperative sentences. In order to formally represent the two aspects of meaning, I use the two phrase translation method proposed by Karttunen and Peters. (Cf. works cited above.)

Taking the above observations into consideration, I formulate the syntactic and translation rules as in (4), (5), and (6).

(4) *New Categories*

Name	Definition	Basic Expressions
a. PI (Proto-imperative)	t	{ Λ }
b. IM (Imperative)	t	{ Λ }
c. IQP (Imp. Quant. Phrase)	T	{ <i>he, you, someone, everyone</i> }

(5) *Proto-Imperative Rule (PI-rule)*

S4': If $\alpha \in P_T$, where $\alpha = he_0$, and $\beta \in P_{IV}$, then $F_{\alpha}(\alpha, \beta) \in P_{PI}$ and $F_{\alpha}(\alpha, \beta) = \alpha\beta!$

(he_0 is an unspecified pro-form, which is phonetically null.)

T4': If α translates into $\langle \alpha^0; \alpha^1 \rangle$ and β into $\langle \beta^0; \beta^1 \rangle$, then $F_{\alpha}(\alpha, \beta)$ translates into $\langle \alpha^0 (\wedge \lambda y F\beta^0(y)); \alpha^1 (\wedge \beta^0) \wedge \alpha^2 (\wedge \lambda z Vx$
[human⁰(x) $\wedge \Gamma_2\{x\} \wedge \Gamma_1\{z\} \wedge \text{want}^0(x, \wedge F\beta^0(z)) \rangle \rangle$

where:

Γ_1 : property of 'being the addressee' (of type $\langle s, \langle e, t \rangle \rangle$)

Γ_2 : property of 'being the speaker' (of type $\langle s, \langle e, t \rangle \rangle$)

F: future time operator (of type $\langle t, t \rangle$)

want: a non-logical constant (translating want)

(6) *Imperative Quantification Rule (IQ-rule)*

S15': If $\phi! \in P_{PI}$, where $\phi!$ comes from F_{α} , and $\alpha \in P_{IQP(=)}$, then, $F_{15',0}(\alpha, \phi!) \in P_I$, where $F_{15',0}(\alpha, \phi!) = \phi!'$, where $\phi!'$ comes from $\phi!$ ($\in P_{PI}$) by replacing he_0 is $\phi!$ by α .

T15': If α translates into $\langle \alpha^0, \alpha^1 \rangle$ and $\phi!$ into $\langle \phi!^0; \phi!^1 \rangle$, then,

$F_{13',0}(\alpha, \phi!)$ translates into $\langle \alpha^* (\lambda x_0 \phi!^e); \alpha^! (\lambda x_0 \phi!^e) \wedge \alpha^h (\lambda x_0 \phi!^!) \rangle$

Let me now illustrate the above rules with a simple imperative sentence in (7). Before this sentence is derived, we need to derive a proto-imperative sentence as in (8) and translate it as in (9).

(7) Come!

(8) $he_0\text{-come!}, \text{PI}, 4'$

he_0, T $come, IV$

(9) a. $he_0' = \langle \lambda PP \{x_0\}; \lambda P [x=x] \rangle$
 $he_0^h = \lambda PP \{x_0\}$

b. $he_0\text{-come!}^e$
 $= he_0^e (\wedge \lambda y F \text{come}^e(y))$
 $\equiv \lambda PP \{x_0\} (\wedge \lambda y F \text{come}^e(y))$
 $\equiv F \text{come}^e(x_0)$

c. $he_0\text{-come!}^!$
 $= \lambda P [x=x] (\wedge \text{come}^e) \wedge \lambda PP \{x_0\} (\wedge \lambda Vx [\text{human}^e(x) > \Gamma_2\{x\} \wedge \Gamma_1\{x\} \wedge \text{want}^e(x, \wedge F \text{come}^e(z))])$
 $\equiv Vx [\text{human}^e(x) \wedge \Gamma_2\{x\} \wedge \Gamma_1\{x_0\} \wedge \text{want}^e(x, \wedge F \text{come}^e(x_0))]$

Now the actual imperative in (7) is derived by the imperative quantification rule as in (10) and translated as in (11).

(10) $he, \text{come!}, \text{IM}$

$he_1, T(=IQP)$ $he_0\text{-come!}, \text{PI}$

(11) a. $he_1' (\in IQP (=T))$ (he_1^h is the same as he_1')
 $= \langle \lambda PVx_1 [\text{human}^e(x_1) \wedge P\{x_1\}]; \lambda PVx_1 [\text{human}^e(x_1) \wedge \Gamma_1\{x_1\}] \rangle$

b. $he_1\text{-come!}^e = \lambda PVx_1 [\text{human}^e(x_1) \wedge P\{x_1\}] (\wedge \lambda x_0 F \text{come}^e(x_0))$
 $\equiv Vx_1 [\text{human}^e(x_1) \wedge F \text{come}^e(x_1)]$

c. $he_1\text{-come!}^! = \lambda PVx_1 [\text{human}^e(x_1) \wedge \Gamma_1\{x_1\}] (\wedge F \text{come}^e(x_0)) \wedge \lambda PVx_1 [\text{human}^e(x_1) \wedge P\{x_1\}] (\wedge \lambda x_0 Vx [\text{human}^e(x) \wedge \Gamma_2\{x\} \wedge \Gamma_1\{x_0\} \wedge \text{want}^e(x, \wedge F \text{come}^e(x_0))])$
 $\equiv Vx_1 [\text{human}^e(x_1) \wedge \Gamma_1\{x_1\} \wedge Vx_1 [\text{human}^e(x_1) \wedge Vx [\text{human}^e(x) \wedge \Gamma_2\{x\} \wedge \Gamma_1\{x_1\} \wedge \text{want}^e(x, \wedge F \text{come}^e(x_1))]]]$

In the translation we actually need to specify the implicature of the verb *come* in order to get a complete translation. This part, however, is not crucial in illustrating the rules, hence assumed and omitted.

The postulated rules, together with the regular quantification rule in PTQ, can effectively account for the ambiguity arising from the sentence in (12), which is derived as in (13).

(12) Everybody choose a subject!

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Semantic Relationship between Subject and Object

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SSO

There are at least eleven roles recognizable in English simplex sentences: (1) Agentive (AG), (2) Affected (AF), (3) Effected (EF), (4) Recipient (RC), (5) Instrumental (IS), (6) Locative (LC), (7) Temporal (TP), (8) Eventive (EV), (9) Empty "it" (IT), (10) Current Attributive (CA) and (11) Resulting Attributive (RA). The subject may assume 1. AG, 2. AF, 3. RC, 4. IS, 5. LC, 6. TP, 7. EV and 8. IT. The object is supposed to play such roles as 1. AF, 2. EF, 3. RC, 4. IS, 5. LC and IT.

Fillmore postulates for English the following rule: "If there is an A, it becomes the subject; otherwise, if there is an I, it becomes the subject; otherwise, the subject is the O". It is clear that the notion of a hierarchy of cases with respect to subject formation is inherited by Quirk et al and a system of priorities developed for assigning a function (role) to the subject: "If there is an 'agentive', it is S; if not, if there is an 'instrument', it is S; if not, if there is an 'affected', it is S; if not, if there is a 'temporal' or 'locative', it may be S; if not, the prop word *it* is S".

The theory of case hierarchy is plausible, but its application is dubious, for the assignment of a function to the subject is fundamentally decided by the properties of the verb and noun concerned, and the subject permutation will go with a case inheritance.

1. a. He dirtied the wall with oil
b. *He dirtied oil on the wall
2. a. Dumdum bullets killed a large number of enemies
b. *Dumdum bullets murdered a large number of enemies
3. a. I've found a place for the pine tree
b. *I've found the pine tree a place
4. a. He gave the room a thorough cleaning
b. *He gave a thorough cleaning to the room

Sentences 1b, 2b, 3b and 4b are ungrammatical because the properties of the verbs and nouns concerned do not semantically permit of those sequences. This is good evidence that relevant semantic information must be coded into lexicon, in particular into verbs, and the number of participants of the case frame is determinable from the verb used. In English there are a great number of multivocal verbs, each of which, when used in a particular sense, has a case frame specifying with what cases it can occur as its participants. Therefore it is possible to state the selectional constraints on the insertion of nouns

under the domination of different case frames, which in turn makes it possible to have a specified semantic reading of a particular sentence in terms of case grammar.

5. a. He tasted the soup
b. The soup tasted good
6. a. John had a car
b. John had supper
7. a. He made her his secretary
b. He made her a new dress

In 5a subject 'He' is AG and object 'the soup' AF, whereas in 5b subject 'the soup' is AF. In 6a subject 'John' is RC because of the nature of the verb 'have' (own, possess), but subject 'John' in 6b is AG, with 'have' meaning 'eat'. In 7 'his secretary' is RA, but 'a new dress' EF.

Topicalization does not necessarily mean subject permutation.

8. a. John hasn't read this novel
b. This novel John hasn't read
9. a. John smashed the window with a stone
b. A stone smashed the window
c. The window smashed (to pieces)
d. The window was smashed

In 8b 'this novel' is the theme of the sentence as a result of topicalization, but it is not the subject; it remains the object (AF). In 9c 'the window' is the subject as a result of subject permutation (a kind of topicalization), which has the same role (AF) as 'the window' in 9a, which is the object (AF). In 9d, 'the window' becomes the subject after passivization without changing its deep structure role (AF).

10. a. The teacher gave the pupil a book (simplex sentence)
b. The pupil was given a book (by the teacher)
(passive subject-permutation)
c. The pupil had (owned, possessed) a book
(parallel subject-permutation)

'Give' is a dative verb which takes an indirect object (RC) and a direct object (AF). The indirect object may be permuted to the subject position (as in 10b), preserving its recipient role, which may be called passive subject-permutation. Furthermore, it can be 'changed' to 10c by changing the verb to 'have' (own or possess), still preserving its recipient role, which may be called parallel subject-permutation.

11. a. Two books are on the desk
b. There are two books on the desk
c. The desk has two books on it

These sentences are synonymous in terms of semantic interpretation. In 11a 'two books' is the subject, having AF as it is associated with 'Someone has placed two books (AF) on the desk'. In 11b 'there' takes the subject position, functioning as formal subject, with 'two books' used as subject in AF, being associated with 11a. In 11c 'the desk' is LC, as related to 11b. These facts show that all the subjects have derived from the same common underlying structure in which 'two books,' has AF, and 'the desk' LC, and their functions (roles) are inherited by their respective surface functions through derivation—parallel subject-permutation.

We have noticed that in the derivation of sentences (or permutation of subjects), in cases where there are two or more participants in a case frame the subject is not determined by the rule of case hierarchy but by the rule of topicalization—a matter of which case is to be chosen as subject.

Sentence 9b is constructed not because there is not an agent, covert or overt, but because the speaker wants to make the noun with IS the theme; similarly, sentence 9c is constructed because the speaker wants to make the noun having AF prominent, but not because there is not a noun with IS. Nevertheless, the rule of case hierarchy is still valid in the semantic interpretation of the occurrence of a certain case (role) as subject, but it is by no means the factor for the choice of case (role), which topicalization certainly is, and the cause of topicalization must be the deciding factor.

The status of a 'surface subject' in a derived sentence through nominalization is determined by that in deep structure.

12. a. He is easy to please
b. It (someone pleases him) is easy
13. a. I am eager to please
b. I am eager for it (I please someone)
14. a. She is likely to come
b. It (she comes) is likely

In 12 'he' is AF, for its status in deep structure (the embedded sentence) is AF. In 13 'I' is AG as its status in deep structure is AG. In 14 'she' may play AG on condition that the action of coming is out of her own will. Thus the validity of case inheritance has been proved.

The foregoing arguments may be summed up as follows:

1. The case frame of a certain simplex sentence is determined by the properties of lexicon (verbs and nouns, particularly verbs). Within the case frame, subject permutation, either passive or parallel, may take place, and any of the participants may be permuted to subject position with certain constraints.
2. In the derivation of sentences, the status of a participant is inherited (or preserved) by the permuted subject in surface structure. This may be called the 'rule of case inheritance'.
3. The rule of case hierarchy is not the deciding factor that determines topicalization, but merely a superficial rule for explaining the priority order for

a participant to be chosen as subject in a fixed particular case frame. It is true that the subject rule (for subject placement) is sensitive to the rule of case hierarchy within the fixed number of participants in such a subcategorized particular frame.

The semantic relationships between subject and object may be approximately diagrammed as follows:

A. With AG as subject:

1. AG + V + AF (EF, RC, LC)
2. AG + V + AF + EF (CA, RA)
3. AG + V + EF + CA
4. AG + V + RC + AF (EF)
5. AG + V + AF (IS) + Prep + LC
6. AG + V + AF (LC) + Prep + IS

B. With roles other than AG as subject:

7. AF + V + EF
8. RC (IS, LC) + V + AF

The major problem confronting us is to decide a set of semantically well-defined cases (or roles) and provide some adequate syntactic criteria for ascertaining the decisive factors determining the choice of subject word in a given language—here the English language.

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Generating Dependency Structures of Fuzzy Word Meanings in Semantic Space

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This paper will report on one of the central objectives of a project in computational semantics which is supported by the Northrhine-Westphalia Ministry of Science and Research under grant IV A 2-FA 8600.

1. Modelling system structures of word meanings and/or world knowledge is to face the problem of their mutual and complex relatedness. Under the notion of semantic relevance and knowledge disposition this interdependency may empirically be reconstructable from natural language discourse although most approaches in linguistic semantics and artificial intelligence do not address these issues. Instead, linguists as well as experts engaged in word meaning and/or world knowledge representation still provide the necessary semantic or external world data introspectively by exploring their own competence and memory capacities to depict their findings in some semantic or conceptual structures (lists, arrays, networks, etc.). They do so with the understanding that their models may have a more or less ad hoc character and tend to lack—beyond their limited operational performance—intersubjective control. Other than these introspective explorations, the present approach strives to derive directly via automatic analysis of natural language discourse some basic data whose relational structure will not be declared but procedurally be defined by algorithms which induce it.

2. Based upon statistical means for the empirical analysis of discourse and for the formal representation of vague word meanings in natural language texts, procedures have been devised which allow for the systematic modelling of a fragment of the lexical structure constituted by the vocabulary employed in the texts as part of the concomitantly conveyed world knowledge concerned (RIEGER 1980). The coefficients applied will map lexical items onto fuzzy subsets of the vocabulary according to the numerically specified regularities these items have been used with in the discourse analysed. The resulting system of sets of fuzzy subsets is a datastructure which may be interpreted topologically as a hyperspace with a natural metric. Its linguistically labeled elements (representing meaning points) and their mutual distances (representing meaning differences) form discernable clouds and clusters which determine the labels' associative meaning relations. Thus, the analysing algorithm takes natural language texts from a certain subject domain as input and produces as output the distance-like datastructure (semantic space) of linguistically labeled elements (meaning points) whose positions represent essential properties of the

conceptual prototypes according to which their labels have been employed in the texts analysed (RIEGER 1981). Their varying dependencies which constitute a (latent) associative relational structure (RIEGER 1982b) may procedurally be defined and modelled on the semantic space data to allow not only for search and retrieval operations being executed but also for inferential processes being performed on that data structure under different aspects of semantic contents and relevance.

3. Taking up ideas from the theory of semantic memory and spreading activation in cognitive psychology (COLLINS/LOFTUS 1975), a new algorithm is presented which operates on the semantic space data to generate—other than the CDS-procedure (RIEGER 1982a)—associative dependency structures (ADS) in the format of general (n-ary) trees. Given one meaning point's position being primed, the algorithm will first start to list all neighbouring points by increasing distances. Then, the algorithm's generic procedure will take the first on the list, determine its most adjacent point among those already primed, and identify it as its mother-node before deleting the new daughternode's label from the list. Repeated successively for each of the meaning points listed and in turn primed in accordance with this procedure, the algorithm of least distances will select a particular fragment of the relational structure latently inherent in the semantic space, depending on the aspect, i.e. the primed meaning

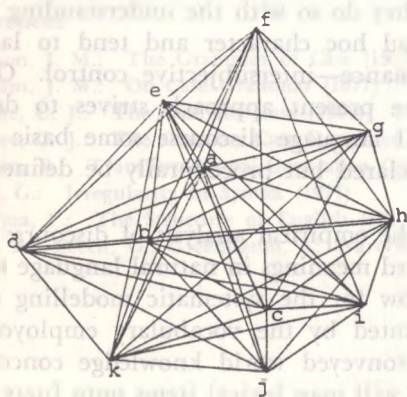


Fig. 1

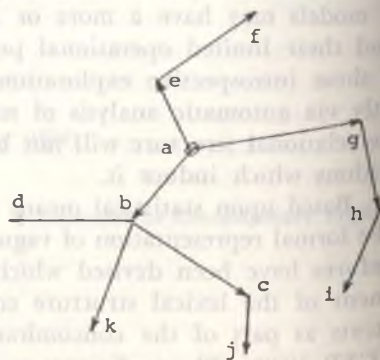


Fig. 2

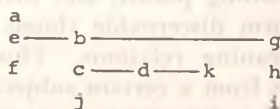
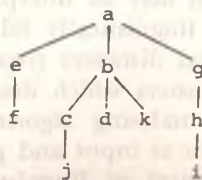


Fig. 3

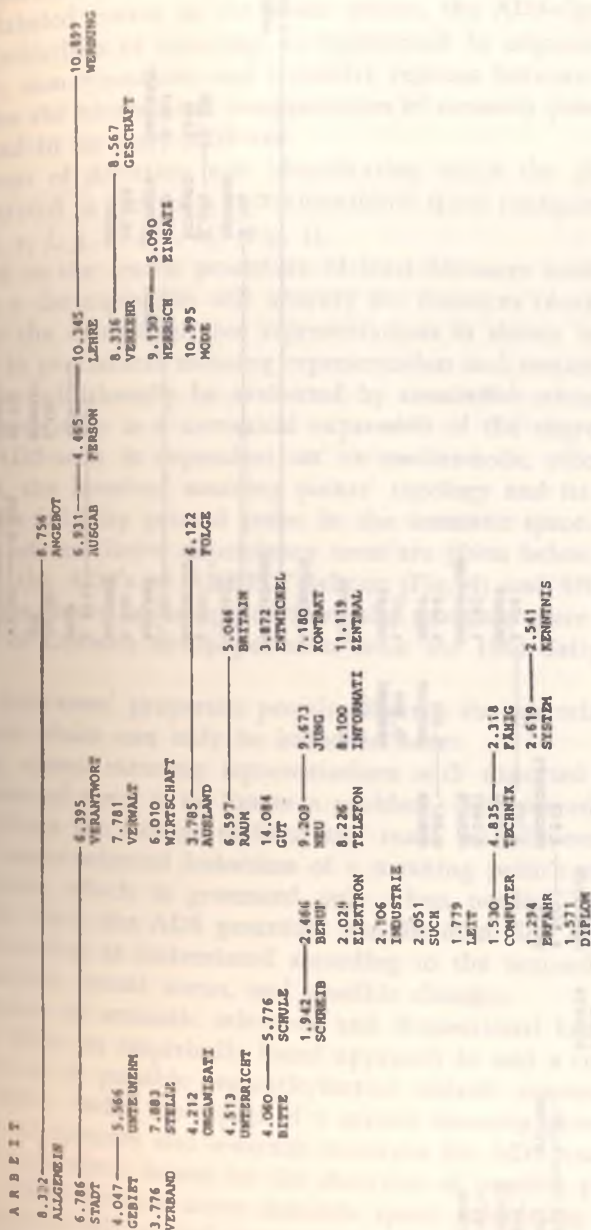


Fig. 4

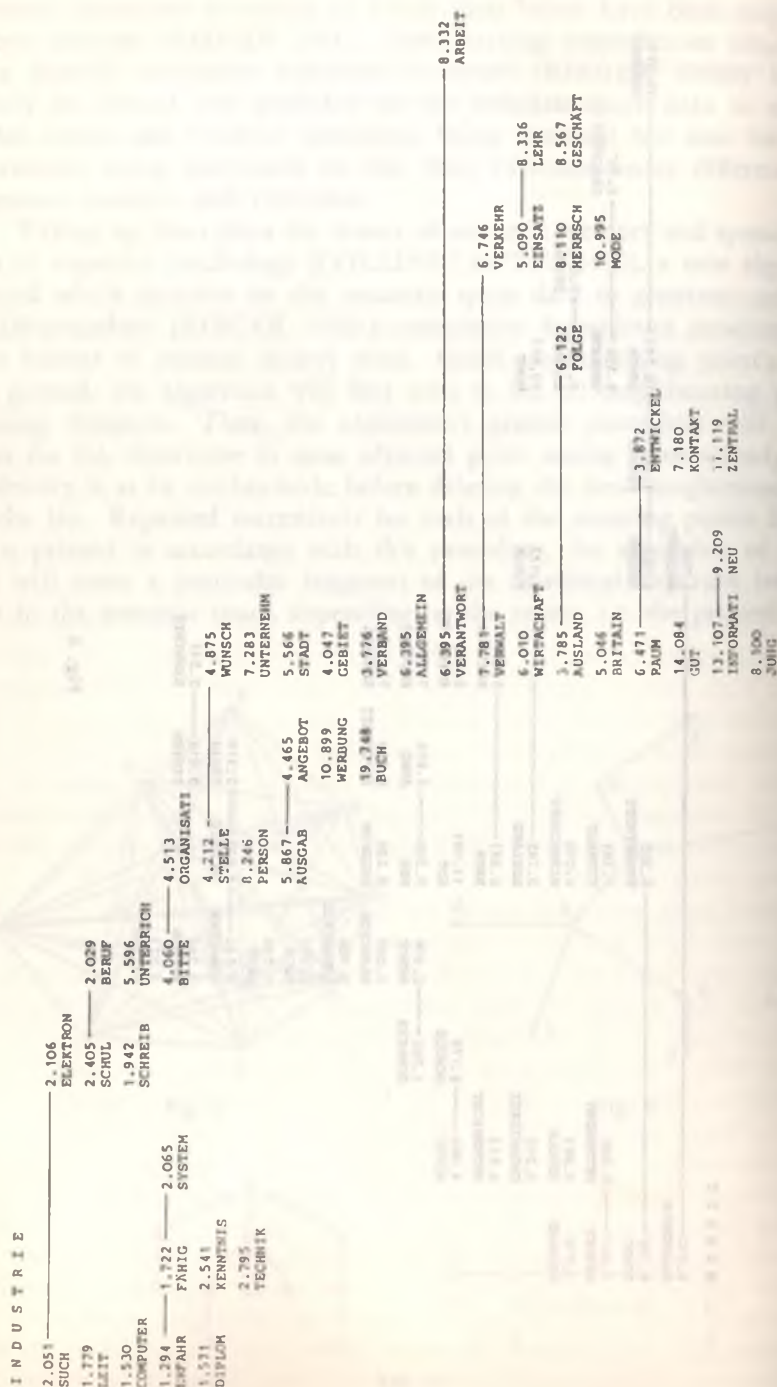


Fig. 5

point the algorithm is initially started with. Working its way through and consuming all labeled points in the space system, the ADS-algorithm transforms prevailing similarities of meanings as represented by adjacent points to establish a binary, non-symmetric, and transitive relation between them. This relation allows for the hierarchical reorganization of meaning points as nodes under a primed head in an n -ary ADS-tree.

The process of detection and identification which the algorithm performs may be illustrated in view of a two-dimensional space configuration of 11 points $\langle \delta\{a, b, c, d, e, f, g, h, i, j, k\} \rangle$ (Fig. 1).

Submitted to the search procedure of least distances under initial priming of the point a the algorithm will identify the distances concerned as in Fig. 2 and produce the equivalent tree representations as shown in Fig. 3. For the effective use in procedural meaning representation and semantic processing, the ADS-trees may additionally be evaluated by associative criterialities, not given here. The criteriality is a numerical expression of the degree or intensity by which any ADS-node is dependent on its mother-node, calculated as a function of both, the involved meaning points' topology and its relative distances leading to the initially primed point in the semantic space.

Examples of associative dependency trees are given below where the upper fragments of the ADS's of ARBEIT/labour (Fig. 4) and INDUSTRIE/industry (Fig. 5) are shown as computed from the semantic space structure derived of a sample of German newspaper texts from the 1964 daily editions of 'Die Welt'.

4. The ADS-trees' properties permit different though related model-bound interpretations which can only be indicated here:

- identifying stored meaning representations with distorted and/or modified instantiations of them is no longer a problem. The procedural semantic approach replaces the storage of fixed and ready set relational (semantic) networks by source-oriented induction of a meaning point's associative dependency structure which is generated only when needed. Triggered by any identifiable label, the ADS generated may be identified with that label's associative meaning as instantiated according to the semantics space data, its subject domain, actual status, and possible changes;
- for the notion of semantic relevance and dispositional knowledge the ADS-procedure offers an empirically based approach to and a contents dependent representation of possible semantic/factual default connotations which become accessible under the aspect of a certain meaning point being primed;
- for models of memory and semantic structure the ADS-procedure provides a flexible, aspect-driven means for the detection of possible paths of spreading activation which branch across semantic space, submitting relevant portions of it to associatively guided semantic search strategies, retrieval operations, and processes of analogical reasoning, as opposed to logical deduction (STEELS 1981).

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Comparative Onomasiology and Semantics

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Comparative Slavic and European Lexico-Semantics will be a progress towards and a basis for a world-wide synopsis of human expression. Both the Dictionary of selected synonyms in the principal Indo-European languages by C. D. Buck, Chicago, 1949, reprinted 1971, and my Wörterbuch der vergleichenden Bezeichnungslehre (Onomasiologie), being published since 1979 in Heidelberg, Carl Winter Universitätsverlag, want to be an answer to the question of many linguists and other researchers after the denotation or denomination of concepts, Walter Porzig's call of 1953 being still to be satisfied by a dictionary of onomasiology (Bezeichnungslehre).

Whereas Buck has about 1500 concepts (catchwords) in the synonyms in a narrower sense of about 28 ancient and modern Indo-European languages including the Celtic and Old Indian and Iranian, but leaving aside a full mention of the functional equivalents (synonyms) in a wider sense and a full inclusion of the Slavic and East European languages, my plan of London 1952 (published 1956 in the Proceedings of the VIIth International Congress of Linguists) has been meanwhile begun to be executed so far that a dictionary of comparative onomasiology has begun, starting from a German and an English catchword, to present in each of its 600 articles the functional equivalents of the respective conceptual words in 30 languages of Central, Eastern and South-East Europe, together with their predecessors and neighbours, i.e. the principal Germanic idioms, Baltic Lithuanian and Lettish, Latin, French, Italian, ancient and modern Greek, Church Slavonic (thirteen literary Slavic languages and extinct Polabian), Hungarian and Turkish.

Expansion and Identification of Reduced Nominals in English

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This paper is about English grammar. It concerns the kind of grammatical units we call "nominals". The points to be made are as follows: 1) Within the class of what we call "nominals" is a distinct subclass which we may call "reduced nominals". 2) Between a "reduced nominal" and another nominal we can discern two very different sorts of relations, namely, the relations of "expansion" and the relations of "identification". 3) Recognition of these facts can make us better understand various problems related to nominals, including the problems of proper names and so-called "pronouns".

By a "reduced nominal" I mean a nominal of any size and any structure that is replaceable, without any change in semantic content, by a regular endocentric noun phrase larger in size, which I then regard as a "paraphrase nominal" of the "reduced nominal". The crucial point, of course, is "semantic content". Since our understanding of what we call "meaning" is still anything but complete, I do not think I can define this term in any really useful way at the moment. I would like to mention only the two assumptions I make in applying the term to what we call "nominals". First, I think every common noun differs in semantic content from every other common noun (so that any endocentric noun phrase containing a common noun as its head must also differ in semantic content from another noun phrase if the latter contains another common noun as its head). Second, I think that, given any two nominals, A and B, we may be sure there is no difference in semantic content between A and B if and only if we can get a statement of the form 'A copula B' (or alternatively 'B copula A') that is tautologous to our intuition (where by "tautologous" I mean "true but completely uninformative"). Thus, based on these assumptions, we say there is a difference in semantic content between a nominal such as a *fool* and another nominal such as a *foolish person*. On the other hand, given a proper name such as *Anwar Sadat* and a larger, endocentric nominal such as *the person called Anwar Sadat*, we may say the smaller nominal is a "reduced nominal" corresponding to the larger one, because it is tautologous to say '*Anwar Sadat is the person called Anwar Sadat*'.

Now, as indicated above, given a "reduced nominal", A, and a corresponding "paraphrase nominal", B, it is always the case that B is larger in size than A. It is thus reasonable to consider replacement of A with B as a sort of "expansion". In other words, we may reasonably say that A stands in a sort of "expansion relation" to B, or alternatively that A is "expandable as" B.

But "expansion relations", or E-relations, are not the only kind of relations

discernable between "reduced nominals" and other nominals. There are relations of another sort, namely, the relations of "identification", or I-relations, which differ from E-relations in several ways, one of the differences being that, while E-relations are characteristics of "reduced nominals", I-relations are not limited to "reduced nominals". What is an I-relation? To say that a given nominal, A, has an I-relation with another nominal, B, or alternatively that A is "identifiable as" B (so that B is an "identifying nominal" of A) is to say that we can have a statement of the form 'A copula B' that is true and not tautologous. This means at least two things. First, like E-relations, I-relations are unidirectional, because statements of the form 'A copula B' are not always reversible. Second, while nominals associated by E-relations must agree in semantic content as discussed above, nominals associated by I-relations must differ in this respect, because a statement of the form 'A copula B' is not tautologous only if B says something that A does not.

Summarizing, given any two nominals, A and B, A has an I-relation with B if 'A copula B' is true and not tautologous, and A has an E-relation with B if 'A copula B' is tautologous and A is smaller in size than B.

Having observed these basic differences between E-relations and I-relations, let us next turn to another important fact about these two relations. It is the fact that, while all E-relations are independent of context and consequently stable enough to form a finite set, not all I-relations are independent and stable, due mainly to the existence of a class of "reduced nominals", many of them so-called "pronouns", which typically can have only context-dependent I-relations, with the result that there can be indefinitely many I-relations, or rather sets of I-relations, for each of them.

Let us first consider "reduced nominals" which can have independent I-relations. These include nominals like *Anwar Sadat* mentioned earlier, which is identifiable as *the Egyptian President who was assassinated in late 1981, a Nobel Peace Prize winner*, etc., with the list varying depending upon our knowledge about the person in question. But, no matter what the actual list may be like, it is clear that, for any speaker or hearer, this "reduced nominal" cannot have more than one set of I-relations associated with it.

Things are different when it comes to forms such as *the building*. I suggest that this form is expandable as something like *the most immediately pertinent building*. But there are indefinitely many things called *building*, and any of them is a candidate for becoming *most immediately pertinent*. Therefore, to any speaker or hearer, this nominal, *the building*, clearly can have, not just one, but indefinitely many sets of I-relations associated with it. For example, it may be identifiable as *the White House*, etc., in one context, but as *part of the Imperial Palace*, etc., in another, and there is no way of knowing what all the other possibilities are like. That, obviously, is also true of everything we have been calling "pronouns".

The status of so-called "pronouns" as "reduced nominals" is perhaps less apparent. But, with some imagination, I think we can see, with little difficulty,

that at least every third-person "pronoun" is expandable in a fixed way. For example, the word *it* is perhaps expandable as *the most immediately pertinent impersonal element* while the "pronouns" *they*, *he*, and *she* are expandable as *the most immediately pertinent collection of elements*, *the most immediately pertinent male person*, and *the most immediately pertinent female person*, respectively. However, it is doubtful whether first-person and second-person "pronouns" are also expandable in this fashion, there being nothing capable of replacing them without making them lose their first-person or second-person status. Nevertheless, they are both clearly variable in I-relations depending upon contexts just like other "pronouns".

Naturally, if a third-person nominal can have indefinitely many sets of I-relations, one constant problem for speaker and hearer whenever the nominal is used is the problem of how to agree upon the relevant set of I-relations. I believe the task is carried out in a manner governed by what we may call "principle of displacement". That is, at any time, for any third-person nominal with context-dependent I-relations, there is one dominant set of I-relations associated with it that has priority over any other possible set. As far as the word *it* is concerned, for example, such a set is always the set associated with the word when we use sentences like *it rained* or *it is getting late*. The principle is that this dominant set of I-relations, whatever it may be actually like, is taken for granted unless it is "displaced" by another set on the basis of hints successfully provided by the speaker. Such hints may be either verbal or non-verbal, and they may be either direct or indirect. If the hint for identification is verbal and occurs in the same sentence with the "pronoun", and further if the "pronoun" directly stands in an I-relation to the verbal hint, then we have a straightforward "anaphoric pronoun". Otherwise, we have either "anaphoric pronouns" of more elusive sorts, or "pronouns" said to be "deictic" rather than "anaphoric" in function.

So a "reduced nominal" may be a "pronoun", a "proper name," or even a regular endocentric noun phrase containing a common noun as its head, while "pronouns" like *I* or *you* are perhaps not "reduced nominals" at all. What, then, are so-called "pronouns"? It is likely that the unifying characteristics of what Western scholars traditionally consider "pronominal" are simply context-dependent I-relations coupled with something as vague as sheer simplicity of form. In any case, it is important to remember that, "pronominal" or otherwise, if a nominal is a "reduced nominal", it can have both E-relations and I-relations and if anything can have both E-relations and I-relations, one of the ways in which we can go astray when talking about its meaning is to confuse the two types of relations. A recent example of such confusion is the current generative-transformational notion of "pronominalization" which, taking the name "pronoun" seriously, supposed "pronouns" to be really things that can replace exact copies of their "antecedents" in an operation said to be "meaning-preserving". I hope this paper will help us better understand the nature of that mistake so that we may be safer from similar mistakes in the future.

The Semantics of Sentence Mood in Typologically Differing Languages

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0. Introduction

The following is a report on a work in progress which aims at developing a formal semantics for the structural as well as lexical means natural languages dispose of in order to indicate sentence moods and illocutionary forces (or, as I shall say equivalently, illocution types). The line of research chosen, which (a) compares several typologically quite different languages, (b) concentrates on the *structural* indicators, and (c) proceeds from form-type to function-type, is diametrically opposed to the strategy adopted by John Searle and Daniel Vanderveken in their "Foundations of Illocutionary Logic", which (a) starts out from one language only (namely English), (b) concentrates on the lexical means English has for *labeling* illocutionary forces (and not on the structural means it has for *indicating* them), and (c) proceeds from function (i.e. illocutionary force) to form (i.e. primarily illocutionary verbs).

Although the research strategies are quite different, the two approaches share the common assumption that "illocutionary logic is part of a theory of meaning" (Searle/Vanderveken[1]: 1-8), and their main goals are also the same, viz. (i) to explicate the logical properties of and relationships between illocutionary acts, and (ii) to construct a formal semantics for the illocutionary force indicators of natural languages and thereby to complete the 'Universal Grammar' as conceived of e.g. by Richard Montague.

1. *An inventory of the sentence moods and their indicators in six typologically differing languages*

1.0 As a starting point, I take the notion of sentence mood as it can be found in most grammars, i.e. undefined, but with a fairly good inter-grammar agreement in the central cases (declarative, interrogative, imperative), and a fairly bad one in the more marginal cases (exclamatory, optative, exhortative etc.).

Using this notion, the following overviews can be given:

1.1 *Overview of sentence moods in the six sample languages*

	English	German	Guarani	Quechua	Chinese	Korean
1. Declarative	+	+	+	+	+	+
2. Interrogative	+	+	+	+	+	+
3. Jussive (Imperative)	+	+	+	+	+	+

4. Exclamatory	+	+	?	+	+	+
5. Dubitative	+	+	+	+	+	+
6. Optative	+	+	+	+	-	+
7. Propositive	+	+	-	-	+	+
8. Quotative	-	+	+	+	-	+
9. Rhetorical interrogative	-	(+)	?	-	-	+
10. Promissive	-	-	-	-	-	+

1.2 Overview of structural sentence mood indicators in the sample languages

	English	German	Guaraní	Quechua	Chinese	Korean
1. Intonation/Punctuation	+	+	+	+	+	+
2. Interrogative words	+	+	+	+	+	+
3. Indicators of Person deictic categories	+	+	+	+	+	+
4. Indicators of verbal mood/Modal verbs	+	+	+	+	+	+
5. Word order	+	+	(+)	+	+	-
6. Affixes	-	-	+	+	-	+
7. Particles	-	+	+	+	+	?
8. Ellipsis of subject	+	+	-	-	+	-
9. Exclamatory words	(+)	(+)	?	-	+	?
10. Special constructions	do-prep.	-	?	?	V-not-V	?

2. Towards an explication of 'sentence mood': the notion of L-sentence type

I will now try to spell out the idea behind the traditional notion of sentence mood, namely a correspondence between syntactic structure and illocutionary force. I presuppose that for any natural language L, there is a set of L-sentence readings the elements of which are ordered pairs of an L-sentence and a structural description of it which is rich enough to exclude ambiguities; I presuppose furthermore that (a) each L-sentence reading determines uniquely the set of its syntactic features, and (b) each meaning of an L-sentence reading determines uniquely the set of its illocution type features.

The definition is then as follows:

(1) X is an L-sentence type iff

- X is a non-empty set of syntactic features of L, and
- there is a non-empty set Y of illocution type features of L such that every L-sentence reading with X has an L-sentence meaning with Y.

If X is an L-sentence type, the Y which satisfies condition (b) will be called the *common illocution type denominator* of the L-sentence readings with X.

Applying (D1) we can hypothesize that there is probably no language L such that the set of explicit performative L-sentence readings is definable as being of type X for any L-sentence type X, since there is no purely structural common denominator, the lexical meaning of the main verb being crucial.

I call the notion just defined sentence *type* and not sentence *mood*, because it is much finer grained than the latter. For instance the interrogative mood corresponds to a whole group of L-sentence types for most languages L. (Guaraní

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with one uniform structural indicator, *-pa*, for all kinds of interrogatives, is one of the exceptions.) Nevertheless, it seems clear that 'L-sentence mood' can be explicated in terms of 'L-sentence type'.

3. *The notion of L-structurally indicated illocution type*

Using (D1) we are now in a position to define as follows:

(D2) Y is an *L-structurally indicated sentential*¹⁾ *illocution type* iff there is an L-sentence type X such that Y is the common illocution type denominator of the L-sentence readings with X.

This notion allows us to cut out two small finite subsets from the probably infinite set of possible illocution types, viz. the two sets of sentential illocution types which are L-structurally indicated (a) in every, and (b) in at least one, but not every natural language L, respectively.

4. *Illocutionary universals hypothesized*

Presupposing an explication of 'L-sentence mood' in terms of 'L-sentence type', it is easy now to derive some hypotheses about illocutionary universals from the findings presented above:

(UH1) The following illocution types and only these are L-structurally indicated in all natural languages L (the corresponding sentence moods are added in parentheses):

- | | |
|--|--------------------------------------|
| 1. <i>Assertive</i> (Declarative) | 4. <i>Subassertive</i> (Dubitative) |
| 2. <i>Erotetic</i> (Interrogative) | 5. <i>A-Expressive</i> (Exclamatory) |
| 3. <i>Directive</i> (Jussive/Imperative) | (Expressing amazement) |

(UH2) The following illocution types are L-structurally indicated in some, but not all natural languages L:

- | | |
|--------------------------------------|---|
| 6. <i>W-Expressive</i> (Optative) | 8. <i>Reporting Assertive</i> (Quotative) |
| (Expressing wish) | 9. <i>Pseudoerotetic</i> |
| 7. <i>We-Directive</i> (Propositive) | (Rhetorical Interrogative) |
| | 10. <i>Commissive</i> (Promissive) |

(UH3) There is no natural language L such that the following illocution types are L-structurally indicated:

11. *Check-Erotetic* or Exam Question, i.e. eliciting an answer in order to check whether the addressee knows it
12. *Abraham-Directive*, i.e. eliciting a response in order to check obedience

1) I call sentential the type of illocutions performed in uttering sentences, as opposed to other phrases ("Hurrah for Porky!") and interjections ("Wow!").

13. *Declarative*, i.e. making true a proposition *p* by happily performing an illocutionary act with the propositional content *p*.
5. *The abstract predicates approach to illocutionary logic as opposed to the Searle/Vanderveken approach*

Searle/Vanderveken (henceforth S/V) claim that illocutionary forces can be represented by septuples whose first coordinate is illocutionary point. There are six basic illocutionary points which are taken as primitive notions: Assertive, Commissive, Directive, Declarative, Expressing Belief and Expressing Desire. Forces with basic point where the other coordinates (mode of achievement, different conditions and degrees of strength) are kept minimal are called primitive. The set of illocutionary forces includes the primitive ones and is closed under certain operations on the coordinates. These operations induce a partial ordering in the set of illocutionary forces and illocutionary entailment is defined as subordination with respect to this ordering.

My approach, which I call the abstract predicates approach²⁾ takes the L-structurally indicated illocution types as primitives, represents them by abstract predicates and characterizes the latter with the help of meaning postulates, introducing thus illocutionary entailment through another door.

Both approaches are axiomatic, but the primitive notions are different. Nevertheless it is possible that one day they turn out to be equivalent. Some aspects, however, of S/V's proposal make me doubt that this will be the case. Let me conclude by commenting on two of them: On the one hand, the definition of force is much too liberal, since if the propositional content conditions are unrestricted, they may admit of only one single proposition and thus 'illocutionary force' includes all elementary illocutionary acts. There would be e.g. the force of asking where the next ICL will take place. On the other hand, the definition seems to be too restrictive too: By separating illocutionary point and degree of strength of its achievement, S/V run into troubles when a change in the latter results in a change of the former. Consider the Guarani suffix *-ndipo* which is described as an interrogativo-dubitative affix and can be translated with either 'perhaps' or the question mark. Here, S/V's definition forces one to classify the corresponding illocution either as low degree assertive or as answer eliciting directive, the corresponding points being strictly separated.

So in its present preliminary version, S/V's definition seems to be both too loose and too restrictive to be really adequate for a formal semantics of sentence mood and other force indicators in natural languages.

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2) It is outlined in [2], spelled out for a large fragment of German in [3], which is complemented in [4].

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About a Tendency to Feminize in Canadian French

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From a normative point of view, there is ample evidence showing the widespread phenomenon in Canadian French which has been qualified of "gender confusion" by many observers interested in this spoken French dialect. As a matter of fact, many entries are found in *Le Glossaire du parler français au Canada* (1968) where nouns are given as being masculine as well as feminine. Example (01) gives an idea of the apparent symmetry between the processes of masculinization and feminization of nouns but, as first noticed by Chamberlain in 1895¹⁾, there exists "a decided tendency to feminize" which seems to only affect standard masculine nouns.

- | | |
|------------------------------|---------------------------------|
| (01) a. acte "act" s.f. | b. aide "aid" s.m. |
| air "air" s.f. | ancre "anchor" s.m. |
| appartement "apartment" s.f. | appendicite "appendicitis" s.m. |
| attelage "harness" s.f. | araignée "spider" s.m. |
| été "summer" s.f. | artère "artery" s.m. |
| hôpital "hospital" s.f. | étable "barn" s.m. |
| ordre "order" s.f. & s.m. | horloge "clock" s.m. |
| office "holy office" s.f. | image "picture" s.m. |
| orteil "toe" s.f. | offre "offer" s.m. |
| ulcère "ulcer" s.f. | oie "goose" s.m. |

(quoted from *Le glossaire du parler français au Canada* 1968)

Statistical results of our research have confirmed this superficial observation. Moreover, we can point out that gender switching also exists in popular metropolitan French where it is generally considered to be a marginal phenomenon. As quoted by Tucker, Lambert and Rigault (1977)²⁾, the gender confusion occurs frequently in vowel initial nouns, here after called VIN. Finally, we proposed the hypothesis that not only there is no confusion in gender assignment but also that it is not masculinization but feminization which is involved in the process of gender feature assignment in those cases where the initial vowel can be taken as a phonological hint of gender.

The most common explanation to the gender switch proposes a neutralization of the phonological alternation of the indefinite article *un/une/œ-yn/* which becomes in spoken French something like *eune/œn/* as in the feminine *une araignée* "a spider" which becomes */œnareŋe/* as well as in the masculine *un habit* "a suit", which becomes */œnabi/*. Hence, such a neutralization can be extended to the whole French determiner system, since the initial vowel

eliminates the phonic indices of the grammatical gender borrowed by the noun.

In many cases we face a problem of gender ambiguity especially when we analysed the well-known Sankoff-Cedergren Corpus. This corpus is based on the recording of 120 French Canadian native speakers, as many men as women. This corpus was established with respect to a number of extra-linguistic and sociological parameters. The treatment of the statistical data has been performed by a sophisticated computer programme based on probabilistic calculus.

Here are the principal results of our investigation. First, we found 1044 non-ambiguous occurrences of gender over more than 10,000 cases of potential occurrences. Thus, there was an important loss of information. Second, the gender switch in favor of the feminine reaches roughly 30% of the non-ambiguous masculine occurrences in standard French while only 1.6% of the non-ambiguous feminine occurrences in standard French were submitted to the masculinization. Nevertheless, there were more standard feminine occurrences (671) in the whole corpus than standard masculine occurrences (373).

Now, if we take into account the fact that this set of non-ambiguous occurrences is realized in the corpus as 80 different words, i.e. so-called "types" against "tokens", we obtained a score of 56% of feminization within the subset of standard masculine words whereas we got no significative masculinization in the subset of standard feminine words. Such a result means roughly that each VIN of the corpus which is masculine in standard French gets more than one chance in two to be feminized whereas each VIN of the corpus which is feminine in standard French gets a 100% chance to remain feminine.

Such a statistical tendency not only confirmed our hypothesis but also strengthened an empirical observation that we can make about the familiar use of the Canadian French. Indeed, we notice that many phenomena of "liaison" are only consistent with feminization. For instance, in the familiar expressions like in example (02), the liaison is always accomplished by the morpheme which is consistent with the feminine as in (a) but never with the masculine as in (b):

- (02) a. une grande hôpital "a big hospital": /œngrādopital/
 b. *une grand-t-hôpital */œngrātopital/
 a. une grosse orteil "a big toe": /œngrosɔrtej/
 b. *une gros-z-orteil */œngrozɔrtej/

Agreement phenomena also indicates that gender switching is only consistent with feminization, especially when a preposed adjective destroys the contiguity of the determiner with the VIN as in (03):

- (03) a. sa / son deuxième examen "her / his second examination"
 b. de la / du bon air pur "some good fresh air"
 c. ma / mon vieil habit "my old suit"
 d. une / un drôle d'accident "a curious accident"
 e. cette / ce grand appartement-là "that big apartment"

On the other hand, feminine VIN in standard French never involve masculin-

nization as illustrated by ungrammatical examples of (04):

- (04) a. une / *un belle étoile "a beautiful star"
 b. sa / *son longue histoire "her / his long story"
 c. la / *le meilleure aubaine "the best bargain"
 d. cette / *ce grande église "this big church"

The same coherence of agreement, which is only relevant to feminization, can be also observed with phonetically variable postposed adjectives like, for instance, in expressions such as: "l'accident *la plus niaiseuse*" ("the most stupid accident") or "une hôtel *ben dispendieuse*" ("a very expensive hotel") or again "son accent est pas mal *différente* de nous-autres" ("his accent is sure different from ours"). Finally, agreement in variable pronominal elements is only consistent with feminization like in the following left or right dislocated sentences (05):

- (05) a. On *la* voyait de même, c't avion-là! "We saw it 'this way', the plane!"
 b. Je *la* trouve pas pire, ton escalier! "I think it's not bad at all, your stairway!"
 c. L'arrêt de Dryden, *elle* était superbe! "Dryden's stop, it was fantastic!"
 d. L'étage où il reste, on *la* connaît pas!
 "The floor where he lives on, we don't know it!"

To conclude the linguistic analysis, we are inclined to advocate for a rule of gender assignment which is phonologically motivated in spoken French as well as in other languages such as Spanish, for instance. This grammatical rule would be internalized at the level of the linguistic competence of French Canadians.

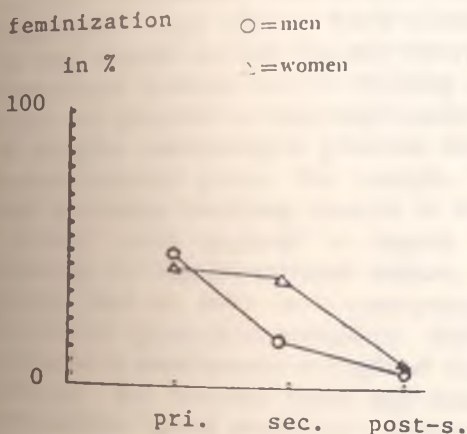
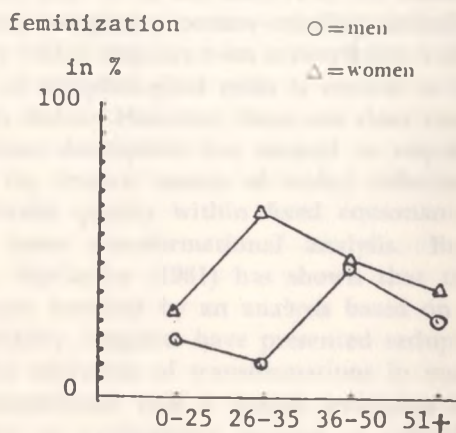
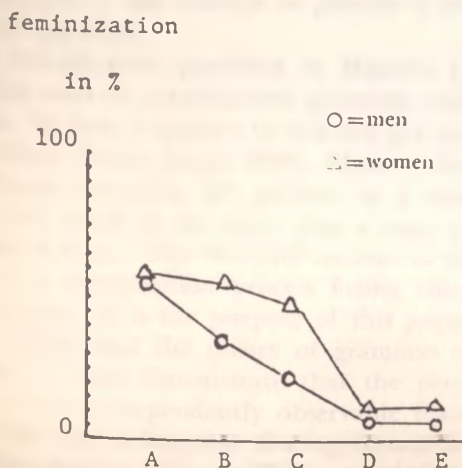
Let us finally say a word about the sociolinguistic aspect of such a variable rule. More details on this matter are available in Barbaud, Ph., Ch. Ducharme and D. Valois (1981; 1982)⁹. Nevertheless, it is interesting but not very surprising to know that the rule of feminization is positively correlated with three sociological parameters that is 1) education; 2) age and 3) linguistic market, something near of the economical status of the head of family. There are no significant correlations with the linguistic contexts or the sex of the informants.

As we can see in the first diagram A, standing for the variable "education", the rate of feminization is about 10% in the group of the most educated informants. But this tendency increases as we go down the education scale, to reach near of 50% in the group of the less educated informants.

The second diagram B is relevant to the variable "age" of the native speakers. At first glance, it shows a contradictory difference between men and women, especially with the youngest. But in fact, their trajectory is the same in spite of the fact that they do not culminate at the same moment. Roughly, we see that the tendency to feminize increases from right to left, starting with a rate of 30% for the oldest and reaching a good 60% for the younger men and 45%

for younger women. Then, we notice a renewed drop in the curve indicating that the youngest men and women are least inclined to feminize. The reason seems to be that the level of education has been increasing in Quebec since the early Sixties.

Finally, the last diagram C standing for the variable "linguistic market", shows that the rate of feminization increases regularly with the deterioration of family revenue since those informants which belong to the two highest levels of the economic scale engage in less than 10% of feminization whereas the least favored informants on the same scale do more than 50% of feminization.

Diagram A *Feminization and education*Diagram B *Feminization and age*Diagram C *Feminization and linguistic market*

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The first graph shows two lines, one with open circles and one with open squares. The line with open circles starts at approximately 80 at x=+12, drops to 60 at x=02, rises to 90 at x=-02, drops to 70 at x=-04, rises to 80 at x=-06, and drops to 60 at x=-08. The line with open squares starts at approximately 60 at x=+12, rises to 80 at x=02, drops to 70 at x=-02, rises to 90 at x=-04, drops to 80 at x=-06, and rises to 90 at x=-08.

The second graph shows two lines, one with open circles and one with open squares. The line with open circles starts at approximately 10 at x=0, rises to 20 at x=1, 30 at x=2, 40 at x=3, 50 at x=4, 60 at x=5, 70 at x=6, 80 at x=7, 90 at x=8, 100 at x=9, and 110 at x=10. The line with open squares starts at approximately 10 at x=0, rises to 20 at x=1, 30 at x=2, 40 at x=3, 50 at x=4, 60 at x=5, 70 at x=6, 80 at x=7, 90 at x=8, 100 at x=9, and 110 at x=10.

The third graph shows two lines, one with open circles and one with open squares. The line with open circles starts at approximately 10 at x=0, rises to 20 at x=1, 30 at x=2, 40 at x=3, 50 at x=4, 60 at x=5, 70 at x=6, 80 at x=7, 90 at x=8, 100 at x=9, and 110 at x=10. The line with open squares starts at approximately 10 at x=0, rises to 20 at x=1, 30 at x=2, 40 at x=3, 50 at x=4, 60 at x=5, 70 at x=6, 80 at x=7, 90 at x=8, 100 at x=9, and 110 at x=10.

Restricting the Power of the Morphological Component: Reduplication in Southern Paiute

Alec Marantz

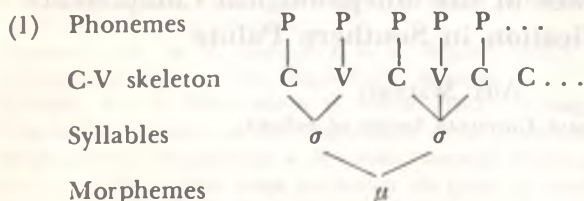
Harvard University Society of Fellows

An explanatory theory of morphology must be restrictive enough that it excludes as impossible morphological rules that we do not observe in the world's languages.¹ From what we know of morphological processes crosslinguistically, we may already conclude that any theory which employs even a restrictive transformational notation for the encoding of morphological rules is certain to be much too powerful to reach explanatory status. However, there are clear cases of complex morphological processes whose description has seemed to require transformational power. For example, the Semitic system of verbal inflection and derivation involving changes in vowel quality within fixed consonantal "skeletal" roots appeared to require some transformational analysis. But, rejecting the transformational account, McCarthy (1981) has shown that the Semitic data are much more appropriately handled by an analysis based on a multi-level approach to phonology. Similarly, linguists have presented reduplication as a paradigmatic example of the operation of transformations in morphology. But Marantz (1982) has demonstrated that a simple extension of McCarthy's "tiered phonology" provides an explanatory account of reduplication without the adoption of transformational power. It has proven enlightening to seek out what appears to be the best evidence for a powerful, and thus non-explanatory, morphology and attempt to provide a more restrictive account of the phenomena in question.

The account of reduplication provided in Marantz (1982) is so restrictive that it rules out many sorts of reduplication processes easily statable in a transformational notation. In fact, it appears to rule out precisely the sort of reduplication found in Southern Paiute (Sapir 1930). Most succinctly stated, reduplication in Southern Paiute (hereafter SP) prefixes to a stem a copy of the first consonant and the first vowel of the stem, plus a copy of the next consonant only if this consonant is nasal. The Marantz account of reduplication prohibits the incorporation of a reduplication process fitting this description into the grammar of any language. It is the purpose of this paper to reaffirm the conclusion of Marantz (1982) that the theory of grammar must prohibit such a reduplication process. I shall demonstrate that the peculiar character of SP reduplication follows from independently observable features of the language. The theory predicts that only a language sharing certain features with SP could exhibit a reduplication process with an SP-like restriction.

Tiered phonology. McCarthy's (1981) account of Semitic morphology

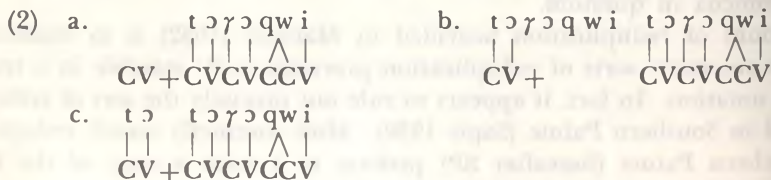
depends on a multilevel analysis of phonological structure, a simplified schematization of which is displayed in (1).



In particular, note that phonemes in (1) are linked to C and V "slots" in what is called a "C-V skeleton." A linking principle insures that [+syllabic] phonemes hook-up to V (vowel) slots and [-syllabic] phonemes to C (consonant) slots in the unmarked case. Also, the hook-up among levels follows the general constraints of autosegmental phonology: constituents at each level are linearly ordered and association lines may not cross. McCarthy argues convincingly that there are morphemes that consist only of a C-V skeleton and others that consist only of a phonemic melody.

Reduplication. In Marantz (1982) I argue that reduplication consists essentially in the affixation to a stem of a C-V skeletal morpheme lacking a phonemic melody and the copying over this skeleton of the stem phonemic melody on the same autosegmental tier as this melody. The phonemes of the "borrowed" stem melody attach to the C-V slots of the reduplicating affix according to general principles, details of which need not concern us here. Extra C-V slots and extra phonemes effectively disappear.

As an example of the operation of reduplication, consider the reduplication of the SP stem $t\gamma\zeta q \cdot wi$ 'runs' to form $t\gamma\zeta \cdot \zeta qwi$ 'runs several times' (I reproduce Sapir's transcriptions here, which are a melange of the phonetic and the phonological).²¹



We see in (2a) the affixation of the reduplicating prefix, simply a C-V skeleton without an attached phonemic melody. In (2b), the reduplicating prefix borrows the phonemic melody of the stem to which it is attached, and this melody hooks up with the skeleton according to the general principles of autosegmental phonology, as well as the couple reduplication-specific principles discussed in Marantz (1982). As shown in (2c), as far as the rest of the morphology as phonology is concerned, the extra (unattached) phonemes (or C-V slots) do not exist.

Southern Paiute. The examples of SP reduplication in (3) illustrate the

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fact that reduplication copies the first CV of a stem plus the next consonant only if this C is nasal.

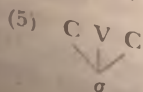
- (3)
- | | | | | |
|----|-----------|------------|----------------|-------------------------|
| a. | tavin'na- | 'to strut' | tarávin'na·ai' | 'to keep strutting' |
| b. | qU'qwi' | 'shoots' | qU'qóq·wi' | 'shoots several times' |
| c. | kwíp·A | 'to hit' | kWI'kwíp·A | 'to hit several times' |
| d. | tón·a- | 'to stab' | tóntón'nai' | 'to stab several times' |
| e. | pīn·i- | 'to look' | pīmpīn'ni' | 'looks repeatedly' |
| f. | wī'īi' | 'dances' | wīwī'īi' | 'dances repeatedly' |

The behavior of "vowel initial" stems under reduplication deserves special note. It is unclear whether SP should be considered to contain any vowel initial stems. Stems beginning with a vowel phonetically may actually start with a glottal stop in their phonological representations (see Pranka 1982 for some discussion). Whether or not apparently vowel initial stems actually begin with a vowel, the reduplicating prefix proposed below will yield the correct results for such stems. In particular, if a string of borrowed phonemes beginning with a [+syllabic] segment is to attach to a C-V reduplicating prefix starting CV. . . ., our linking principles dictate that the initial syllabic phoneme will attach to the V slot in the skeleton, just as if the skeleton began V. . . . The reduplicated forms of some "vowel initial" stems are given in (4).

- (4)
- | | | | | |
|----|-------------|------------------|-----------------|---------------------------------|
| a. | īŋa·'p·its· | 'baby' | īī·ŋ'ap·itsiŋwī | 'babies' |
| b. | īmwiī- | 'several arrive' | īm'mūip·īŋa | '(they) arrive each by himself' |
| c. | ivi- | 'to drink' | i'ip·i' | 'drinks repeatedly, sips' |
| d. | orŋwi' | 'roars' | or'or'wŋi' | 'roars several times' |
| e. | avi- | 'to lie down' | a'áΦi | 'to begin lying down' |

Note that the analysis of reduplication sketched above does not allow us to express the sort of reduplication rule that the data in (3) imply. The formalism does not provide the means to postulate a CVC+ reduplicating prefix and specify that a phoneme should only attach to the final C if this phoneme is [+nasal]. There is a provision in the Marantz (1982) for preattaching distinctive features to slots in a C-V skeleton, but a [+nasal] feature attached to the last C in a CVC+ reduplicating prefix for SP would have the effect of always copying the second consonant in a stem and making this consonant [+nasal], regardless of its inherent nasality.

However, SP exhibits a feature that allows us to suppose that the reduplicating prefix in this language is simply CVC. The fact that only a nasal second consonant is copied follows immediately if we assume that the C's and V's of the CVC+ prefix are grouped together to form a syllable, as shown in (5).



Yip (1981) has pointed out the implications of grouping C-V reduplicating skeleta into syllables and has suggested such grouping is necessary to account for features of Chinese "secret languages." For our purposes, it is not crucial whether the SP prefix specifies particular C-V slots as shown in (5) or whether the prefix simply consists in the "maximal expansion" of the syllable symbol. As Sapir notes, only nasals among the consonants may close syllables, and even nasals will close a syllable only if the following syllable begins with a stop consonant (1930, p. 37):

Every Paiute syllable consists, properly speaking, of a vowel (long or short) or diphthong preceded, or unpreceded, by a consonant; or of such a primary syllable stopped by a nasal consonant that is itself followed by a stopped consonant or *w*. [Sapir immediately takes back his statement about *w*.]

In fact, the only time the reduplicating prefix contains a nasal (or any final C) is when the root begins with a stop consonant. By grouping the CVC of the SP reduplicating prefix into a syllable, we allow the syllable structure constraints of the language to determine which phonemes may attach to the C and V slots. In SP, these syllable structure constraints allow only a nasal among consonants to attach to the final C slot in the reduplicating prefix, and even this possibility for a syllable final consonant is restricted according to the onset of the following syllable.

Alternative hypotheses. It seems clear that the unusual restriction on reduplication in SP must be related in some manner to the constraint on syllable final consonants. Grouping the C's and V's of the reduplicating prefix into a syllable automatically accounts for the restriction and makes it depend on the independently observable syllable structure constraint. However, we should examine two alternative accounts of the condition on reduplication that also relate it to the syllable structure facts. First, one might suppose that SP simply copies the first syllable in a stem. But examples like (3d & e) show that a nasal is copied even when it begins the second syllable in a word.

A second alternative account, one not so easily dismissed, would suppose that the SP reduplicating prefix is CVC+ and that the second stem consonant always attaches to the second C slot. However, this hypothesis would maintain unless this C is a nasal, it cannot be syllabified in the resulting derived word and thus deletes.³⁾ The problem with this hypothesis is that the required deletion rule is ad hoc. Since morphemes in SP do not end in consonants, one cannot motivate a consonant before consonant deletion rule on reduplication independent grounds. There are prefixes in SP which "geminate" the first consonant of the C initial stems to which they attach. The behavior of such prefixes has led previous investigators to suppose that they end in an underspecified consonant, which assimilates in point of articulation with a stem initial C. Pranka (1982) argues that such prefixes should be seen as ending in a free C slot, to which the stem initial consonant phoneme may attach. Which ever analysis we adopt, in this case of geminating prefixes, the only situation

in which a consonant final morpheme is well-motivated, we find no evidence of a deletion rule of the sort necessary to account for the facts of reduplication.

If a second non-nasal C in the reduplicating prefix could never be syllabified with the beginning of the stem, we could argue on reduplication internal grounds that no rule is necessary to explain the non-appearance of such a C in reduplicated forms. However, the behavior of "vowel initial" stems under reduplication reaffirms the necessity for an ad hoc deletion rule under the alternative analysis. Recall that it is unclear whether apparently vowel initial stems actually begin with a vowel or with a glottal stop. In either case, the hypothesis that the C-V slots of the reduplicating prefix are grouped together into a syllable makes the correct prediction about the reduplicated forms of such stems. No consonant, not even a nasal, may close a syllable before either a vowel or a glottal stop. Thus, since the reduplicated phonemes must fall into a single syllable, we should expect to copy only the vowel (or the initial glottal stop and the vowel) of vowel initial stems regardless of the identity of the C after the initial vowel.⁴⁾ As illustrated in (4) above, this expectation is born out. However, if the C's and V's of the reduplicating prefix are not syllabified, as in the alternative hypothesis under consideration, there is no reason why the final C of the prefix should not become part of the onset of the first syllable of the stem. As C plus glottal stop combinations are common in SP, regardless of whether the "vowel initial" stems begin with a V or a glottal stop the C could be syllabified with the first syllable of the root. Therefore, an ad hoc rule would be required under the alternative hypothesis to delete the second C in the reduplicating prefix either before a glottal stop or before a vowel. On the other hand, the syllable structure constraint which correctly predicts the condition on SP reduplication under the hypothesis I am supporting is directly observable in SP outside the context of reduplication, and is thus not ad hoc.

Conclusion. The apparent exception to my (1982) restrictive theory of reduplication turns out to provide strong support for this theory. We predict that a language should not exhibit SP's "nasal only" condition on reduplication unless the language independently restricts the distribution of consonants in such a way as to assure this condition without requiring more powerful morphological mechanisms. We can truly say that the theory explains the peculiar constraint on SP reduplication: given the restrictions on possible reduplication rules and independently observable facts of SP, Southern Paiute reduplication must operate in the apparently strange manner it does.

Notes

1) This paper arose from a discussion with Moira Yip, who should be credited with the idea of grouping the C-V slots of a reduplicating prefix into a syllable template, and depends heavily on the work of Paula Pranka (see Pranka 1982).

2) Among other functions, reduplication in SP is used to form various sorts of plurals.

3) Consider a stem which begins with a "spirantizing" C, i.e., a consonant which shows up as a stop initially but as a spirant after V final prefixes. If such a stem's second consonant were identical with its first, the alternative hypothesis under consideration would derive a structure like (i) for the stem's reduplicated form.



There is no reason in principle to suppose that the two identical phonemes attached to two adjacent C slots should not be interpreted as a geminate consonant. Depending as it does on the possibilities of syllabification to account for the restriction on the copying of the second C in reduplication, our alternative hypothesis should predict that no stem of the form CVC... where the first and second C's are identical will be classed by Sapir as "spirantizing" under reduplication. The reduplicated forms of such stems should include the structure in (i) on this hypothesis, yielding gemination. In fact, I have found no stems in Sapir's grammar which are "spirantizing" but whose first two C's are identical. I am betting that this is just an accidental gap in the data. However, if no such stem exists, its absence would constitute strong evidence for the alternative analysis under consideration.

4) The careful reader should note the very unorthodox way I am employing syllable structure constraints in this paper. Technically, the analysis of SP reduplication I offer is flawless. However, it requires a particular interpretation of grouping C-V slots into a morpheme symbol, the interpretation I exemplify here.

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Gender Distinction and Feminine Formation in Modern Hebrew

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All Hebrew nouns have a grammatical gender, masculine (M) or feminine (F), which determines morphosyntactic agreement. In animate nouns the gender coincides in general with sexual distinction, where the male takes the unmarked morphological short form, and the female—the marked long one, e.g., *sus/susá* 'horse/mare,' *student/studentit* 'student (m/f).' In non-animate nouns the distinction is arbitrary with regard to sex, and each noun is lexically assigned a grammatical gender according to the morphological clues, e.g., *kóva* 'hat (m),' *migbá'at* 'hat (f),' *mifné* 'turning (m),' *tafnit* 'turn (f),' Both pairs of words denote essentially the same object/concept, nevertheless, each of them is assigned a different grammatical gender. Moreover, there is no implicit or explicit sexual connection between forms which seem morphologically close but differ in gender, e.g., *mištár/mištará* 'regime (m)/police (f),' *'eškól/'eškolit* 'cluster(m)/grapefruit (f).'

Animate nouns and dependent phrasal components are, therefore, inflectionally marked for gender, whereas non-animate nouns carry only derivational gender markers. The F nouns *tayéret* 'tourist' and *tayélet* 'promenade' are formed alike (from the roots *t-y-r* and *t-y-l*). The former corresponds to *tayár* 'tourist (m),' while the latter has no M counterpart. In *hatayéret hayafá hazót* 'this pretty tourist,' and *hatayélet hayafá hazót* 'this beautiful promenade' there is a morphosyntactic agreement. Both the adjective *yafé* 'pretty, beautiful' and the demonstrative *ze* 'this' take the F forms *yafá*, *zot* (cf. the phrase *hatayár hayafé hazé* 'this handsome tourist'; *ha-* is the definite article in all these phrases).

The Hebrew F morpheme is typified by final /+a/ or by /+(V)t/. Its allomorphs are: (1) [+a], (2) [+it], (3) [+t], (4) [+et], (5) [+at], (6) [+ut], (7) [+ot], and (8) zero ending, as demonstrated in A:

- A. (1) *malká* 'queen' (m: *mélex*), *simlá* 'dress'
 (2) *'ezraxit* 'citizen' (m: *'ezráx*), *šlulit* 'puddle'
 (3) *sinit* 'Chinese' (m: *sini*)
 (4) *zamáret* 'singer' (m: *zamár*), *rakévet* 'train'
 (5) *rokáxat* 'pharmacist' (m: *rokéax*), *karáxat* 'baldness'
 (6) *malxút* 'kingdom'
 (7) *'axót* 'sister' (m: *'ax*), *xacót* 'midnight'
 (8) *'em* 'mother' (m: *'av*), *'éven* 'stone.'

The first five allomorphs are both derivational and inflectional. The [+ut],

[+ot], and the zero endings are restricted to derivational morphemes, in spite of 'axot in (7).

In the present study, Hebrew adjectives and animate nouns were examined in order to describe the factors which determine the F formation. The data seem quite confusing with regard to the masculine/feminine relations, e.g., *barón/baronit* 'baron/baroness,' *rišón/rišóná* 'first (m/f),' *susón/susónet* 'pony (m/f).' The M form in all three pairs is bi-syllabic and ends in *-on*, but the F endings vary: *+it*, *+a*, *+et*. Nevertheless, the endings are rule governed and distributed in a quite clear-cut way: *+a* ending is the most common among M XoC bases; *+it* is typical of loan words (cf. *student/studentit* mentioned above), and *+et* occurs after *+on* diminutive (and derogatory) ending (cf. *šayit* 'horse').

A similar, apparently unstable and unpredictable, situation could be found in the following cases, where the M bases end in XaC, and the F endings differ, e.g., *šfanfán/šfanfaná* 'little rabbit (m/f),' *muksám/muksémet* 'enchanted (m/f),' *nadván/nadvanit* 'donor (m/f),' *xadáš/xadašá* 'new (m/f),' *nagán/naganit* 'performer (m/f),' *zamár/zaméret* 'singer (m/f).' These examples, like the former, are consistent with F formations. Many examples of these types clearly prove that nouns and adjectives of the duplicate $C_1C_2C_3C_2C_3$ root pattern (*šfanfán*) or of the CaCaC non-occupational pattern (*xadaš*) take *+a* F ending; participle forms (*muksám*) and most occupational CaCaC nouns (*zamár*) take *+et* ending; *+an* attributive ending (*nadván*) requires *+it* feminine ending; finally, some occupational CaCaC nouns (*nagán*), especially those with phonetic *+an* ending take *+it* F ending.

Various factors coincide in determining the distribution of the F endings; (a) morphological, such as XaC participles and duplicate roots, (b) morphosemantic, such as occupational/non-occupational CaCaC, attributive *+an* or diminutive *+on*, and (c) morphophonemic, such as loan words or XoC type nominals.

Table 1 presents the distribution of the inflectional allomorphs in nouns and adjectives. Given the specific M base form, the proper F forms could be automatically predicted with relatively few exceptions. The pluses in Table 1 indicate the regular unmarked cases.

XaC, XeC and Xe environments are the most opaque because various factors (morphological, morphosemantic and morphophonemic) coincide. It is obvious that in the case of the occupational XaC, the morphosemantic factors interfere: the Xan occupational stem with *+it* ending interferes with the occupational CaCaC with *+et* ending, and therefore, many nouns of CaCaC pattern take the *+it* ending for the feminine.

The fact that phonetic *+it* is the most popular ending plays an important role in this tendency too. The *+t* attached to the Xi bases together with *+it* suffixed to the loan, Xan and Xay bases constitute the largest number of *+it* endings (47% of the nouns, 38% of adjectives). This phonetic influence no doubt causes the increase of *+it* ending in the CaCaC occupational patterns.

Table 1: The distribution of the inflectional feminine suffixes

Masculine Base Stem	Feminine Suffixes			
	+a	+et/+at	+it	+t
§CVC§	+	—	—	—
Xi	some (gentilic)	—	—	+
XiC	+	—	—	—
XuC	+	—	—	—
XeC	CaCeC CéCeC	+	—	—
XoC	+	diminutive participles	—	—
XaC	nonoccupational CaCaC C ₁ C ₂ C ₃ C ₂ C ₃	occupational CaCaC	+an +ay some occupational CaCaC	—
Xe	+	—	—	niCCe muCCe
Loan	—	—	+	—

The interference of the various factors occurs not only in the CaCaC occupational pattern but also in the morphophonemic Xe and the morphological niCCe and muCCe (participles of final weak roots in two of the verbal patterns). Hence *mukcé/mukcét* 'set apart' alternates with *mukcá* in the F. On the other hand *mištanē/mištnaá* 'changing' alternates with *mištanēt*, etc. One cannot point, however, to the most important factor towards which new morphemes will be inclined. They will probably be an extension of the proposed major factor environments, with deviating single lexical items.

The autonomous exceptions to the classifications presented in Table 1 are relatively rare. They include a few lexical items with idiosyncratic morphological F endings, such as *mumxé/mumxít* 'expert,' instead of the expected **mumxet* or even **mumxa*, *giyór/gyóret* 'proselyte' instead of the expected **giyora*. Others exhibit free variation such as *mumár/muméret* 'apostate' as expected, and *mumarit* as unexpected, *rašá/reša'a* 'wicked,' as expected, and *rašáit* as unexpected. In a few words various feminine markers serve for semantic differentiation, e.g., *ganán/ganénet* 'kindergarten teacher,' *ganán/gananit* 'gardener,' *kómer* 'priest,' *komriyá* 'nun,' *komrit* 'minister's wife.'

The unexpected forms alternating with the regular forms serve in general as stylistic variations. The expected endings provide the standard, unmarked, everyday forms, whereas the marked forms with the unexpected ending provide the high stylistic, literary, learned forms, e.g., *melaméd/melamédet* 'teacher' as expected, and *melamdá* as unexpected. The oddity of their inflection will be handled, like all the lexeme idiosyncratic features in the lexicon. In other words, nouns and adjectives deviating from the generalizations stated above will be separately marked in the lexicon for their inflected forms, either by minor rules or by single lexical markings.

Morphological Operations, Historical Evidence and English [ŋ]

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The velar nasal of most standard varieties of English provides an interesting test case for two approaches to phonology/morphophonology: the "abstract" approach which attempts to treat all phonetic alternation as phonological and which permits "well-motivated" abstractness in underlying representations (UR), and the "concrete" approach which distinguishes between phonological and morphophonemic alternations and which limits the abstractness of UR.

It has long been standard practice in abstract treatments of English phonology to derive [ŋ] from underlying /ng/ via a sequence of two rules: one which assimilates *n* to *g* in point of articulation and another which deletes *g* under appropriate conditions. This analysis is often cited as justification for the general principle that underlying representations must be permitted to contain segments which never appear on the surface (Fromkin 1973:22, Kenstowicz and Kisseberth 1977:7).

This paper is part of a larger project to evaluate, in the light of available evidence, this abstract analysis alongside a more concrete analysis which treats [ŋ] as a single underlying segment. In earlier work (Smith 1979) I have presented formal arguments for the concrete analysis, and have shown that behavioural evidence from speech errors can be accommodated by both analyses and hence favours neither. Here diachronic evidence is examined in order to gain an insight into how [ŋ] was treated in the grammars which past native speakers constructed for themselves.

Since it is clear that in earlier English [ŋ] could not occur without a following [g] or [k], it is instructive to look at the circumstances surrounding the loss of [g] in [ŋg] clusters in the late C16/early C17. The writings of orthoepists and phoneticians of the period (Dobson 1968) indicate that the developments took place as follows:

Word finally. [ŋg] regularly changed to [ŋ].

Medially. Developments require the specification of two phonological environments (P_1 and P_2) and two morphological subcases (M_1 and M_2).

P_1 : before a nasal or non-sonorant.

P_2 : before a vowel or a non-nasal sonorant consonant.

M_1 : forms (such as *finger*) having original medial [ŋg] which did not occur word finally in any related form.

M_2 : forms (such as *singer*) having original medial [ŋg] which occurred word finally in a related form (viz. *sing* in our example).

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In M_1 [g] was regularly lost in P_1 and nearly always retained in P_2 . M_2 initially exhibited this same phonologically conditioned development. There then arose a period of fluctuation between [ŋ] and [ŋg] in both P_1 and P_2 , which settled down only in the middle of C17, leaving [ŋ] and [ŋg] in approximately their modern distribution.

Let us now compare how abstract and concrete analyses of [ŋ] account for these data. The abstract analysis assumes that the loss of [g] was brought about by rule addition and that UR did not change. However, the evidence just cited indicates that g-deletion originally took place under purely phonetic conditions and that the morphological conditions in the present form of the rule did not become firmly fixed until the middle of the C17. This is also consistent with what we know about the way in which phonological change proceeds (cf. Hooper 1976:101-110, Campbell 1974:89). Intermediate developments are now difficult to account for. In the speech of Robinson (Dobson 1968:964), for example, we would need to claim that pre-suffixally the g-deletion rule was simultaneously undergoing (1) extension in some words to P_2 environments, where its phonological conditions were not satisfied, e.g. *lo[ŋ]er* (but cf. *si[ŋg]ing*), and (2) restriction in some words in P_1 environments, where its phonological conditions were satisfied, e.g. *belo[ŋg]s* (but cf. *ha[ŋ]s*).

While the lexical spread of such changes may not be problematical, it is not at all clear why such complications should be spontaneously added to a phonetically transparent rule. Indeed, three crucial questions are left unanswered:

1. What motivated the fluctuation between [ŋ] and [ŋg]?
2. Why did it not occur in word final position?
3. Why did it occur only in M_2 cases and not M_1 cases?

The most obvious answer to 1 and 3 is the tendency towards paradigm uniformity. However, the basic incompatibility of this concept with an abstract model of phonology is well known (Hooper 1976:94-95, Bynon 1977:132-135). Paradigm uniformity is a surface phenomenon which (unless one is prepared to admit global rules into the model) has no input to "higher" levels of phonological organisation and thus can not serve to direct their evolution. Nor is paradigm uniformity a consequence of some type of principled reorganization at higher levels. (Note that in the present case it results from neither simplification nor reordering.)

No such difficulties are faced by the concrete analysis. Following the loss of [g] under phonetic conditions phonological restructuring takes place in words showing no paradigmatic alternations. Consequently M_1 words show little subsequent fluctuation. In M_2 words where the loss of [g] does lead to paradigmatic alternations, however, analogy can operate lexically to introduce presuffixal [ŋg] in P_1 environments and presuffixal [ŋ] in P_2 environments. To avoid the objection that analogy is a loose concept, I will now provide a rigorous formulation of the processes involved.

Data cited by Hale (1971), Klausenberger (1974), Skousen (1973) indicate

that speakers tend to treat morphologically and semantically complex forms as derived from simpler forms. Similarly, Venneman's work on rule inversion (1972, 1976) demonstrates the tendency to take prepausal forms as basic and non-prepausal forms as derived. In the present case then there would be a tendency to derive suffixed forms from unsuffixed forms, which would have been affected by word final loss of [g]. If we assume these base forms were restructured, then firstly we would not expect to find [g] reappearing in word-final position. Secondly, we are able to provide a model for the observed analogical processes. In order to maintain the paradigmatic alternations, speakers would be required to insert [g] under certain phonological conditions in the course of deriving suffixed forms from base forms in [-ŋ]. Although the conditions under which g-insertion applies could be stated in purely phonetic terms, speakers could be expected to treat it as a morphonemic rule since it applies only during morphological operations. (The model adopted here owes much to Linell 1979.) Once morphologized this g-insertion becomes subject to two competing generalizations: it could be suppressed altogether or it could be extended to all suffixed forms. Suppression would lead to forms such as *si[ŋ]ing*; extension would lead to forms such as *si[ŋg]s*. Thus the fluctuation in the M_2 environments is seen here as a natural development rather than an unmotivated complication of the grammar.

Thus the concrete analysis answers all three of the questions posed above and provides an explanation for the evolution of independent [ŋ] which accords with known principles of historical change. I conclude that the concrete analysis provides a better explanation of the behaviour of native speakers with respect to [ŋ] in the early part of the 17th century. If native speakers preferred the more concrete analysis even at a time when the phonetic conditions under which both [ŋ] and [ŋg] occurred were clear surely they must show the same preference when, as in current English, the conditions are more obscure.

More generally, the data support a concrete approach to phonology which distinguishes sharply between phonological and morphophonemic rules and treats the latter as part of morphological operations.

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Some Notes on the Paradigmatic Dimension of Morphological Productivity*

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It is one of the most salient features of Aronoff's theory of word-formation, that the phenomenon of morphological productivity is assigned to such a prominent position.¹⁾ Given this interest in the dynamic aspects of word-structure, it is far from surprising that Aronoff has made an attempt to deal with morphological productivity within the framework of his—explicit—morphological theory. In concreto, the strategy that is followed by Aronoff boils down to following: the phenomenon of morphological productivity is conceived of as relating to direct properties of what in Aronoff's conception constitutes the main topic of morphological research, viz. the rules of word-formation.

Leaving aside all details, the central claim of Aronoff's views on morphological productivity seems to be that the productivity of each rule of word-formation depends completely on the direct properties of the base of that rule. Consequently the core of Aronoff's approach is, that no information other than what is stored in the base, is relevant in relation to the productivity of the rule in question. Given a rule such as

- (1) $[X]_{Adj} \rightarrow [un\#[X]_{Adj}]_{Adj}$ (Aronoff, 1976: 63),

all factors which may be relevant with respect to the productivity of this rule are considered to bear upon the—particularly *morphological*—properties of $[X]_{Adj}$ (on the left-hand side of the arrow).

It seems to be beyond all doubt that the approach sketched above leads to an interesting characterization of morphological productivity in cases such as the following: according to Aronoff (1976: 53), the English prefix *un#* 'attaches most productively to deverbal adjectives', a class of words including present and past participles and deverbal derivatives in *#able* (ibid.: 53). Crucial to statements of this type is, in fact, that they can be 'translated' into *positive* conditions on the (morphology of the) base of the rule of *un#* attachment. In order to deal with conditioning factors of this kind, Aronoff takes the line that rules such as the one in (1) are provided with a series of positive conditions relating to the morphology of the base. As a result, the rule of *un#* attachment is considered to be of the following format (Aronoff, 1976: 63):

- (2) *Rule of negative un#*

- (a) $[X]_{Adj} \rightarrow [un\#[X]_{Adj}]_{Adj}$
semantics (roughly) $un\#X = \text{not } X$

- (b) Forms of the base
- 1 $X_{\vee}en$ (where *en* is the marker for past participle)
 - 2 $X_{\vee}\#ing$
 - 3 $X_{\vee}\#able$
 - [...]

On the basis of this example we are able to make our above-mentioned characterization of Aronoff's views on morphological productivity somewhat more precise: (i) the 'direct properties of the base' that determine the productivity of a rule bear upon *the presence of specific 'morphemes' (viz. 'affixes') in the underlying word*, while (ii) it is exclusively in terms of the positive morphological conditions that these conditioning factors are given expression to.

In the remainder of this paper three cases will be discussed which, to our mind, have in common that they—irrespective of their no doubt highly diverse nature—support the basic claim of this paper, viz. the claim that

there are factors determining the productivity of a certain morphological process which do *not* bear upon the direct morphological make-up of the base of that process, i.e. which do *not* relate to the presence or absence of specific 'morphemes' in the base.

Case 1

A first indication in favour of our claim that the productivity of morphological processes may depend on factors other than the presence or absence of a certain affix in the base, can be observed in the coining of complex adjectives in *un-* in modern English. One of the time-honoured truths about the coining of this type of adjectives is—in essence this observation can be found in *The Oxford English Dictionary* already—that there is a class of 'short and simple adjectives' (cf. *OED*, *un-*, prefix¹), 7) that do not readily constitute the starting-point for prefixation in *un-*. In Zimmer (1964: 43–44) the restriction on *un-*prefixation at issue is defined as follows²):

un- prefixation is not applied to 'strictly monomorphemic' adjectives that have 'strictly monomorphemic' antonyms.

In conformity with the above 'rule', prefixation of *un-* to adjectives such as *good*, *bad*, *evil*; *long*, *tall*, *short*; *hot*, *cold*; etc. is not of a regular occurrence (cf. Zimmer, 1964: 44).

Crucial to the above impediment of *un-* prefixation is, of course, that the restriction at issue does not relate to 'a direct property of the base', in that it does not bear upon the presence or absence of a specific morpheme in the underlying word. What the restriction on *un-* prefixation actually has a bearing on, is the relationships holding within the class of simplex adjectives in modern English. For it is the relationship with its antonym *bad*, which renders *good* unsuited to serve as a potential base for *un-* prefixation. As a result, the hindering factors that *un-* prefixation encounters do not simply harmonize with

the basic idea of Aronoff's approach to morphological productivity, i.e. the idea that the productivity of a given process depends exclusively on the morphological composition of the base of this process³)

Case 2

In modern Dutch several classes of denominal derivatives in *-er* may be distinguished. In this connection I will concentrate on those derivatives in *-er*, which—in a preliminary wording—do not denote 'membership' or 'origin'. An example illustrating this pattern is e.g. *bromfiets-er* 'mopedallist', which is derived from the compound *bromfiets* 'moped'. A salient feature of the process at issue is, that *-er* attaches more readily to compounds than to simplex nouns. There is one class of compounds, however, on the basis of which the coining of denominal personal names in *-er* is particularly common, viz. the class of the (as we will call it) *verbal-parallel compounds*⁴).

Let me illustrate first, what we understand by 'verbal-parallel compound'. In modern Dutch—like in English—nouns may be converted into verbs, compare such 'pairs' as *film-en* 'to film' beside *film* 'id. (N)', *sport-en* 'to practice a sport' beside *sport* 'id. (N)', etc., whereas these verbs, in their turn, may underlie 'agent nouns' in *-er* (cf. *film-er* and *sport-er*). Further, nouns may function as head of several types of compounds; in *lachstuijp* 'convulsion of laughter' (lit. 'laugh-convulsion'), for instance, the noun *stuijp* 'convulsion' figures as the head of a V + N compound, while the noun *dier* 'animal' is the head of a N + N compound in a case as *waterdier* 'aquatic animal' (lit. 'water animal'). Verbal-parallel compounds then, are those compounds, whose nominal head is paralleled by a verb that is its 'conversion-mate' and—not infrequently—by the related 'agent noun' in *-er*. Examples of this type of compounds are e.g. *lachfilm* 'humoristic film' (lit. 'laugh-film') and *watersport* 'aquatic sports' (lit. 'water sport'). The nouns that figure as head of the compounds in question—viz. *film* and *sport*—are, as we have seen, paralleled by verbs that have come into being as a result of conversion (and which may underlie an 'agent noun' in *-er*), in consequence of which *lachfilm* and *watersport* are verbal-parallel compounds.

As was hinted at above, the coining of the type of denominal derivatives in *-er* that we concentrate on is particularly popular on the basis of verbal-parallel compounds (resulting in e.g. *lachfilm-er* and *watersport-er*). An explanation of this is not hard to give. The by far most prominent process of coining in *-er* is on the basis of verbs. The denominal counterpart of this latter process then, is as it were 'stimulated' by the intimate relationship of the verbal-parallel compounds with verbs (and, not infrequently, with the related 'agent noun' in *-er*). Nouns with clear-cut 'verbal ties', to put it differently, rank for derivation in *-er* more readily, than nouns lacking such relationships.

As was the case in the preceding example, we cannot but ascertain that the factors determining the productivity of the coining of denominal derivatives in *-er* do not exclusively relate to the direct morphological composition of the

underlying words. As in our first example, the productivity of the denominal coining in *-er* depends on the 'relationships with other words' that the forms constituting the base display.

Case 3

Our third and final example is of a somewhat different nature. From Schultink's study of the morphological properties of the simplex adjective in modern Dutch we can learn that in this language there are two types of highly similar deadjectival personal names, viz. one in *-erd* ([ərt]) and one in *-aard* ([art])⁵. Compare (3) where we have listed some examples:

- (3) (a) *deadjectival personal names in -erd*
- | | | | |
|-----------|----------------|-------|--------------------|
| bang-erd | 'coward' | bang | 'afraid, cowardly' |
| flauw-erd | 'silly fellow' | flauw | 'insipid, silly' |
| suff-erd | 'dull fellow' | suf | 'dull' |
| etc. | | | |
- (b) *deadjectival personal names in -aard*
- | | | | |
|-------------|--------------|--------|----------|
| gierig-aard | 'miser' | gierig | 'stingy' |
| lui-aard | 'lazy-bones' | lui | 'lazy' |
| wreed-aard | 'cruel man' | wreed | 'cruel' |
| etc. | | | |

Of particular interest, in this connection, is the productivity of the personal names in *-aard*. Schultink's interpretation of the morphological productivity of the word-type in question can best be summarized as follows: the productivity of the formations in *-aard* is restricted to those cases where the coining in *-erd* is 'uncommon' (but note, *not* impossible)⁶. In concreto, the productive coining of new words in *-aard* is, according to Schultink, restricted to bases ending in [əx], since it is precisely these adjectives on the basis of which coining in *-erd* is 'unusual'⁷.

To our mind, Schultink's finding that there is a question of a connection between the —highly restricted— productivity of the formations in *-aard* and the impediment that the far more general coining in *-erd* meets with, cannot be adequately given expression to within the framework of Aronoff (1976). For, given the fact that in Aronoff's conception the factors determining the productivity of a rule relate exclusively to the —particularly morphological— properties of the base, we must content ourselves with ascertaining that —as far as the coining of new words is concerned— the rule of *-aard* suffixation is restricted to adjectives in [əx]. Consequently, within the framework of Aronoff (1976) we lack the means to give expression to the fact that this restriction on *-aard* suffixation is, above all things, the result of a hindrance that the process of *-erd* suffixation experiences. The productivity of *-aard* suffixation, to put it differently, does *not* primarily depend on a —in this case phonological— property of the base, but the scope of suffixation in *-aard* is first of all delimited by the process of coining deadjectival personal names in *-erd* and the impediments

that this latter process encounters.

As has been pointed out above, the common characteristic of the three above-mentioned morphological processes is, that their productivity is determined by factors which do not have a bearing on the morphological composition of the base. In the former two examples it was the relationships of the words constituting the base with other words that—respectively, negatively and positively— influenced the productivity of the process at issue, whereas in the third example it was the influence radiated by a rival process which turned out to be a determining factor. To our mind, the above examples unequivocally indicate that the productivity of a given morphological process cannot be studied in isolation from the morphological system it forms part of, in that the 'position' of both (i) the words forming the base, and (ii) the morphological process as such within the system as a whole, may turn out to be of decisive importance. The examples indicate, to put it differently, that the word-structure component of natural languages should not be conceived of as a mere inventory of separate rules, but that we are dealing with a *system* in which both the processes as such and the words they act upon may be interrelated in several ways. The investigation of the nature and scope of these latter relationships forms one of the major topics of the study of the *paradigmatic dimension* of morphological structure, and it is with the above examples that we hope to have made clear that no insight in the phenomenon of morphological productivity can be obtained, if the importance of these paradigmatic relationships is not discerned⁸⁾. In our view, it is particularly in this respect that Aronoff's theory of morphological productivity is deficient.

Notes

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1) Compare Aronoff (1976: chapter 3), Aronoff & Schvaneveldt (1978), and Aronoff (1981). Like Aronoff, I will concentrate on the more or less 'variable' factors that determine morphological productivity. Compare Van Marle (in prep.) for a further discussion of this point.

2) Compare Henzen (1957: 99) and Schultink (1962: 125) for similar remarks with regard to the coining in *un-* in modern German and the coining in *on-* in modern Dutch. Moreover, compare Schultink (1962: 125) for a further discussion of the restriction on *un-/on-* prefixation at issue.

3) Note, that our conclusion will not be different if the view is taken that the restriction on *un-* prefixation should be conceived of as the result of some sort of blocking-device. For in that case too, it is no direct property of the base which prevents the coming into being of *un-good*, *un-bad*, etc. Observe finally, that the phenomenon at issue can be characterized in terms of a positive conditioning as well, viz: monomorphemic adjectives lacking a morphemic antonym rank for prefixation in *un-* more readily than monomorphemic adjectives which have such an antonym.

4) In this connection our characterization of the morphological properties of these antonyms cannot be but tentative, of course.

5) With Schultink we are of the opinion that *-aard* should not be conceived of as an 'allomorph' of *-erd*; both *-erd* and *-aard* should be conceived of as independent suffixes. As to the class of 'actual words', namely, the domains of *-aard* and *-erd* overlap nearly completely.

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Moreover, even as far as the coining of new words is concerned, there is no question of a specific domain in which *-erd* is precluded by *-aard* (cf. below).

6) As a result of which Aronoff's blocking-device cannot be invoked.

7) With e.g. Schultink (1961) and Booij (1977: 122 ff) we take the line that phonological factors may determine the productivity of a certain morphological process as well.

8) Further remarks on the paradigmatic dimension on morphological creativity—and morphological structure in general—can be found in Van Marle (in prep.).

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Categorial Morphology

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0. Ideally, in a categorial morphology input- and output categories encode their distributional properties like plugs and contacts. Impossible derivations simply would be wrong concatenations of functors and arguments, that is: of bound morphemes and free morphemes. For example, the English suffixes *ity* and *ness* never combine, since they are of the same type $T_a/(CN/CN)$. They both take adjectives as arguments and yield abstract terms as values. Thus *electricityness* and *goldness* are category mistakes of the same sort, in the sense that the functor *ness* is not subcategorized for taking abstract terms as arguments, *electricity* and *gold* being abstract nouns functioning as proper names of abstract entities. It is my claim that the problem of affix distribution, for the most part, can be tackled by an appeal to their categorial types. However, there are also other factors reducing the set of theoretically possible derivations which are of a phonological and morphological nature. So the ideal of a mere categorial morphology is hampered by rather pedestrian demands regarding the articulatory and compositional structure of derivations. Phonological restrictions, according to Aronoff and Siegel, pertain to segmental and accentual properties of the base of a derivation. One of Aronoff's examples of a segmental condition is the blocking of the suffix *ish* in case of words which also end on a coronal fricative. For that reason *fishish* is out but *piggish* is not. Suffixation in Dutch is also, in a few cases, sensitive to segmental properties of the base, as is observed by Zwarts for the abstract term forming suffix *te*, which does not attach to consonant clusters the first being a non-coronal sonorant. So *lompie* (*rudeness*) is out and *sterkte* (*strongness*) is in. Morphological conditions on the base, such as the feature [Latinate] in the English lexicon, also hold for Dutch in the sense that affixes borrowed from Latin or French do not combine with native words, although there are some exceptions in this area. But if native words are themselves derivations, then they can never be inputs for a non-native derivational rule, as is observed by Van Marle. Without reference to the origin of the affixes, this fact can be stated in terms of the impossibility of root affixes occurring outside of word (stem) affixes. This situation partly overlaps with the prosodic facts covered by the Ordering Hypothesis developed by Siegel and Aronoff, given that all non-native affixes are stress sensitive or Class I, cyclically preceeding stress neutral or Class II affixes. However, as is noticed by many, the Ordering Hypothesis is confronted with obstinate cases, baptized as Boundary Paradoxes (Aronoff). An Aronoff example of a sequence which is ruled out is *analyzability*, where *able* is Class II and *ity* is Class I. A

Schultink example is *lerares* (female teacher), *aar* being Class II and *es* being Class I. Although there is no logical connection between cyclic stress assignment and level ordered morphology, the division of Class I and Class II affixes in English is based on pre- and postcyclic stress assignment. But since Dutch word stress, as opposed to English, is not cyclic, the Ordering Hypothesis can not be stated in terms of the cyclic stress properties of affixes. As an alternative, I will argue that the distribution of affixes is a manifestation of a more general phenomenon, which is the sensitivity of affixes for the categorial status of the base words. In a limited set of cases, the categorial system is overruled by phonological and morphological demands. In the next section I will jump aside dwelling on Dutch stress in order to show the main differences with English and in the final section I will give a categorial typology of a representative set of Dutch native suffixes. The moral is that it is not level ordering but categorial type that accounts for the distributional facts.

1. According to Schane, English word stress generally is either antepenult or penult. In the latter case the prefinal syllable is heavy. Final stress points to French borrowing and is in need of lexical marking. The stress patterns of polysyllabic monomorphemic words, as in the celebrated pair *Win²nepesau¹kee* and *Monongah²ela¹*, are obtained by the iterative application of a word level stress rule. On the other hand, the stress patterns of derived words are assumed to be a function of the component parts. So, for example, the location of primary and secondary stress in *theat²rical¹ity* is not iteratively assigned, but cyclically. Dutch stress is simpler. Suppose *Winnepesaukee* and *Monongahela* were Dutch words. Then primary stress would be on the penult syllable, as in English, and in both cases secondary stress would be word initial. Dutch non-primary stress is not to be derived by iterative or cyclic application of a word stress rule. Rather a surface rhythmic constraint is apparently at work, forcing the secondary stress to be at a maximal distance from primary stress. In order to observe the differences compare the following pairs:

*Italy¹ Italian¹ / Itali¹e² Italiaan¹, Aristotle¹ Aristotelian¹ / Aristoteles³
Aristoteliaan³, theater²¹ theatric²¹al / theater¹ theatraal¹.*

In the above examples the Dutch words have final stress, since the suffixes show up strong clusters. Some suffixes, however, are stressless, although they also show up strong clusters: For example *heid* and *schap*. They can be called 'Strippers', since a word level stress rule neglects them. Stress determining suffixes, 'Magnets', locate primary stress on the penult syllable of the derived word and stress bearing suffixes, 'Stressers', always show up with strong clusters. Without resource to the cycle, Stressers and Magnets are responsible for the location of primary stress. In a sequence of a Stripper and a Magnet, as in *wetenschappelijk* (scholarly, scientific), even the Stripper receives main stress,

and in a sequence of two Stressers the first one is stressless, leaving no trace behind of an allegedly previously assigned primary stress, as in *nationaliteit*, to be compared with *nationaal* and *natie* (English *nationality*, *national* and *nation*). So, there are grounds to dismiss the phonological cycle and the cycle dependent Ordering Hypothesis for Dutch, making way for a categorial account.

3. Basic syntactic categories

T	Term; individual proper name. T_a is an abstract proper name.
CN	common noun
CN/CN	adjective, abbreviated as A
IV	intransitive verb
TV	transitive verb

(Comment: T can be given feature [-count]; CN [+count]. Where whatever verb can serve as an input category V is used as an abbreviation).

Morphological functor categories

CN/CN	Female suffixes <i>es</i> and <i>in</i> ; Stressers. Diminutive suffix (<i>e</i>) (<i>t</i>) <i>je</i> (allomorphs); Stripper. Semantics: 'subset selectors'.
T_a /CN	<i>schap</i> ; Stripper. Like English <i>ship</i> and <i>hood</i> . Semantics: 'set abtractor'.
T_a /A	<i>heid</i> ; Stripper. Like English <i>ness</i> . Semantics: 'property abtractor'
CN/V	<i>er/aar</i> ; Stripper. Like English <i>er</i> . No semantic effect on verb meaning, since both CN and V denote a set. <i>ing</i> ; Stripper. Like English <i>ing</i> . Semantics: 'activity or result of an activity'. <i>sel</i> ; Stripper. Like English <i>ing</i> . Semantics: 'result of an activity' <i>erij</i> ; Stresser. Like English <i>ery</i> . Semantics: 'location of an activity'. (Both <i>ing</i> and <i>erij</i> can have T_a as values)
A/V	<i>achtig</i> (stripper) and <i>ig</i> (Magnet). Like English <i>ish</i> . Semantics: 'fuzzy operator'.
A/CN	<i>elijk</i> ; Magnet. Like English <i>ish</i> and <i>like</i> . Semantics: 'fuzzy operator'. This suffix serves a mere transpositional function as well.

The functor categories encode the type of syntactic operation they perform, which is the concatenation of functor and argument categories, the left category of the functor being the output or value of the operation. One can easily imagine other morphological operations, such as substitution or zero transposition. But concatenation is most common in Dutch, although some dispute actually exists about substitution (or truncation). The semantics should be a model theoretic procedure consisting of a compositional mixing of the component parts of the derivation, to be specified by the relevant morphological functions. This of course is not easy, but the above names marked with inverted commas

should give an indication of what an intuitive semantics would entail. Technicalities can be found in the work of Dowty ('Word Meaning and Montague Grammar').

The distribution of affixes is not just categorially determined. For example, Magnets preferably attach to monomorphemic words; a peculiarity which has to be stated as a morphological condition on the input expression. There are, however, derivations in which a Magnet is preceded by a Stripper, as is the case in *wetenschappelijk* (*scholarship-ish*) and *verraderlijk* (*traitorous*). Those words are exceptions for the Ordering Hypothesis, but in a categorial account they also need special treatment: There is a small closed set of derived words that can combine with *lijk*, although this suffix generally only combines with non-derived words. Of special interest is the diminutive suffix which is, in a sequence of suffixes, always the peripheral one: *tekenaresje* (*little female drawer*) is wellformed, though in conflict with the Ordering Hypothesis (*aar + es + je* = Class II + Class I + Class II), but *tekenaartje-es* is not. This fact mirrors, I presume, the marked word order of 'female little drawer', the markedness being due to semantics or pragmatics rather than syntax.

A categorial analysis can give, apart from phonological and morphological considerations, an explanation for sequences of suffixes where the Ordering Hypothesis fails, as in *tekenares*, and where that hypothesis simply has nothing to say, as is the case wherever an impossible word is derived which does not show the prohibited Class II followed by Class I.

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On the Compositionality of German Prefix Verbs

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Verbs of movement play a prototypical role in the organization of the lexicon. This can be seen by a number of processes as, e.g., prefixation, auxiliarization, change of syntactic valency, and idiomatization. Since German makes extensive use of these processes, it is chosen here for a case study.

In German, prefixation of a verb of movement with a local particle (preposition or adverb) is a rather productive process. It leads to both semantically transparent and new idiomatic verbs. However, it seems to be predictable under what conditions idiomatization can take place. Moreover, idiomatization can be hindered by the additional prefixes *hin* and *her*. Some prefixes are able to form twins: a separable and an inseparable prefix verb (incidentally only one of them is lexicalized). The set of prefixes with this property is {*durch*, *um*, *hinter*, *über*, *unter*} ('through, around, behind, over, under'). All other prefixes form separable prefix verbs. Separability means that only the stem is affected by Finite Verb Fronting and Perfect Participle Formation. As a tendency, a separable prefix verb omits the otherwise required prepositional object, hence reducing the number of required complements by 1, whereas an inseparable prefix verb turns the prepositional object into a direct object. The choice between these processes is stimulated by certain stereotypical features of the context of use. These features can also induce different uses of a verb with a separable prefix. As there will be shown, the same features control the possibility for idiomatization so that not every idiomatic use must be learned separately, it can often be derived. In its core, lexical extension of this sort can be reconstructed by semantical rules. The impact of these processes for a general theory of the lexicon will be discussed.

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Chukchi Vowel Harmony: abstractness versus complexity

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The phenomenon of vowel harmony in Chukchi, and the analyses of it which have appeared in the literature, raise a variety of fascinating questions. The one of major concern in this paper is that of abstractness, primarily in the sense in which a phonemic inventory is abstract in so far as it differs from the phonetic inventory which realizes it. We will contend that to pursue concreteness of a certain sort in the analysis of Chukchi vowel harmony is to obscure the essence of the process and to impede the natural resolution of a number of problems.

As described in Skorik (1961), each Chukchi word belongs to one of two classes with respect to vowel harmony. Words of class I contain vowels from the set:

e o a ə

while words of class II contain vowels from the set:

i u e ə

Thus class I words are restricted to non-high vowels, and class II words to non-low vowels.

There are two related classes of morphemes with respect to vowel harmony. Morphemes of class I always appear in words of class I, and show no harmonic vowel alternations. Morphemes of class II, on the other hand, may appear in words of either class, and show the following characteristic harmonic alternations:

in a class I word

e

o

a

in a class II word

i

u

e

Note that ə is a neutral vowel, without either distributional restrictions or alternations with respect to harmony.

To illustrate, consider the following partial noun paradigms:

- (1) umqə 'polar bear'
umqet 'polar bears'
umqenu 'as a p. b.'

- (2) wopqə 'elk'
wopqat 'elks'
wopqano 'as an elk'

omqajpə 'from a p. b.'	wopqajpə 'from an elk'
omqaytə 'to a p. b.'	wopqaytə 'to an elk'
ɣumqete 'by a p. b.'	ɣawopqata 'by an elk'
ɣomqama 'with a p. b.'	ɣawopqama 'with an elk'

The fourth, fifth and seventh forms in (1) as well as all the forms in (2) are class I words; the rest class II words. The noun stem meaning 'elk' and the case suffixes here glossed as 'from', 'to' and 'with' are class I morphemes; the rest class II morphemes.

Since no word or morpheme may have more than four contrasting vowels, it seems reasonable to assume four vowel phonemes. We propose as the Chukchi phonemic vowel system:

/i/ /u/ /a/ /ə/

This in turn implies as phonemic representations for those morphemes which occur in (1) and (2):

/umqa/	'polar bear'
/wupqa/	'elk'
/t/	absolute plural suffix
/nu/	designative case suffix
/jpə/	ablative case suffix
/ɣtə/	dative/allative case suffix
/ɣa/	ergative/comitative case prefix
/ta/	ergative case suffix
/ma/	comitative case suffix

The distribution of phonetic vowels may now be formulated in a straightforward fashion.

(3) $V \rightarrow$ [-high] / in class I words

(4) $\begin{matrix} V \\ [+low] \end{matrix} \rightarrow \begin{bmatrix} -low \\ -back \end{bmatrix}$ / in class II words

It would of course be desirable to state rule (4) by analogy with (3) as simply raising low vowels. The major impediment to this is the phoneme /ə/, with which /a/ must not merge. There is reason to believe that ə is historically secondary, and many ə's are predictable either by morphological process (as in the first forms in (1) and (2)), or alternate with zero according to the syllable structure. There seems to be a residue, however, which resists explanation in either way (as in the fourth and fifth forms in (1) and (2)).

Of course (3) and (4) as they stand give no account of morpheme classes in particular of why class II morphemes show harmonic alternations, but not class I morphemes. Let us interpret 'class I-ness' as a morphological property [+H]; then 'class II-ness' is [-H]. The morpheme classes correspond to the lexically specified values for this feature. There is a harmony rule which spreads [+H] within the word.

$$(5) \mu \rightarrow [+H] / \left\{ \begin{array}{c} -\mu^* [+H] \\ \mu \\ [+H] \mu^* - \end{array} \right\}$$

In (5), ' μ ' represents an arbitrary morpheme, and we assume that morphological features are accessible to phonological rules so that the environments of (3) and (4) may be rewritten in terms of [H] as determined by (5). Thus lexically [+H] morphemes are harmonically 'dominant'; the lexically [-H] 'recessive' morphemes become [+H] when 'in the presence' of a 'dominant' morpheme, in the sense of (5). The rule might well be given an autosegmental interpretation, though this is beside the points we are focussing upon.

This completes the analysis we are proposing; it is now time to correct a possibly misleading aspect of the discussion. The output of rule (5) followed by (3) and (4) is not a fully phonetic representation. In fact, the forms in (1) and (2) are cited in a transliteration of Skorik's (1961) phonemic representation. The relevant further detail may be summarized by saying that *e* subsumes [e] and [ɛ]; *o* subsumes [o] and [ɔ]; and *a* subsumes [æ] and [ɑ]. These variants correlate with the *lexical* values of [H]:

[+H]	ɛ	ɔ	ɑ
[-H]	e	o	æ

While Skorik's phonetic descriptions are rather vague, Krause (1980) plausibly interprets [H] phonologically as [tense] for these vowels. This leads him to choose the phonetic feature [tense] as the primary parameter for vowel harmony in Chukchi. Thus he arrives at an eight vowel phonemic system:

/i/ /u/ /e/ /ɛ/ /ʌ/ /ɐ/ /ɔ/ /ɑ/ /ə/

together with the following harmony rule:

$$(6) \begin{array}{c} V \\ \left[\begin{array}{c} +\text{tense} \\ \langle -\text{high} \rangle \\ -\text{back} \end{array} \right] \end{array} \rightarrow \begin{array}{c} \left[\begin{array}{c} -\text{high} \\ +\text{low} \\ \langle +\text{back} \rangle \end{array} \right] \end{array} / \text{ in the presence of } \begin{array}{c} V \\ [-\text{tense}] \end{array}$$

Rule (6) is not exactly the same as Krause's, largely due to differences in the treatment of 'recessive' ə. Skorik says that there is a difference without clearly describing it. Krause assumes a mid high lax /ɜ/; and it causes problems for his analysis. Our version as a mid central tense /ʌ/ is more coherent, but seems inconsistent with what Skorik does say. It is also the case that the high vowels are phonetically lax; an adjustment rule to lax high vowels and /ʌ/ will eliminate the inconsistency while leaving open the real nature of the variants of ə.

$$(7) \begin{array}{c} V \\ \left\{ \begin{array}{c} [+high] \\ -low \\ +back \\ -round \end{array} \right\} \end{array} \rightarrow [-\text{tense}]$$

The argument that the analysis with (3), (4) and (5) is preferable to that with (6) and (7) cannot be directly empirical, since both analyses 'work' equally well given our understanding of the facts. The key observation is that (6) is not a vowel harmony rule in any intuitive sense: it does not change any phonetic property of one vowel toward that of another. In fact, the effect is precisely the opposite so far as the supposed harmonic parameter [tense] is concerned. And there will need to be an additional statement to the effect that tense and lax vowels may not cooccur in the same morpheme. (6) is complex, obscure in a way unrelated to feature notation or bracketing, and problematic from the point of view of constraining phonological theory; (3), (4) and (5) are clear, simple, and 'modular', though we have not shown that they function independently of one another.

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Dimensions of Perception for Italian Consonants: Multidimensional Analysis

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A number of important studies have been carried out in order to determine the perceptual correlates of distinctive features and their psychological reality, without directly taking into account their articulatory and/or acoustic characteristics. The assumption that the mental representation of linguistic sounds is, at least in part, organized in distinctive features has had an important implication in the perceptual domain, that is, it has suggested the idea of defining a *distance* between linguistic sounds.

It is possible, in fact, to set up a series of distances between linguistic sounds and then to try to represent them in a space configuration so that spatial distances correspond to perceived differences in the sound similarities.

The concept of a perceptual space permits a logical distinction between perceptual processes that locate stimuli at positions in the space and subsequent decision processes that make use of the information contained in the representation.

A difficulty with the concept of a perceptual space is that the space is not directly observable and, therefore, must be inferred by indirect methods.

Presumably, if we are able to understand and model the decision process of the observer in a particular task sufficiently well, then we should be able to work backwards from the observers' judgments to the structure of the space underlying them.

We might do this in the context of various tasks: identification, classification, similarity judgment, dissimilarity judgment, association measures, etc.

Considerable effort has been expended in recent years in modelling the similarity-judgment process. One product of these efforts is a collection of multidimensional scaling (MDS) procedures, each of which is designed to abstract the dimensions of the underlying perceptual space, and the loci of the stimuli within that space, from judgments of stimulus similarity or other measures of proximity.

The main assumption of this type of analysis is that stimuli are encoded internally in terms of continually varying parameters or dimensions. The aim of multidimensional techniques is to detect the number of the dimensions which are relevant for the perception of the stimuli, and to determine their co-ordinates on each dimension (that is the dimensionality) and the stimuli configuration in a multidimensional space: the configuration would mirror the deep structure of

the data.

The resulting perceptual dimensions can be used to examine the complex relation between distinctive features and acoustic cues.

The purpose of the present investigation is to find by means of MDSCAL and INDSCAL analysis: 1) The location of 21 Italian consonants in the perceptual space; 2) the perceptual dimensions and their weights for similarity judgments of these consonants; 3) whether or not dimensions could be interpreted in terms of phonological features.

DATA COLLECTION

We examined the 21 Italian consonants in initial syllable position followed by the vowel /a/. The 21 syllables were pronounced by a trained female radio speaker, and were recorded by means of a professional apparatus. The tape was fed into a digital computer (PDP 1114), with 16 KHz sampling frequency and low pass filtering at 7 KHz. The intensity of all syllables was normalized.

The procedure used for data collection is the diadic comparison with seven-point scaling. In preparing stimulus frames for diadic comparisons each stimulus was randomly paired with all other stimuli. The 210 pairs of stimuli were presented twice—in AB and BA order—for a total of 240 pairs. Moreover we added 21 same pairs as a measure of subject and stimulus reliability. The final list resulted in 441 total stimulus frame.

The stimuli were recorded with 4 sec interstimulus interval and a 15 sec pause after 20 presentations, in three different test sequences. All three tapes were reproduced on a Revox B77 tape recorder and presented binaurally through Beyer DT48 headphones, to 30 subjects (one tape for every ten subjects), at level 85 dB SPL. The subjects were University students (15 females and 15 males), speaking regional Italian of Veneto. They were normal hearing subjects and phonetically naive.

MDSCAL Analysis

In order to obtain a single measure of the similarity of two phonemes, the ratings of the 30 subjects were averaged. The entire set of data were then reduced to a single full matrix, from which was obtained through further averaging the half matrix Δ of dissimilarity among phonemes (see Tab. 1.).

As these dissimilarities do not represent real distances, but subjective evaluation of distances, they contain an error which, in MDS, is corrected by minimizing a function called stress (S) (see Kruskal, 1964). The best R number of the spatial dimension of the stimuli is generally determined according to the interpretability of the configuration and to the stress variations as a function of the number of dimensions, as there are no reliable automatic procedures for determining how many dimensions give the best approximation to the ideal configuration.

The most widely used procedure is to examine the stress value as a function of the number of dimensions. Shepard (1973), however, states that inter-

pretability of solution rather than stress reduction is a more reliable way to determine the number of dimensions.

The initial configuration used for each R is L type configuration because it gives lower stress values than others (e.g. than the configuration which, for each R, takes into account the final configuration of the preceding R).

Data matrices were analysed in 1-6 dimensional space in both Euclidean ($r=2$) and city-block ($r=1$) metrics. Observing the configurations obtained, it turns out that the most suitable metric for measuring the distances between our stimuli is the Euclidean metric. The city-block metric, in fact, gives higher stress values and non interpretable configurations.

Also the scatter diagram of $d\delta$ shows that the distances fit the perceptual dissimilarities rather well.

By analysis of stress variation as a function of dimensions, using Euclidean metric, monotone regression and initial L configuration, the best dimensionality seems to be $R=5$.

The final configurations are projected on the planes of dimensions, taken two by two. For $R=5$ the dimensions D_1 and D_4 divide the consonants in two large groups as in Fig. 1. D_1 is interpreted as *Palatal* as it clearly distinguishes \int , $t\int$, $d\int$, η , λ from the nonpalatal constants.

D_4 can be interpreted as *Continuant* as it separates plosives and affricates

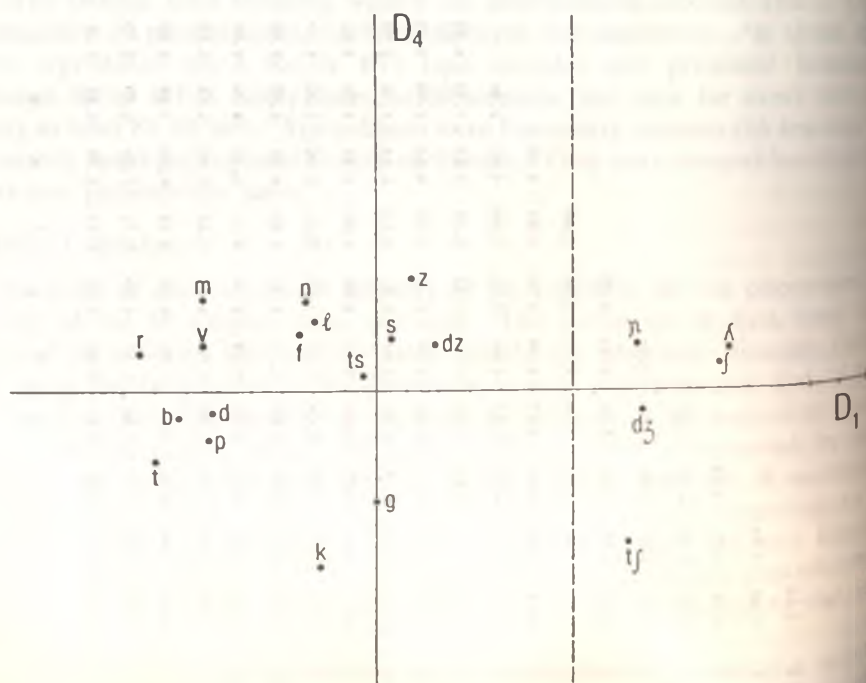


Fig. 1

from all others.

In Fig. 2 are shown dimensions 2 and 3. Dimension 2 separates the sibilants (s, z, ʃ, ts, dz) from all other consonants, D_3 separates lateral consonants from all others. Fig. 2 shows, however that these groups are not categorical: the consonants are in fact distributed along D_2 according to their manner of articulation; this structure, within the groups might be due either to other categorical features or to some continuous properties of the stimuli.

Observing Fig. 1 and 2 we can point out that:

1. if we interpret D_4 as \pm *Continuant*, the nasal consonants turn out to be + continuant;
2. the affricates /ts/ and /dz/ are always grouped with the homorganic fricatives (s, z), as these affricates do not exist in the phonematic system of our subjects;
3. /r/ is singled out from the other consonants nearly in all configurations.

However one disadvantage of MDSCAL method is that while the configurations are stable, the coordinate systems are not. The orientation of the space is arbitrary and the fact that there are standard rotation algorithms does not make it less arbitrary. Hence only the constellation of points has psychological reality.

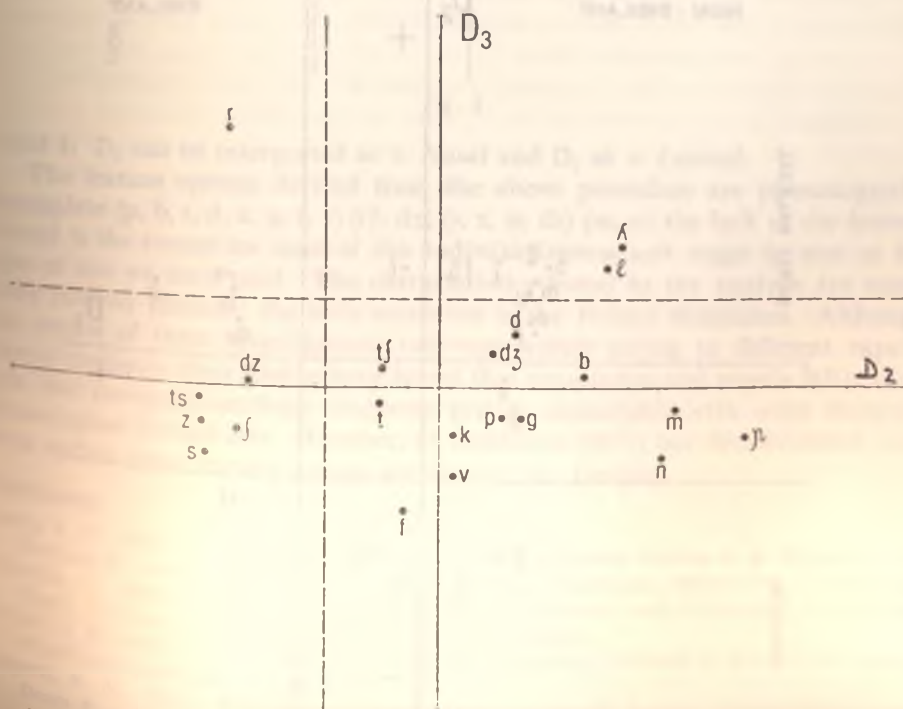


Fig. 2

INDSCAL Analysis

The INDSCAL approach instead, is not subject to the rotation of axes problem. INDSCAL differs from other MDS methods in assuming that different individuals perceive a given set of stimuli in terms of the same set of perceptual dimensions but with different weights (or saliencies) applied to these shared dimensions. According to this model, interstimulus distance is determined by a weighted generalization of the Euclidean metric in which the contribution of each dimension to distance is modified by an individual's salience weight for that dimension. An important property of this method is that the axes of the derived space are not subject to rotation and, consequently may be interpreted as corresponding directly to the observer's perceptual dimensions (Carrol, 1972).

We applied INDSCAL technique to the matrices of the responses of the 30 subjects. The scaling was repeated in a two to five dimensional space with several different initial configurations, the clearest interpretation was found for the four-dimensional space ($R=4$), whose interpoint distances are correlated at 0.708 with the data. In Fig. 3 are shown Dimension 1 and 2: Dimension 2 can be interpreted as \pm Palatal, Dimension 1 as \pm Sibilant. Fig. 4 shows Dimension

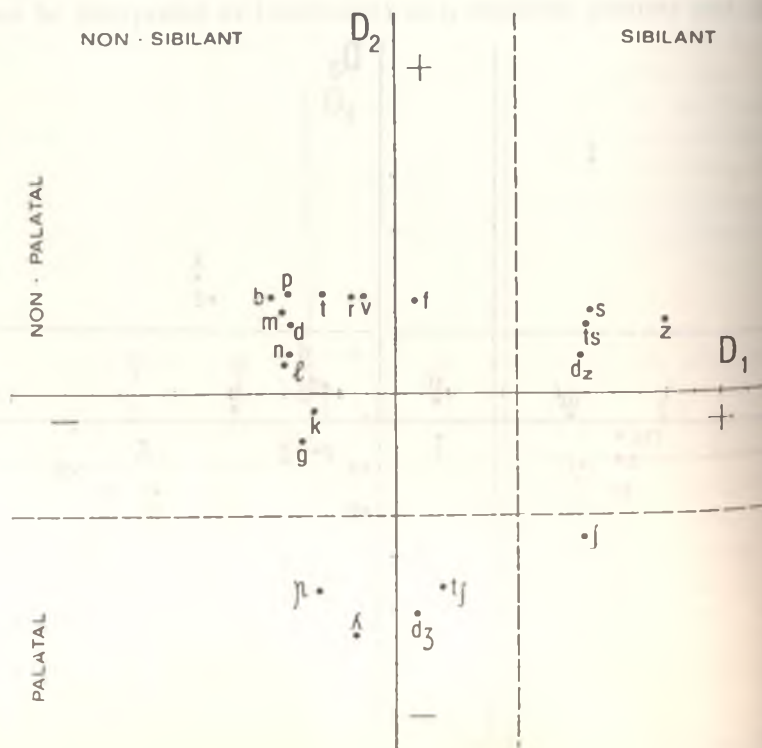


Fig. 3

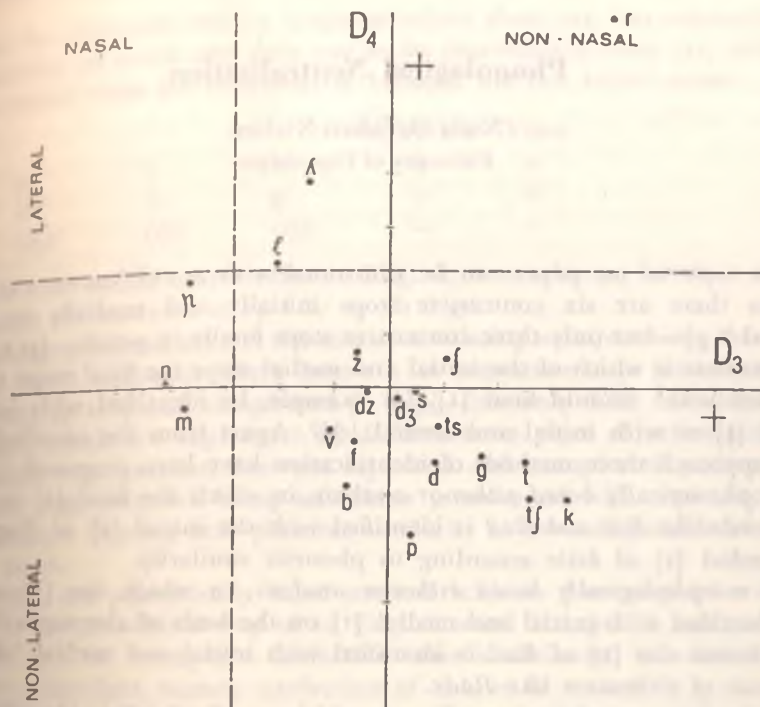


Fig. 4

3 and 4: D_3 can be interpreted as \pm Nasal and D_4 as \pm Lateral.

The feature systems derived from the above procedure are phonologically incomplete (p, b, t, d, k, g, f, v) (tʃ, dʒ) (s, z, ts, dz) (m, n) the lack of the feature *Voiced* is the reason for most of this indistinctiveness and might be due to the type of test we have used. The characteristics found in the analysis are essentially manner features: the only exception is the *Palatal* dimension. Although the results of these investigations can vary widely owing to different experimental methods, these studies have found that consonants and vowels fall readily into low configuration/high similarity groups isomorphic with some standard phonological feature sets. However, as Goldstein (1977) has demonstrated relations within these feature groups are usually not random.

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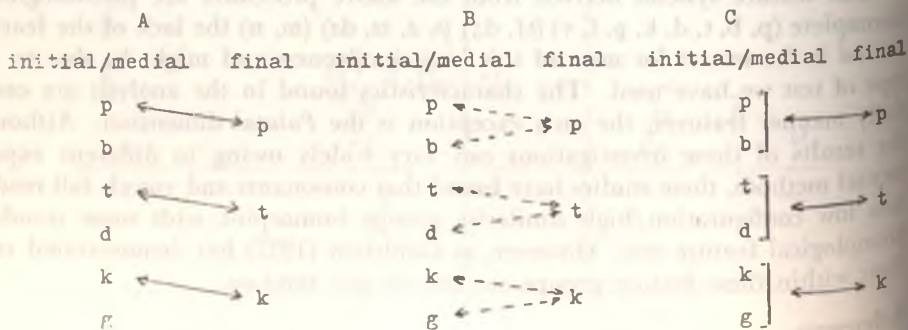
Phonological Neutralization

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The topic of my paper can be illustrated with a well-known example: in German there are six contrastive stops initially and medially in words—[p b t d k g]—but only three contrastive stops finally in words—[p t k]. The problem here is which of the initial and medial stops the final stops should be identified with? Should final [t], for example, be identified with initial and medial [t] or with initial and medial [d]? Apart from the so-called polysystemic approach three methods of identification have been proposed:

- A *phonetically based either-or analysis*, in which the final [t] in German words like *Rat* and *Rad* is identified with the initial [t] of *Tag* and the medial [t] of *bitte* according to phonetic similarity.
- A *morphologically based either-or analysis*, in which the [t] of *Rat* is identified with initial and medial [t] on the basis of alternants like *Rate*, whereas the [t] of *Rad* is identified with initial and medial [d] on the basis of alternants like *Rade*.
- A *both-and analysis*, according to which any final [t] is identified simultaneously with initial-medial [t] and initial-medial [d].



According to the *phonetically based analysis* the final stops in German represent the phonemes /p t k/. The phonemes /b d g/, on the other hand, are assumed to be absent in word-final position. This means that the analyst operates with *defective distribution* and rejects the concept of neutralization. This method is connected with the following problems:

First, it leads to arbitrary decisions in the case of *intermediate manifestation*.

This can be illustrated with a language where there are two contrastive stop series initially in words and only one series post-initially after /s/, and where the post-initial stops are intermediate between the two initial series:

			Aspiration	Voice
p-	t-	k-	+	—
b-	d-	g-	—	+
(s)p-	(s)t-	(s)k-	—	—

In such a case, which nearly exists in English, it is arbitrary whether the post-initial stops are analysed as /p t k/ or /b d g/.

Secondly, problems arise in the case of *oscillating manifestation*. An example of this is found in Danish. Here there are six contrastive stops initially in words—aspirated [p t k] and unaspirated [b d g]—but only three contrastive word-final stops, which are kept apart by differences in place of articulation only. These final stops oscillate between aspirated and unaspirated pronunciation, words like *lap*, *ladt*, *lak* being sometimes pronounced with aspiration, sometimes without. Having only phonetic similarity at one's disposal is of no help in such cases.

Thirdly, the phonetically based method leads to *phonological overspecification*. If the post-initial stop of an English word like *spill* is analysed as the phoneme /p/, for example, a distinctive value is attributed to a feature which is in fact redundant, namely attribution of 'minus' to 'voiced'.

The greatest difficulty with the *morphologically based method* is that it leaves a residue of indeterminate cases, namely those where a morpheme is realized in one way only. Consider the German word *und*. Here it is impossible to prefer the analysis of the final stop as /t/ over /d/, or vice versa, on morphological grounds. The same applies to the stops after initial /s/ in English.

Secondly, it leads to *phonological overspecification*. If a German word like *Bund* is analysed as containing /d/ before a word-boundary, then this is an overspecification in the sense that in word-final position the feature specification [+ voiced] never serves a distinctive function in German.

Let us proceed to the *both-and method*. We have seen that in the German example, we in this way avoid phonological overspecification. If the final stop of German *und* is identified with both initial-medial /t/ and initial-medial /d/, this means that it is unspecified for the feature 'voiced'. But let us investigate how far the *both-and method* is pushed. Initially in English words a triple consonant cluster can only begin with [s], as in *split*, *string*, *scream*. Consequently, only the feature [+ cons] serves to keep it apart from other segments in this position. If we wish to avoid overspecification altogether, we must identify this [s] with all the contrastive consonants which occur before vowels. That is, we must interpret it as an *archiconsonant*. This, however, is not what the adherents of the *both-and method* have preferred. In a case like the one just mentioned, they identified the [s] of *split* with that of *sit* in spite of the fact that this is an overspecification. The adherents of the *both-and method*,

then, operated not only with neutralization but also with *defective distribution*.

The reason why the both-and method is not carried to its logical conclusion is probably the *psychological orientation* of early Prague phonology. The Praguians attached great importance to psychology and argued that in German, for example, the final stops were represented in the speaker-consciousness as *neutral sound images*—neither strong nor weak. Now in an example like English *split* it is unlikely that the [s] is stored in the speaker-consciousness as an archiconsonant. Substantive evidence has never suggested that this sound represents anything but the phoneme /s/. So in some cases a both-and analysis seems psychologically unmotivated.

Let us now examine whether the original Prague method, which leads to neutralization in some cases and to defective distribution in others, is operationally feasible. First, it may be proposed that neutralization is recognized only in those cases where *one feature* is involved and where it is possible to set up a rule of the type $[\pm f] \rightarrow \emptyset / x_y$. The second requirement is that the context specification of the rule must be a special property of this feature, that is, it may not simultaneously apply to other features. This approach can be illustrated with a Russian example:

$$[\pm \text{voiced}] \rightarrow \emptyset / \left\{ \begin{array}{l} \text{---} \# \\ \text{---} [-\text{sonorant}] \end{array} \right\}$$

Since in Russian there are no other features than 'voiced' which are eliminated word-finally and before obstruents, neutralization may be recognized, and the [p] of words like [xlep] 'bread' and [rɪpka] 'little fish' may be identified with both /p/ and /b/.

If neutralization is defined in this way—as *sequentially determined loss of one distinctive feature*—it is kept apart not only from defective distribution but also from segment redundancy. Defective distribution involves more features than one and in the case of segment redundancy the loss of feature relevance is not sequentially determined.

Let me now give an example where the both-and method may *not* be used. As already mentioned only [s] can begin a triple cluster in English. In this case we cannot set up a neutralization rule which eliminates, say, the feature 'voiced', for the loss of feature relevance in this particular context is not a property of 'voiced' alone, but also of all other features except 'consonantal'.

Although a distinction between neutralization and defective distribution along the lines just mentioned seems operationally feasible, it is not always easy to draw the line. Here is a difficult case: in American English there are according to Moulton twelve contrasting monophthongs. Before intervocalic *r*, however, the number of vocalic contrasts is in most parts of the US strongly reduced:

i {nearer}	{poorer}	u
i {mirror}	{fury}	u

e	{Mary}	ɜ	{furry}	{boring}	o
ɛ	{merry}	ʌ	{hurry}	{warring}	ɔ
æ	{marry}			{starry}	ɑ

As regards the non-high front vowels, only the contrast between /e/ and /ɛ/ is eliminated in some parts of the East, but usually all three vowels coalesce before *r*. Now it is possible to interpret these reductions as neutralizations for those Eastern dialects which preserve a contrast between *Mary*, *merry* on one hand and *marry* on the other on one condition: that the members of all the vowel pairs involved are kept apart by one and the same feature, say 'tense':

$$[\pm \text{tense}] \rightarrow \emptyset / \left[\begin{array}{c} + \text{syllabic} \end{array} \right] r \quad V$$

In those dialects where *Mary*, *merry* and *marry* are all pronounced alike such a rule is insufficient, for the /æ/ of *marry* would still be separated by [+low]. This means that we need an extra rule:

$$[\pm \text{low}] \rightarrow \emptyset / \left[\begin{array}{c} + \text{syllabic} \\ - \text{high} \\ - \text{back} \end{array} \right] r \quad V$$

The problem is that the context specifications of these two rules are sequentially—though not segmentally—identical. In such a case it is difficult to decide whether both rules are legitimate neutralization rules. I am inclined to say yes since an inclusive relationship obtains between the two rules: tenseness suspension can be observed independently of lowness suspension, but no vice versa.

Finally a few words about *substantive evidence* which supports the both-and method. In *Irish*, the voicing contrast is suspended after /s ʃ x/. This can be expressed as a neutralization rule which eliminates the feature 'voiced' after these fricatives. Now precisely in this context there is *orthographic vacillation* between *p*, *t*, *c* and *b*, *d*, *g*, which are consistently kept apart elsewhere. This seems to indicate that the stops are in this context *psychologically indeterminate entities*. Basically the same situation exists in *Old High German*, where post-fricative tenseness-irrelevant stops are spelled not only with *p*, *t*, *c*, but also sometimes with *b*, *d*, *g*, cf. *sbrēhhen*, *priesda*, *fiſg*, *durfdige*, *wihd*.

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Vowel Duration and Vowel Quality

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INTRODUCTION

In many languages of the world vowel systems occur with contrasting pairs of long and short vowels. According to RUHLEN (1975) who made a survey of 700 languages, this situation holds for 48% of the sample languages. In most cases (70%) the vowels of the two systems are equal in number and arrangement, in another 20% the long vowel system is larger than the short vowel system, while 10% of the languages have more short than long vowels. By far the most common difference of quality between long and short vowels of corresponding positions is centralisation of the short high vowels. This tendency can be considered a universal of vowel systems.

It is further known that vowel quality and vowel duration undergo changes in various speech conditions: for vowels in spoken context and without stress a centralisation takes place compared to isolated vowels and the vowel contrast is reduced. This phenomenon of vowel contrast reduction has been investigated for Dutch by KOOPMANS-VAN BEINUM (1980). She studied a whole range of eight speech conditions from isolated vowels to vowels in unstressed position in normal conversation and measured the decrease in duration and the change in quality as related to the formant frequencies F1 and F2. For shorter duration due to the speech condition the vowels become more centralized and therefore the contrast between all vowels in the system is reduced. The acoustic contrast of whole vowel systems in various speech conditions can be denoted by a measure (the acoustic system contrast) that can also be applied to other languages like English (KOOPMANS-VAN BEINUM, 1981) and Japanese (DE GRAAF and KOOPMANS-VAN BEINUM, 1982).

In a vowel system with both long and short vowels the long vowels are produced in a less centralized position than the corresponding short vowels in the same speech condition. In order to compensate for the stronger centralisation one can hypothesize (JANSON, 1977) that a kind of perceptual compensation takes place for the short vowels. This compensation effect causes shorter vowels to be perceived as less central than longer ones with the same formant values. With synthetic stimuli this hypothesis can be tested: In a listening test subjects listened to 7 different synthetic vowels along the formant frequency continuum from /i/ to /e/, each one presented with two different durations, 65 ms and 115 ms. It turned out that there was a tendency for Swedish subjects to perceive vowels near the phoneme boundary /i/-/e/ as more like /i/, i.e.

as less central when of shorter duration. The same effect was found among Greek subjects, but to a much smaller extent. Greek has no pairs of long and short vowels, whereas Swedish does have this distinction. The result of the perception experiment can be interpreted as hinting that there is a considerable amount of perceptual compensation in languages with length distinction and this effect is much smaller in other languages.

MEASUREMENTS

We have investigated the phenomena in question in a study of the vowel system of Frisian, a regional language spoken in the Netherlands. This vowel system can be represented in the following scheme:

short vowels					long vowels				
i	y		u		i:	y:		u:	
ɪ	ø	ə	o		e:	ø:		o:	
ɛ		ɔ			ɛ:		ɔ:		
	ɑ					a:			

Compared with the vowel system of the standard Dutch language the main difference consists in the lack of pairs of long and short high vowels in Dutch. The acoustic analysis of the vowels is based on material produced by 10 native speakers of Frisian; 5 persons belong to the older generation (average age 67 years) and 5 are students (average age 22 years). They have pronounced isolated monosyllabic words containing the vowels in a similar consonant context. With the digital analysis system of the Groningen Institute of Phonetic Sciences the formants F1 and F2 are determined, using the autocorrelation method of Linear Predictive Coding. These formant values can be plotted as functions of time and in the stationary part of the vowel we can determine their average values. These values for all vowels can be plotted in the F1-F2 plane and the results for the different speakers can be compared. We find the following tendencies:

1. For all speakers the short vowels are produced in a more centralized position than the long vowels. This holds in particular for the high long and short vowels and confirms the universal property mentioned in the Introduction.
2. For the younger generation the distance between short and long high vowels in the F1-F2 plane is systematically smaller than the corresponding distance for the older generation. This is illustrated in Fig. 1 and Fig. 2 where the plots for 2 representatives of these groups are given: the first one for the older generation, the second one for the younger generation.
3. When measuring the duration of long and short vowels we can determine for each speaker an average ratio of these durations when averaging over the high vowels /i/, /y/ and /u/. It appears that the obtained average ratio becomes smaller for the younger generation.

These results indicate that there is a decrease in contrast between long and

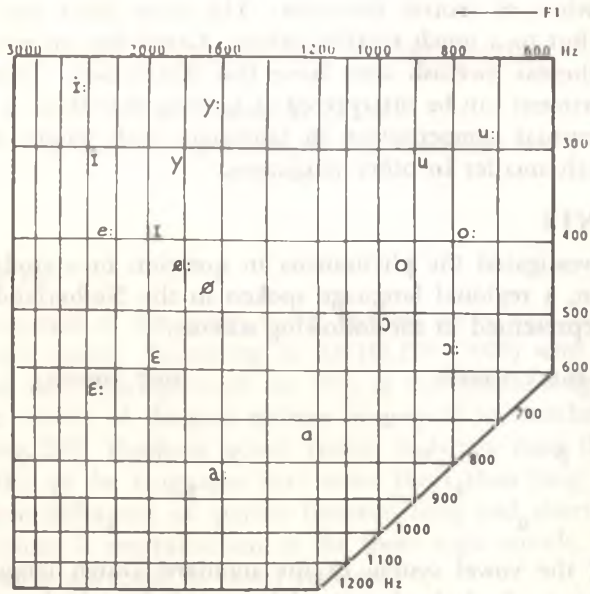


Fig. 1 Formant values for the Frisian vowels spoken in isolated words
Male speaker of 72 years old
Average duration ratio of long and short vowels: 1.9

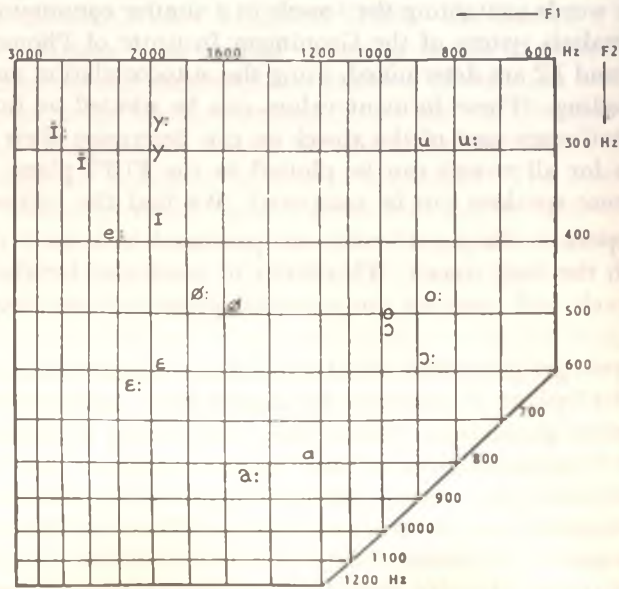


Fig. 2 Formant values for the Frisian vowels spoken in isolated words
Male speaker of 23 years old
Average duration ratio of long and short vowels: 1.4

short vowels taking place in the Frisian language: duration and formant values have come closer to each other. This language change is possibly due to the influence of interference with the official Dutch language where, as we mentioned before, the difference between long and short high vowels does not exist.

We can investigate this phenomenon of language change not only from the acoustic point of view, but also in relation with perception experiments analogous to the ones of JANSON (1977). For this purpose test stimuli were presented to similar groups of native speakers. These stimuli have durations of 60 ms and 200 ms, representing short and long vowels in the F1-F2 plane between the points with F1=300 Hz, F2=2400 Hz and the point with F1=500 Hz, F2=2000. The synthesized vowels in these end points are perceived as /i/ and /ɛ/, respectively.

In our results we find a difference in perception of long and short vowel stimuli: short vowels near the phoneme boundary are perceived as less central than their long counterparts. This effect appears to be smaller for speakers of the younger generation than for speakers of the older generation of Frisians.

CONCLUSION

From the results of acoustic analysis and perception experiments related to the long and short vowels in Frisian it can be concluded that a change in this language is taking place, in that the difference between the components in these pairs is decreasing. This effect may be a result of language interference between Frisian and Dutch with resulting modification of the Frisian phoneme system.

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On Defining Aspiration

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Throughout the history of phonetics, the phenomenon of aspiration has been examined primarily in relation to the description and classification of plosive stops. The earliest extant description of aspiration comes from 'Śikṣā' and 'Prātiśākhya' literature of Sanskrit, where aspirated stops, both voiced and voiceless, are described and classified as 'mahā-prāṇa' (literally, 'big-breath') and unaspirated stops, both voiced and voiceless, as 'alpa-prāṇa' (literally, 'little-breath'). To Sanskrit phoneticians 'big-breath', however, does not simply mean a large volume of breath but also a high rate of air flow. Thus aspiration ('mahā-prāṇatā') is defined in terms of a large volume of breath flowing through the open glottis at a higher rate than normal. Because of the higher than normal rate of flow, the breath becomes turbulent at the glottis. Hence, in their view, aspiration is glottal friction. In fact, they have likened aspiration with both voiced and voiceless fricatives which are also produced with higher than normal volume velocities of the breath stream. According to Sanskrit phoneticians aspiration basically is voiceless noisy breath but it can occur together with voice. They have observed (Allen, 1953: *Phonetics in Ancient India*) that when the glottis is open, breath is produced; when it is closed, voice is produced; and when it is in an intermediate position, that is, when it is half-open, both breath and voice are produced. Further, breath is discharged for the voiceless consonants; voice is discharged for the voiced consonants and vowels; and both breath and voice are discharged for the voiced glottal fricative and the voiced aspirates. Also, more breath is discharged for the aspirated consonants than for the unaspirated consonants. Obviously, the term 'voice' in the above observations has been used as a cover term for two distinctly different modes of glottal vibration. These observations provided the phonetic basis for the separation of the voiceless and voiced aspirates from the voiceless and voiced inaspirates, respectively.

Until recently, the above description of aspiration and its application in the phonetic classification of stop consonants enjoyed general acceptance. However, after the publication of Lisker and Abramson's classic paper: A cross language study of voicing... (1964), the situation has changed. Lisker and Abramson spectrographically investigated the phenomenon of aspiration, along with voicing, across various languages possessing two, three, or four manner categories of homorganic stops. Looking for a single phonetic dimension for such a classification of stops, they directed their attention to "the timing relation between voice onset and the release of occlusion." The differences in voice

onset time (VOT) separated the voiced from the unvoiced stops as well as the aspirated from the unaspirated stops, but the latter two only in the voiceless category. The voiced aspirated stops could not be separated from the voiced unaspirated stops on the basis of VOT. Further, "the noise feature of aspiration" came to be regarded "simply as the automatic concomitant of a large delay in voice onset." Consequently, the "large delay in voice onset" or "voicing lag" became equivalent of aspiration and were thus used regularly by most phoneticians and linguists.

Apparently, influenced by Lisker and Abramson's work, Abercrombie (1967: Elements of General Phonetics) defined aspiration as "a period of voicelessness that follows the closure of a stop." Ladefoged (1971: Preliminaries to Linguistic Phonetics) proposed a similar definition of aspiration as "a period of voicelessness during and immediately after the release of an articulatory stricture." The definition of aspiration given in Ladefoged's recent book (1975: A course in Phonetics) is no different than the one given above. Later, Catford (1977: Fundamental Problems in Phonetics) suggested that the "retardation in the onset of voicing, or 'voicing lag'... is characteristic of aspirated consonants." Kim (1970: A theory of aspiration), on the other hand, defined aspiration in terms of the size of the glottal opening. He asserted that "aspiration is nothing but a function of the glottal opening at the time of release" and observed that "there seem to be a direct correlation between the degree of the glottal opening at the time of release and the degree of aspiration" or voicing lag. He also argued that aspiration is not glottal friction but rather cavity friction, since, according to him, it is produced "at the point of constriction for the following vowel... through coarticulation..."

The purpose of this paper is to examine various views on aspiration in the light of some spectrographic, glottographic and aerodynamic data from Hindi, presented in Figures 1, 2, and 3, respectively, and to suggest a phonetically viable and comprehensive definition of aspiration.

The dictionary meaning of aspiration, relevant to the present topic, is 'breathing', or 'breath'. Sanskrit equivalents of aspiration are 'śvāsa' and 'prāṇa' which also mean breathing, or breath, or air. In the phonetic sense, however, silent breath is not aspiration. Only audible breath, that is, breath made audible, or noisy at the glottis, is called aspiration. Further, the breath cannot become audible, or noisy, in other words, aspiratory, unless the rate of air flow through the glottis is adequate, that is, greater than it is during normal breathing.

The aerodynamic data presented in Figure 3 show, as observed by Sanskrit phoneticians, that aspiration in the aspirated stops, such as [ph] and [bh], cannot be produced without higher than normal rate of breath stream, which in turn cannot be generated in the presence of the oral closure, or the glottal closure, or both as shown in the glottograms in Figure 2. Their observation that aspiration is glottal friction is also well supported by the acoustic pattern of the aspiratory noise during [ph] and [bh] in Figure 1. If the aspiratory noise were not glottally generated the acoustic pattern then would have been dif-

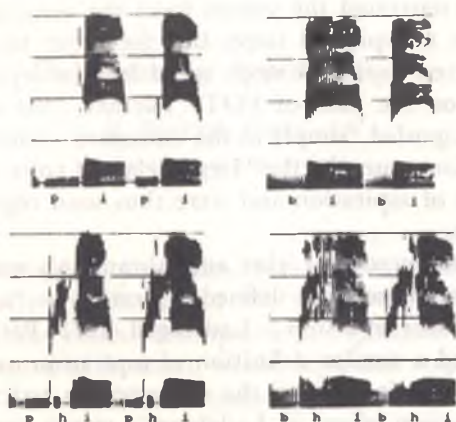


Figure 1. Spectrograms

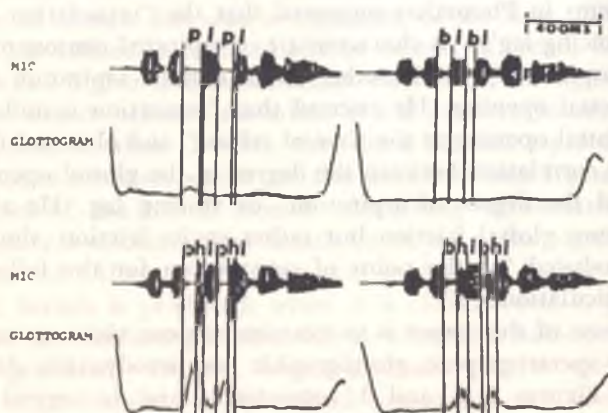


Figure 2. Glottograms

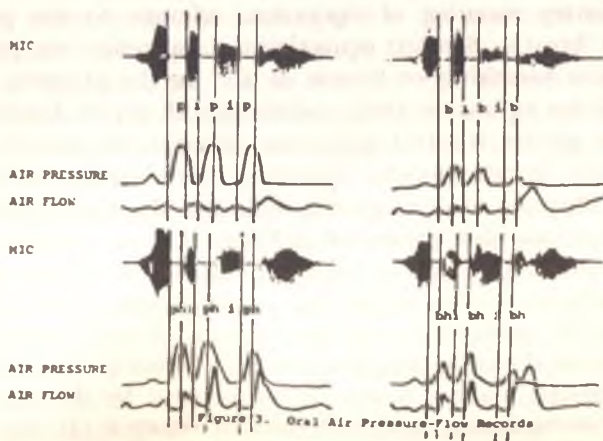


Figure 3. Oral Air Pressure-Flow Records

ferent.

On the other hand, the recent definitions of aspiration, which are essentially the same, are not supported by the spectrograms in Figure 1. Since, acoustic energy is present during the period between the stop release and the onset of the following vowel, this period is inadequately described as "a period of voicelessness" or "voicing lag". Fant (1969: Stops in CV-syllables) has shown that this period in fully developed voiceless stops, that is voiceless aspirated stops, is composed of a transient, a fricative, and an aspirative segment. Hence, aspiration is not simply a period of voicelessness or voicing lag.

Ejective stops, which are produced by an entirely different breath stream mechanism than the plosive stops, regularly show a period of voicelessness or voicing lag following the release of articulatory closure. However, this period of voicelessness or voicing lag is never described as aspiration, since no aspiratory noise is observed during this period. This further damages the definition of aspiration as a period of voicelessness or voicing lag.

Furthermore, the so-called voiced aspirated stops of Indo-Aryan and certain other languages cannot be called aspirated according to the recent definitions of aspiration, since, in such stops the release of the articulatory occlusion is not followed by a period of voicelessness or voicing lag. However, their description as aspirated stops cannot be reasonably rejected. They indeed are phonetically aspirated, since they are produced with glottal friction or aspiration which regularly occurs between the release of the articulatory closure and the onset of the following vowel. But this period in the voiced aspirated stops is not voiceless. Hence, aspiration cannot be justifiably defined as a period of voicelessness or voicing lag.

As regards Kim's claim that there is a direct correlation between the degree of glottal opening at the time of release and the degree of aspiration, it appears to be at variance with the glottographic and spectrographic data presented in Figure 2 and 1, respectively. Clearly, the degree of glottal opening at the time of release is considerably less during [bh] than [ph], yet the degree (or the period) of aspiration following the release is approximately the same. This falsifies Kim's major claim. Further, Kim's contention that aspiration is cavity friction rather than glottal friction is also not supported by these data. The spectrograms in Figure 1 show that the aspiratory noise during [ph] and [bh] is found in about the same frequency regions as the second, third and fourth resonances of the following vowel, which indicates that the noise source is located at the glottis and the whole vocal tract participates in its resonance. If the source were located in the supraglottal vocal tract a different resonance pattern would have resulted.

Since the recent definitions of aspiration discussed above, did not allow for the voiced aspirated stops, Ladefoged et al. (1976: The stops of Owerri Igbo) have proposed a new definition: "Aspiration is a period after the release of a stricture and before the start of regular voicing in which the vocal cords are further apart than they are in regularly voiced sounds." What is meant by

"aspiration is a period" is unclear. What is clear is that this period occurs between the stricture release and the start of regular voicing and that during this period the vocal folds are separated, a condition that must be met for the production of aspiration. But what is aspiration? It can't simply be a period.

The experimental data presented in this paper clearly show that aspiration, whether produced with vocal fold vibration or without it, is primarily and predominantly glottal friction rather than cavity friction. Further, aspiration cannot be produced unless the following conditions are met: (1) the glottis be open, (2) the supraglottal vocal tract be unobstructed, and (3) the rate of air flow through the glottis relative to the size of the glottal aperture be adequate for the generation of turbulence at the glottis. In the production of the aspirated stops, if the first two conditions are met the third condition is automatically met, assuming that the function of the respiratory system during speech is to generate undifferentiated uniform air stream. The fluctuations that occur in air flow rate seem to result from the variations in the glottal and supraglottal resistances. It is important to note that the size of the glottal opening and the timing of the glottal and the supraglottal events in relation to one another play a crucial role in the production of aspiration. Thus, aspiration can be defined as glottal friction produced with higher than normal breath flow through the glottis, with or without glottal vibration, while the glottis is moderately or widely open and the supraglottal vocal tract is unobstructed.

Patterning of Distinctive Features in Relation to Variability

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1: Variable rules and systematic constraints

Spoken language of any speech community has variable and invariable characteristics. Their mutual relations require further investigation (cf. a.o. Labov 1973), in the course of which functional conditions on variable rules must be formulated in a testable way (cf. Kiparsky 1972), and an evaluation measure pertaining to the invariable part of the system must be formulated as a set of constraints on variability.

This paper concentrates on invariable constraints operative in two distinct areas of variability: sound change and speech errors.

2: Constraints on general principles of vowel shifting

Labov, Yaeger and Steiner (1972: 106) formulated the following general principles of vowel shifting:

- I: in chain shifts, tense or peripheral vowels rise (mid tense vowels may develop either ingliding or upgliding diphthongs; op. cit.: 228);
- II: in chain shifts, lax or nonperipheral vowels usually fall, particularly the lax nuclei of upgliding diphthongs;
- III: in chain shifts, back vowels move to the front.

These general principles are constrained, as can be seen from Kajkavian Serbo-Croatian concerning the first two principles, and Hungarian concerning the third one. Are these constraints of a general nature?

The vowel system underlying Kajkavian dialectal differentiation can be reconstructed as indicated in (1), following Ivić (1968: 58f.).

- (1) i u (These vowels could be distinctively either long or short. The
o long ones can be considered 'peripheral', and the short ones
e o 'nonperipheral'. Whenever length is irrelevant to a develop-
e ä ment, I make no reference to it.)

Developments of /o/: (a) /o/ → /o/ in the northern and northeastern areas;
(b) /o/ → /u/ in the western and eastern areas;
(c) /o/ remains preserved in most parts of the central area;
(d) /o/ → /ou/ if long, and → /u/ if short (in Bednja either short or rising) in the northern and western parts of the central area;

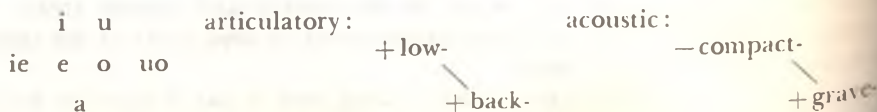
- (e) /o/ → /ou/ if long, and → /u/ if short; original long /o/ → /uo/ and long /e/ → /ie/ in Ključ and Koškovec.

The Kajkavian developments show that both peripheral and nonperipheral vowels may either rise or fall in chain shifts. They may either merge with other vowels or remain distinct. There is a tendency for peripheral mid vowels to rise in diphthongization, but in different ways (further left unexplained by Labov, Yaeger and Steiner), as can be seen from a comparison of the systems of the type (d) with those of (e): /o/ is diphthongized as /ou/, and /o/ as /uo/. The same regularity holds for Slovenian (cf. Rigler 1967: 133f.). Following Andersen's (1972) definition of diphthongization as due to a distribution of feature specifications in the order 'unmarked, marked' within a segment, we can see that in the /o/ → [ou] diphthongization, the values of [\pm diffuse] (or [\pm high] in the articulatory terminology) are distributed in that order as directly reflecting the distinctive phonetic specifications (irrespective of [\pm compact], or [\pm low], which is redundant in such segments—at variance with Andersen's analysis). In the /o/ → [uo] diphthongization, on the other hand, it is due to the distinctive [−compact] (or [−low]) specification of the given segment that [+diffuse] (or [+high]) is treated as 'unmarked', and [−diffuse] (or [−high]) as 'marked'. This explains the [+diffuse, −diffuse] (or [+high, −high]) diphthongization, in accordance with Andersen's analysis.

The Kajkavian types of diphthongization show that specification with respect to [\pm diffuse] (or [\pm high]) is subject to variation, whereas in such cases specification with respect to [\pm compact] (or [\pm low]) is not. Is there anything that makes [\pm compact] (or [\pm low]) 'special'?

In the systems of the type (e), where the latter feature opposition was relevant to diphthongization of the segments distinctively specified for it, an asymmetry connected with this feature opposition can also be observed. In those systems, the opposition between [−grave] and [+grave] (or [−back] and [+back]) vowels is operative only in the distinctively and redundantly [−compact] (or [−low]) ones, but not in the [+compact] (or [+low]) ones. This asymmetry can be analysed as a hierarchical relation, in which [\pm compact] (or [\pm low]) is the dominating feature opposition, and [+grave] (or [+back]) the subordinate one. This then makes [\pm compact] (or [\pm low]) 'special' in comparison with [\pm diffuse] (or [\pm high]): the former is a dominating feature opposition, whereas the latter is not.

- (2) Kajkavian systems of the type (e):—distinctive feature hierarchy



This definition of hierarchy is in accordance with Brøndal's (1943) 'principle

of compensation', on which e.g. Andersen's (1975) definition of hierarchy was also based. There is a difference between the two approaches, however. According to Andersen's definition, the terms of the subordinate feature opposition are not combined with the marked term of the dominating feature opposition unless they are also combined with its unmarked term. But how do we know which feature opposition is 'dominating' if a 'subordinate' one is combined with its both terms? In other words, the problem with Andersen's definition of hierarchy as an implicational universal is that it is not fully decisive for a given language system. In order to make it testable, I propose to restrict the definition of hierarchy so as to equal only asymmetrical ordering.

- (3a) **ASYMMETRICAL ORDERING**, equalling **HIERARCHY**, holds for a pair of distinctive feature oppositions if and only if an opposition is distinctive in combination with one of the terms of another opposition, but not with its both terms.

In an asymmetrically ordered pair of distinctive feature oppositions, dominance is determined by markedness. In accordance with Kiparsky (1982), I propose to view as 'marked' an unpredictable feature specification, and as 'unmarked' the corresponding predictable one. Predictability is either determined by the paradigmatic or syntagmatic context, or directly related to the phonetic specifications as elaborated by Jakobson, Fant and Halle (1952). The paradigmatic context should be restricted to cases of asymmetrical and symmetrical ordering of distinctive feature oppositions. The latter occurs if two oppositions are patterned within the same phonetic dimension, as are e.g. [\pm compact] and [\pm diffuse] (or [\pm low] and [\pm high]). Note that this amounts to a revision of the markedness theory as an evaluation measure as proposed by Chomsky and Halle (1968), Kean (1980) and others, in the sense of its further analysis in terms of two constitutive parts, a formal and a substantive one. The former is found in the principle of asymmetrical ordering, and the latter in 'markedness' as derivable from the phonetic features and their predictability.

- (3b) In an asymmetrically ordered pair of distinctive feature oppositions, dominance is determined by the principle of attaching the subordinate opposition to the unmarked term of the dominating one.

Coming back to the Kajkavian data discussed above, we can see that if a sound change involves a segment which is distinctively specified for a dominating feature opposition, this feature specification remains preserved, whereas other feature specifications change. Consequently, we can hypothesize that a specification of a dominating feature opposition changes only if the corresponding subordinate ones change too. In order to test this hypothesis based on developments related to the first two principles stated by Labov, Yaeger and Steiner (*op. cit.*), let us examine developments related to their third principle, and those observed in other areas of variability.

The third principle of vowel shifting formulated by Labov, Yaeger and

Steiner stated that [u] would generally change into [ü] in the course of vowel shifting. But why then did [i] change into [ü] in Hungarian from the 10th to 15th centuries (cf. Kálmán 1972), whereas in the same period or somewhat earlier Dutch had the predicted change of [u] into [ü]? The markedness theory of Chomsky and Halle, and Kean does not help us in this respect, as [ü] is in both approaches more 'marked' than either [u] or [i], and the latter two segments differ in features, not complexity. In search of an answer, let us compare the Dutch and Hungarian vowel systems preceding the change (i) and following it (ii).

(4a) Dutch: -stage (i)

i u (long vowels and diphthongs were phonologically sequences)

vowel shift: /u/ → /ü/, except for some eastern dialects; monophthongization of the diphthongs, etc.

(4b) Hungarian: -stage (i)

i ü u (long vowels and diphthongs were phonologically sequences)

vowel shifts: /ü/ → /ö/, /i/ → /ü/

-stage (ii)

i ü u (vowel sequences, including e o diphthongs, became tense a vowels, as opposed to originally single, now lax, ones)

vowel shifts in the central dialects: tense /ü/ → /Δü/, tense /i/ → /ei/; in some marginal, especially southern dialects: /ü/ → /i/

-stage (ii)

i ü u (vowel sequences, including e ö o diphthongs, became long e á vowels, as opposed to originally single, now short, ones)

Differences between Dutch and Hungarian in stage (i):

- Dutch had no asymmetrical ordering between [\pm grave] and [\pm flat] (or [\pm back] and [\pm round]), as those features cooccurred; there was (presumably) asymmetrical ordering between [\pm compact] (or [\pm low]) and [\pm grave] (or [\pm back]), redundantly accompanied by [\pm flat] (or [\pm round])
- Hungarian had an asymmetrical ordering between [\pm grave] and [\pm flat] (or [\pm back] and [\pm round]) such that [\pm grave] (or [\pm back]) was the dominating feature opposition.

In stage (ii), on the other hand, both systems had an asymmetrical ordering between [\pm grave] and [\pm flat] (or [\pm back] and [\pm round]).

We can conclude that sound change in Dutch and Hungarian was constrained by the same general principle as in Kajkavian Serbo-Croatian and Slovenian: in the presence of asymmetrical ordering, specification of the dominating feature opposition was preserved, whereas specification of the subordinate one changed. Thus [−grave] (or [−back]) remained preserved in the Hungarian /i/ → /ü/ change in stage (i), and in the Dutch changes in stage (ii). In the absence of asymmetrical ordering, general phonetic principles were

operative.

Sound change thus shows that asymmetrical ordering and markedness constitute an evaluation measure constraining general phonetic tendencies.

3: Constraints on speech errors

Another area of variability which proved to be indicative of language structure comprises speech errors. Spontaneous occurrence and correction of speech errors give evidence of the existence of distinctive segments, morphemes, words, and phrases as processing units in speech production and perception. Segmental errors involve distinctive features, as the interchanged segments differ in one distinctive feature or a limited number of them. Various studies of segmental errors based on spontaneous speech, short-term recall, shadowing, or spoonerisms (as a controlled subset of 'errors'), yielded comparable results by pointing to differences in the treatment of the various distinctive feature oppositions, so far not being able to give a full account of them. The different treatment seems to be due to relations among the features, not to their formulation in articulatory, acoustic, or perceptual terms (cf. also Van den Broecke and Goldstein 1980). There is apparently an evaluation measure involved.

Shattuck-Hufnagel and Klatt (1979) investigated the matter on the basis of spontaneous errors in the nonsyllabic segments of English and concluded that there was no evidence of markedness as formulated by Jakobson, Fant and Halle (op. cit.), or Chomsky and Halle (op. cit.). Distinctive feature hierarchy in the sense of relevance of certain distinctive features to markedness conventions of other distinctive features, as formulated by Kean (1980), was not supported by the data either. For example, the unmarked specification for $[\pm\text{back}]$ is according to Kean (op. cit.: 23) $[+\text{back}]$ in the context of $[-\text{anterior}]$, but a change of $[+\text{anterior}]$ into $[-\text{anterior}]$ did not entail a change of $[-\text{back}]$ into $[+\text{back}]$ in the errors, which would be the unmarked case.

In search of an evaluation measure, let us have a detailed look at the distinctive feature exchanges involved in the data presented by Shattuck-Hufnagel and Klatt, and compare them with ordering in the articulatory and acoustic feature oppositions of English. My analysis of the articulatory features is based on Chomsky and Halle (op. cit.), and of the acoustic ones on Jakobson, Fant and Halle (op. cit.). Following Jakobson, Fant and Halle, I propose to replace $[\pm\text{voice}]$ in the consonants of English by $[\pm\text{tense}]$. Following Chomsky and Halle, I propose to introduce $[\pm\text{tense}]$ in the acoustic analysis of the English vowels as well, and to separate $[\pm\text{diffuse}]$ from $[\pm\text{compact}]$. Other details of the distinctive segments of English are not at issue in this study.

Does it hold for speech errors as well that a change of a dominating feature opposition implies a corresponding change of its subordinate one(s), whereas no constraints hold for nondominating feature oppositions?

(5a) The most common nonsyllabic segmental errors as established by Shattuck-Hufnagel and Klatt (1979: 49): $/r/ \rightarrow /l/$, $/s/ \rightarrow /z/$, $/p/ \rightarrow /f/$, $/m/ \rightarrow$

/n/, /w/—/r/, /k/—/t/, /p/—/k/, /s/—/θ/, /s/—/t/, /f/—/s/, /n/—/l/, /t/—/p/, /w/—/m/, /l/—/ɹ/, /d/—/b/, /d/—/j/, /w/—/l/, /m/—/r/, and /b/—/g/.

- (5b) Articulatory distinctive features of English involved in the errors:
 —single exchanges: [\pm anterior], [\pm coronal], and [\pm strident];
 —independent exchanges: [\pm continuant] and [\pm nasal];
 —implicational exchanges: [\pm high] implies [\pm anterior], [\pm vocalic] implies [\pm continuant] and [\pm nasal] (if distinctive in either of the segments), and [\pm consonantal] implies either [\pm high] or [\pm low]; [\pm back] implies [\pm high], unless [\pm consonantal] is exchanged too.
- (5c) Acoustic distinctive features of English involved in the errors:
 —single exchanges: [\pm grave], [\pm compact], and [\pm strident];
 —independent exchanges: [\pm continuant], [\pm nasal];
 —implicational exchanges: [\pm vocalic] implies [\pm continuant] and [\pm nasal] (if distinctive in either of the segments), and [\pm consonantal] implies either [\pm diffuse] or [\pm compact].
- (5d) Asymmetrical ordering in pairs of articulatory distinctive feature oppositions:
 [\pm vocalic] dominates [\pm continuant], [\pm nasal], and [\pm strident] (the latter three feature oppositions are mutually unrelated); [\pm consonantal] dominates [\pm low], [\pm high] dominates [\pm anterior], and [\pm back] dominates [\pm anterior] and [\pm coronal].
- (5e) Asymmetrical ordering in pairs of acoustic distinctive feature oppositions:
 [\pm vocalic] dominates [\pm continuant], [\pm nasal], [\pm grave], and [\pm strident] (the latter four feature oppositions are mutually unrelated); [\pm consonantal] dominates [\pm diffuse].

We find a striking isomorphism between articulatory and acoustic features as far as asymmetrical ordering goes. And we find a striking regularity in its imposing constraints on speech errors. These constraints are of the same nature as those established for sound change (cf. also Gvozdanović 1982). They form a workable hypothesis for an investigation of other areas of variability, such as language acquisition, where we only expect dominating distinctive feature oppositions to be acquired earlier than their subordinate ones.

The general conclusion amounts to viewing asymmetrical ordering of distinctive feature oppositions as an evaluation measure constraining variability. It should be put to a test as an universal principle with language-specific applications.

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The Rhythm of *Tanka*, Short Japanese Poems

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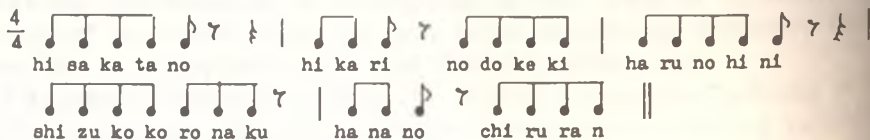
1. Introduction

This paper attempts to observe the structure of *Tanka*, short Japanese poems, and to find out why this traditional type of verse sounds rhythmic, measuring the duration of each segment, mora, pause, line, and whole poem.

In my papers of 1981 and 1982, I investigated durational relationships between Japanese consonants and vowels using test words in a sentence frame, and found that such variables as closure or frication duration, voice onset time, and vowel duration work together to fix the word duration, although the duration of syllables is phonetically different. The present paper deals with longer utterances of poems with five lines, so that we can see the temporal structure of Japanese beyond the word level, and as the poems contain all the consonant and vowel phonemes except /p/, we can examine various combinations of them in natural speech.

Tanka is basically composed of thirty-one moras, in 5-7-5-7-7 lines, as seen in the first poem on the handout. This verse form is said to have the most traditional rhythm in Japanese. Actually, *Tanka* form has been handed down since the seventh century and today too many Japanese people enjoy writing and reciting *Tanka* and *Haiku*, shorter poems with 5-7-5 lines.

In the book titled *Rhythm in Japanese* (1977), Bekku claimed, "The rhythm of *Tanka* is made of quadruple time, and this quadruple time is the most basic rhythm in Japanese." He showed the first poem on the handout in the following way:



According to him, the key to the equidistant lines of *Tanka* is the boundary and pause pattern. His claim caused a kind of sensation because most native speakers of Japanese thought the numbers of moras, that is 5 and 7, determined the rhythm of *Tanka*. In other words, the combination of shorter lines with five moras and longer lines with seven moras were thought to create the rhythm of *Tanka*.

The purpose of this paper is to confirm acoustically the structure of the underlying rhythm of *Tanka*.

2. Experiment

2.1 Methods

Six *Tanka* were chosen from the *One Hundred Poems from One Hundred Poets* (thirteenth century). This anthology contains *Tanka* ranging from the seventh century to the early thirteenth century, namely the ages of Old and early Middle Japanese. We do not know how people recited the poems in those days. However, this anthology is still loved by people, and we play cards on New Year's Days, using and reciting these hundred poems.

Among the six poems chosen, one is the basic thirty-one mora type, and the others have irregular patterns in different lines. The irregularly longer lines are called *ji amari*, hypermeter, or excess of letters in literal translation; and irregularly shorter lines, *ji tarazu*, deficiency of letters. Note that the Japanese *Kana* letters represent moras. Four speakers read these poems twice in natural tempo. The total forty-eight utterances were recorded on tape, and wide-band spectrograms were made with a Kay-Sonagraph (6061B).

2.2 Measurements

Duration of all the segments was measured from the onset to the end of acoustic energy of each segment. There were no voiceless stops after pause, but some /t/ allophones occurred after the word boundary and it made the measurements a little difficult, because the word boundary caused a very short delay of the onset of the following phone, as reported by Han (1962). It was not a pause, but it made the closure duration of the following /t/ a little longer, probably because of the post-boundary lengthening. Devoiced high vowels were excluded from vowel measurements. Long vowels like [ii] or [juu] were impossible to divide, so they were also excluded from measurements of segment and mora duration. Voice onset time after the burst of voiceless stops was included in the following vowel duration. All the values were rounded to the nearest 5 milliseconds.

2.3 Results and discussions

2.3.1 Segment duration

2.3.1.1 Vowels

Table I shows the results for vowel duration in milliseconds. Average vowel duration became shorter in this order:

(longest) /o, e, a, i, u/ (shortest)

Table I Average vowel duration in ms, all environments pooled

	number of occurrences	ms
/i/	42	69
/e/	16	86
/a/	62	84
/o/	33	90
/u/	24	64
Mean		79

This order was the same as in Homma (1973), and [+high] vowels had much shorter duration than [-high] vowels, which seems to help devoice high vowels.

2.3.1.2 Consonants

Table II shows the results for consonant duration in milliseconds. The dental phoneme /t/ has three positional allophones: [tʃ] before /i/, [ts] before /u/, and [t] elsewhere; [tʃ] and [ts] are affricates, and naturally they were longer than [t]: 119 ms, 84 ms, and 71 ms, respectively. The alveolar /s/ has two allophones: [ʃ] before /i/, and [s] elsewhere; [ʃ] was 103 ms, and [s], 75 ms. The /d/ and /z/ did not occur before /i/, so we had no affricate allophone [dʒ]. The /N/ is a mora nasal, the duration of which was the second longest. The /r/ is a flap, and of course it was the shortest. The consonants became shorter in the following order:

(longest) /s, N, t, h, k, z, b, m, n, j, d, g, w, r/ (shortest)

The results agreed, by and large, with previous experiments by Han (1962), and Sato and Hashimoto (1976).

Table II Average consonant duration in ms, all environments pooled

	no. of occ.	ms		no. of occ.	ms
/t/	15	78	/h/	10	61
/k/	22	57	/r/	22	29
/b/	5	55	/m/	12	54
/d/	5	45	/n/	30	52
/g/	5	41	/N/	5	86
/s/	8	93	/j/	5	49
/z/	5	57	/w/	5	36
			Mean		57

2.3.2 Mora duration

2.3.2.1 Mora vowels

Table III shows the average duration of mora vowels. The position of mora vowels had considerable effects on the duration, so we calculated the average in accordance with their positions: line initial, medial, and final. The average duration of mora vowels was a little longer than the average for vowel segments, but the difference was small: 89 ms and 79 ms respectively. On the contrary,

Table III Average mora vowel duration in ms

	no. of occ.	line initial	no. of occ.	line medial	no. of occ.	line final
/i/	3	52	2	95	0	
/e/	0		1	93	0	
/a/	4	72	1	83	0	
/o/	1	62	1	88	3	119
/u/	1	47	0		0	
Mean		58		90		119

the positional difference was great. Line initial mora vowels were half as long as line final vowels, a phenomenon we will discuss later in the section of irregular lines.

2.3.2.2 Mora nasal /N/

As mentioned above, the average value for mora nasals was 86 ms, close to the value for mora vowels. Mora nasals never appear at initial position, and there was no conspicuous difference between medial and final positions.

2.3.2.3 CV moras

The positional difference of CV moras was not as big as in the case of mora vowels: initial, 121 ms; medial, 129 ms; and final 173 ms. Line final CV moras were apparently longer, but vowels were prolonged more than consonants. Table IV shows the durational relationship between consonants and vowels within the CV moras.

Table IV Average consonant and vowel duration within CV moras in ms, all environments pooled

	consonant	vowel	CV moras
/t/	78	75	153
/k/	57	86	143
/b/	55	82	137
/d/	45	114	159
/g/	41	76	117
/s/	93	52	145
/z/	57	68	125
/h/	61	50	111
/r/	29	87	116
/m/	54	76	130
/n/	52	84	136
/j/	49	75	124
/w/	36	81	117
Mean	54	77	131

The consonant and vowel segments taken separately had a greater range of difference than the CV moras as a whole. This implies that a large degree of temporal compensation works at a CV mora level; the consonant and vowel duration is in inverse relation, as reported by Port et al. (1980). In other words, the longer the consonants, the shorter the vowels, and vice versa. The following example illustrates this:

h	a	n	a	n	o	ch	i	r	u	r	a	n
77	54	38	78	49	83	119	44	28	88	33	100	83

The vowel /u/ after a flap was twice as long as /i/ after an affricate [tʃ]. This kind of compensation between a consonant and a vowel clearly helps make

Japanese a syllable-timed language. The above example also indicates that the preceding consonant has much more influence on the vowel duration than the following consonant, which was discussed in Homma (1981).

2.3.3 Line duration

2.3.3.1 Mora and pause

Table V shows the average duration of each line of the six poems, with the duration of mora average and pause. The + signs after the mora average value indicate irregularly longer lines, and the - sign, a shorter line.

Table V Average line and whole poem duration in milliseconds

line	1			2			3			4			5			total
poem	mora ave.	pause	sum	mora ave.	pause	sum	mora ave.	pause	sum	mora ave.	pause	sum	mora ave.	pause	sum	
(A)	144	156	878	123	0	859	128	372	1012	134	47	983	125	—	874	4606
(B)	119+	79	794	120	0	842	132	428	1088	122	35	887	128	—	896	4507
(C)	130	214	863	119+	0	949	134-	239	774	136	81	1035	120	—	839	4460
(D)	126	211	842	122	0	853	136+	374	1190	130	82	992	118	—	829	4706
(E)	142	156	865	112	0	784	134	479	1147	117+	14	952	130	—	908	4656
(F)	128	226	865	120	0	845	138	443	1133	129	75	975	107+	—	854	4672
Mean	132	174	851	119	0	855	134	389	1057	128	56	971	121	—	867	4601

From Table V, the following points were observed.

1. The duration of each line including pause was fairly regular except for the third line, at the end of which the speakers took a breath. Although the number of moras of the lines was different, the average durational difference was small: around 150 milliseconds at most. Compared with the results obtained by Lehiste in her English experiments (1973), this difference in Japanese seems not to be above the just-noticeable differences.

2. The adjustment for equidistant lines was achieved in two ways: first by changing the duration of pause as claimed by Bekku; the speakers put a certain amount of pause after the 5-mora lines but no pause or very short pause after the 7-mora lines. Secondly, the speakers changed the duration of moras by means of the speech rate. They read the longer lines, especially irregularly longer lines, a little faster; thus the average duration of moras became shorter.

2.3.3.2 The rules for hypermeter of *Tanka*

Among the several rules for irregularly longer lines (hypermeter) of *Tanka* (Mōri, 1979), the rule proposed by Motoori Norinaga in the eighteenth century is the most essential. His rule was this: hypermeter occurs when mora vowels /a, i, u, o/, come in the line medial position, not initial nor final. Mōri (1979) investigated hypermeter in the *Manyōshū*, an anthology of Old Japanese poetry in the eighth century, and found Norinaga's rule worked more than 90% in the first, the third, and the fifth lines of *Tanka* (A group), but only about 25% in the second and the fourth lines (B group). Two questions are raised here. The

first question is why hypermeter appeared only in the medial position. The second is why there were two groups of lines, (A) and (B): the (A) group had many hypermeters and the (B) group, few.

The answer to the first question seems to be in the fact that production and perception of duration have positional differences as pointed out by Lehiste (1973, 1979). In her production experiments (1973), Lehiste reported that the speakers produced the first foot shortest, and the last foot longest among the four intervals. In perception, on the contrary, when all intervals had the same duration, the listeners tended to hear the first interval as longer than the others, and the last interval as the shortest, even though all intervals had equal duration (1979).

The present experiment showed that a mora vowel was much shorter than a CV mora, average 89 ms and only 58 ms at initial position, while the CV mora duration was 131 ms. The difference is surely above the threshold (Lehiste, 1970). However, at the line initial position, we do not perceive the actual difference; therefore, we have no hypermeter for an initial mora vowel. But in the medial position, a mora vowel is too short to be compensated, so an extra mora has to be added. At the final position, a mora vowel becomes very long, which corresponds to the fact that there was no hypermeter for a mora vowel at the final position.

The answer to the second question is found in Bekku. After the (A) group lines—the first, the third, and the last—a fair amount of pause was inserted, and the adjustment of line duration might be more easily achieved by reducing the duration of pause. On the other hand, the (B) group lines—the second and the fourth—scarcely had room for pause, so the adjustment caused by hypermeter has become more difficult. In fact, Mōri (1980) reported that the same combination of moras produced hypermeter in the last line, but not in the fourth line of several *Tanka* in the *Manyōshū*.

2.3.4 Whole duration of poems

The last column of Table V indicates the average duration of the whole poems. The difference among the six poems was surprisingly small. The big differences of the third lines were compensated in the whole duration.

3. Conclusion

The present paper concludes that although the duration of each segment and mora had a greater range of difference and the number of moras of the lines was different, the average durational differences of lines and whole poems were small. The adjustment was first achieved by the duration of pause and secondly by the duration of moras. This implies, as Allen suggested in 1975, timing information for *Tanka* might be precomputed in the speakers' brains, so they produced the regular duration. This is the underlying rhythm of *Tanka*. According to Allen, when we hear a sequence of pulses between 0.1 second and 3.0 second, we hear it as rhythmic. The duration of a CV mora and the duration of a line of *Tanka* fall within this range. It seems that these tendencies

of both speakers and hearers help us produce and hear the pleasing rhythm of *Tanka*.

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On the Contrast between [æ] and [a] in Modern Arabic

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Most available accounts of Arabic vowels consider [æ] and [a] as variants ("allophones") of the same vowel. It is usually stated that the back variant [a] occurs in the environment of emphatic and some other consonants including [q] and [r], whereas the front variant [æ] occurs in other contexts (see, e.g., Abdo, 1969, 9ff.; Cowell, 1964, 11ff.; Harrell et al., 1963, 6-8; Inglefield et al., 1970, v). A more extreme position on this matter is taken by Erwin (1969, 5) who believes that "variations in the quality of the vowel from one word to another...are automatically determined by the environment." Mitchell (1962, 22-4), whose account is not based on classical phonemic theory, is the only writer I know of who recognizes the need to maintain a distinction between these two vowels, stating explicitly that "the consonant context...is not an infallible guide to the quality" of these vowels (p. 23).

Although it is true that [æ] and [a] appear to be phonologically determined variants in a large number of words, it is also true that there are many other words in various dialects of spoken Arabic in which the alternation between [æ] and [a] is not automatic or phonologically determined. Here I argue that a phonological ("phonemic") contrast between [æ] and [a] needs to be established, at least in some varieties of spoken Arabic. This conclusion is based on the following evidence (unless otherwise indicated, all of my examples come from Palestinian Arabic):

1. In Palestinian Arabic, there are many minimal and near-minimal pairs in which [æ] and [a] contrast in the absence of emphatic sounds and [q] or in the presence of [r]:

[ʔæxx] brother

[ʔaxx] ouch!

[mrææti] my mirror

[mraati] my wife

[mæraʔu] they diluted

[maraʔu] they passed

[bæirmi] I throw away

[barmi] my chatting

[mæirmi] lying

[marma] goal

[mæyy] Mayy

[mayy] water

Altoma (1969, 16) cites a number of similar pairs from Iraqi Arabic:

[xælli] let, leave

[xalli] my vinegar

[xææli] my maternal uncle

[xaali] empty

[dæxlæ] let him enter

[daxla] wedding

[gællæ] he told him

[galla] he roasted

In addition, there are numerous pairs in which [æ] and [a] contrast in the vicinity of [r]:

[jæri]	running	[jaari]	my neighbor
[θæwri]	revolutionary	[θawri]	my bull
[ðæri]	naked	[ðaari]	my shame
[zæhri]	pink	[zahri]	my flowers
[nææri]	fire-like	[naari]	my fire

Selim (1967, 137) cites similar pairs from Egyptian Arabic. Mitchell (p. 23) also cites from Egyptian Arabic the pairs [ʔæmar] 'he ordered' and [ʔamar] 'moon'; [ʔændæh] 'I call' and [lukanda] 'hotel.' Moreover, many varieties of spoken Arabic have words with [a] or [aa] in contexts where [æ] or [ææ] would be expected according to the "allophonist" view as in the following words from Palestinian, Syrian, Lebanese, and Egyptian dialects:

[maama]	mummy	[baaba]	daddy
[waawi]	fox	[baay]	good-bye
[mayyæ]	water	[habhab]	it barked (Egyptian)

2. There are strong indications that [æ] and [a] develop independently of their phonetic environments in the course of the acquisition of Arabic as a first language. Personal observation of the speech development of one of my children some three years ago revealed that [a] could be found in some words and baby forms long before any emphatic sounds had appeared. Similarly, before the development of emphatics [a] appeared in some words which later would have emphatics. There were also many child words and baby forms in which [a] occurred instead of the expected [æ] as in [baay] 'good-bye,' [ðaww] 'doggie' and [nam] 'food.' Furthermore, the vocabulary of my son Omar at 1;8 included the following two minimal pairs:

[mææmæ]	= [hæmææmæ] 'bird'	[maama]	'mummy'
[mæt]	= [sæmæk] 'fish'	[mat]	= [malʔaT] 'peg'

3. The sociolinguistic weight of the [æ]—[a] contrast seems to be too great to be relegated to allophonic variation. For one thing, [a] is definitely the norm in some phonetic contexts when reciting the Qur'an or reading classical texts. Under less formal conditions, [æ] appears in the same phonetic environments. In other words, the distinction between [a] and [æ] corresponds to the distinction between the 'formal' and 'informal' style of reading:

[xaalid]	[xæælid]	'immortal'
[ʔaalib]	[ʔæælib]	'conqueror'
[ʔaaʔib]	[ʔææʔib]	'absent'
[maryæm]	[mæryæm]	'Mary'

On the other hand, the contrast between [a] and [æ] in a number of Palestinian and other dialects sometimes corresponds to a stigmatized-nonstigmatized

distinction. Thus a form like [jaaja] instead of [jæxjæ] 'hen,' [haaða] instead of [hæxðæ] 'this,' or [maalak] instead of [mæxlæk] 'what's the matter with you?' would most certainly be avoided by educated speakers outside the security of their homes or close friends. Mitchell (p. 24) also reports that the difference between [a] and [æ] "tends to relate to differences between the speech of men and women respectively." Thus, the form [gærræxh] 'surgeon' occurs typically in women's speech whereas [garraah] is the corresponding form in men's speech.

4. Finally, there are two phonological rules which apply to [æ] but not [a], indicating that the two vowels behave independently and should, therefore, be treated as distinctive sounds. The first rule applies to the feminine ending [æ] (but never [a]) in some contexts and changes it to [e], the conditioning factor being the consonant preceding [æ] (cf. Cowell, 1964: 138):

[kbiiræ] → [kbiire] 'big' but [muniira] → * [muniire] 'Muniira'
 [ʔibræ] → [ʔibre] 'needle' but [numra] → * [numre] 'number'
 [ræʔæbæ] → [ræʔæbe] 'neck' but [ʃooraba] → * [ʃoorabe] 'soup'

The second rule also applies to [æx] but never to [aa] and raises the vowel to [ee], a phenomenon known in Arabic as *imaala* and found in most Lebanese and some Syrian dialects (op. cit., 14-5):

[kæxtib] → [keetib] 'writer' but [ʔaaDi] → * [ʔeeDi] 'judge'
 [sæxlim] → [seelim] 'safe' but [Saayim] → * [Seeyim] 'fasting'
 [ktæxb] → [kteeb] 'book' but [ʔaarib] → * [ʔeereb] 'boat'

To conclude, I believe that all of the preceding evidence points unmistakably to a phonological distinction between [æ] and [a]. In addition, considerations for symmetry and adequacy seem to favor strongly the view adopted here. Limitations on space preclude any detailed discussion of the vowel systems which have been proposed for various Arabic dialects (cf. Ibrahim, 1972: 25-6). It is enough, therefore, to present in table form some of these systems and to compare them with the one proposed here to see that the latter, besides accounting more adequately for the phonological data, is more symmetrical.

Blanc (1964: 40)	Cowell (1964: 13)	Erwin (1969: 78)	Proposed here
i	i	i	i
u	u	u	u
e	e		e
a	(ə) u	o	o
	a	a	æ a

References

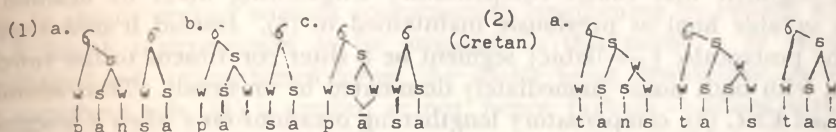
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When a nonsyllabic segment is deleted, sometimes it causes compensatory lengthening of a preceding vowel, as in (1) and sometimes it does not as in (2), depending on its position in syllable structures, as illustrated from Greek.



Therefore, compensatory lengthening is said to result only when the deleted consonant is postvocalic as well as syllable final, as noted in the Empty Node Convention (3).

- (3) Empty Node Convention (ENC) (Ingria pp. 471, 472 and 466)

Empty *w*-nodes which are part of a syllabic coda are to be associated with the terminal element dominated by the immediately preceding syllabic nucleus. [This] empty node is the rightmost node in a rhyme. All other empty nodes are to be pruned.

Thus we have *pāsa* with long *ā* in (1) on the one hand but *tas* with short *a* in (2) on the other. Consequently, the corresponding Attic form *tās* with a lengthened vowel from the same underlying *tans* cannot be derived by ENC (3)—hence it has been explained away as ‘lexicalized’ by analogy with a dissyllabic form, the second syllable of which begins with a vowel, since ENC (3) operates both intra- as well as intersyllabically.

The Empty Node Convention so defined is said to be universal (pp. 476, 481). However, there are numerous counterevidential forms in English throughout its history as shown in (4) that contradict ENC (3) in as much as these English forms are all regularly derived, not lexicalized, from the input structure of the form (2a).

- (4) 1. With palatal g-deletion: (a) *mægden* ~ *mæden* 'maiden'; (b) *þegn* ~ *þēn* 'thane',
2. With h-deletion: (a) *sēon* (<seohan) 'to see'; (b) *þwēales* (<þweahles, gen. sg. of *þweahl* 'washing'); (c) *fūrum* (<furhum, dat. pl. of *furh* 'furrow'), *wēales* (<wealhes, gen. sg. of *wealh* 'foreigner'); (d) *cniht* (ME knight) → post ME *nit*.
3. With r-deletion: *twīn* (<twirn) 'linen' (but see *ræn* (<rærn) 'house').

4. With nasal deletion: *fif* (<fimf) 'five', *dūst* (<dunst) 'dust'.

Hence for the Empty Node Convention to be equally applicable to English, (3) has to be revised as (5) to account for the forms in (4), which represent all the compensatorily lengthened types in English.

(5) Revised Empty Node Convention (first version)

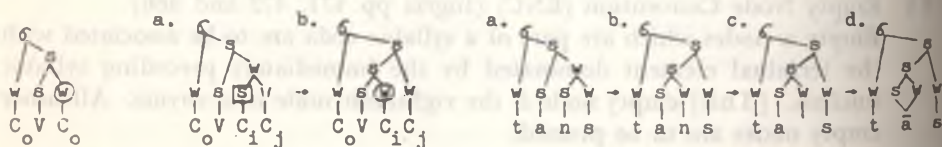
An empty *w*-node immediately dominated by an *s*-node is to be associated with its sister *s*-segment for compensatory lengthening. All other empty nodes are to be pruned.

The Empty Node Convention so revised no longer requires that the postvocalic segment that causes compensatory lengthening upon its deletion be always syllable final as previously maintained in (3). Instead it now requires that the postvocalic [-syllabic] segment be a sister constituent to the vowel to its left, with both nodes immediately dominated by an *s*-node. Thus according to revised ENC (5), compensatory lengthening occurs only when a *w*-segment, here shown circled in (6) and (7b), 'commands' the [+syllabic] *s*-segment as its sister constituent.

(6)

(7)

(8)



Since the squared *s*-segment in the coda in (7a) cannot command the *s*-segment to its left, it has to move from its original position to be sister-adjoined to the [+syllabic] *s*-segment, resulting in (7b), to which revised ENC (5) can now be applicable.

By revised ENC (5) then, even the Attic form *tās* with lengthened vowel, previously explained away as 'lexicalized', can now be correctly derived from underlying *tans* via the intermediate restructured stage (8b), as shown in (8) above.

Compensatory lengthening is an innovation, that is, the addition of a rule to a grammar, which sometimes repeats itself several times as in English. In what follows, we will examine how revised ENC (5) is able to account for the English forms in (4).

The first forms to consider are dissyllabic forms in (4.1a) and monosyllabic forms in (4.1b), (4.2d), (4.4), which can be correctly derived by (5)—the dissyllabic forms like (1) and the monosyllabic forms ending in more than one nonsyllabic segment like (8).

The next forms to consider are those of (4.2a, b, c). There are three conflicting interpretations for these forms, as summarized in (9).

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- (9) 1. (a) With lengthened vowels, only when the *h* is adjacent to the preceding vowel, hence: *seohan* → *sēon* (4.2a), *þweahles* → *þwēales* (4.2b); (b) With unlengthened vowels, when the *h* is not adjacent, hence: *furhum* → *furum* (4.2c).
 2. All with lengthened vowels, whether the *h* is adjacent or not, hence: *sēon*, *fūrum*.
 3. All with unlengthened vowels by rule (10), whether the *h* is adjacent or not, hence: *seon* and *furum*.
 (10) $h \rightarrow \emptyset / [+son] \text{ — } [+son]$

In this third interpretation (9.3) then, the intersonorant consonant *h* must be interpreted to be a segment in the onset of the second syllable, which would then be simply pruned without compensatory lengthening by revised ENC (5).

A fourth interpretation to be proposed in this paper is to allow both lengthened and unlengthened forms, the choice being determined by whether the metathesis rule (11) is applied or not; hence, *furhum* → *fūrum* with application of (11) but *furum* without.

(11) Metathesis rule

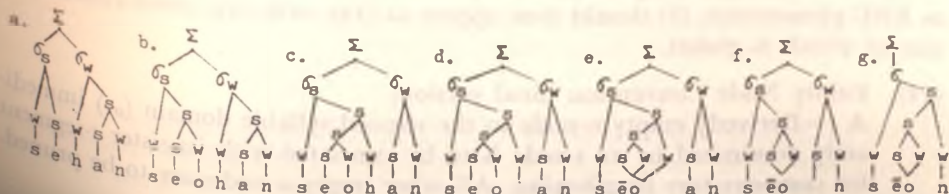
$$V \left\{ \begin{array}{c} 1 \\ r \end{array} \right\} h V \rightarrow 1 \ 3 \ 2 \ 4$$

$$1 \quad 2 \quad 3 \quad 4$$

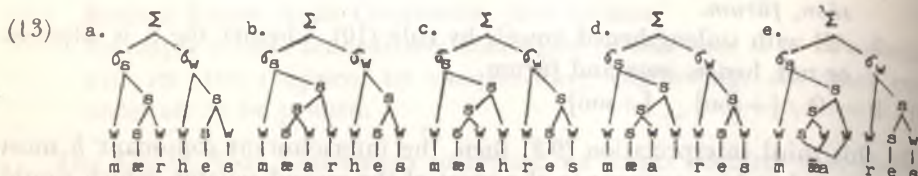
The evidence for fluctuating short and long variants for (4.2c) is usually inferred from Old English meter. Thus in *Beowulf* (see Klaeber, pp. 328, 372), the oblique case forms of *feorh* 'life' (*fēores*, *fēore*, *fēorum*) are scanned 8 times with lengthened stem vowels and only 3 times with unlengthened short stem vowels. Modern English place names like *Wales* and *Hale* from Anglian *Walh* and *Halh* respectively attest to such unlengthened short forms. On the other hand, the oblique case forms of *mearh* 'horse' (*mēaras*, *mēara*, *mēarum*) are all scanned with lengthened stem vowels. The only way to account for these coexisting long and short stem forms is by revised ENC (5)—the lengthened forms in conjunction with the optional metathesis rule (11), as described below.

The lengthened forms in (4.2a-b) with an adjacent *h* can be derived straightforward by (5), as shown in (12). Klaeber scans *sēon* as *sēon* with a circumflex accent, when required by the meter. This I interpret to reflect the dissyllabic stage *sēo-an* in (12e), with the ictus on the long first syllable and the thesis on the suffix syllable.

(12)



The co-occurring lengthened forms with a nonadjacent *h* in (4.2c), on the other hand, are to be derived via (11). The purpose of the metathesis rule (11) is to bring the nonadjacent *h* into a tautosyllabic position with the preceding vowel so that rule (5) could apply to it for compensatory lengthening, as shown in (13) below.



The co-existing forms with unlengthened short stem vowels, especially in place names, on the other hand, can be derived without the optional metathesis rule (11), with the *h* of the onset of a weak syllable simply pruned by (5), as previously explained in connection with (9.3).

The last remaining forms in (4) to be examined are *twīn* (< *twirn*) and *ræn* (< *rærn*), which are derived like (8) for the former but like (2) for the latter. It is interesting to note that even though both forms have the identical rhyme structure ending in the underlying postvocalic *rn* cluster, they surface differently, one with compensatory lengthening of the preceding vowel and the other without. The difference between them cannot be captured at the segmental level nor even at the metrical level unless ENC (3) has been revised as (5). A similar derivation with application of a metathesis rule can be illustrated from Greek (see Ingria pp. 482-83).

We have thus seen from above that all types of compensatory lengthening in English throughout its history can be accounted for by revised ENC (5).

Now, let us examine two types of monophthongization—one with compensatory lengthening as in *taym* 'time' → *tām*, *bowt* 'boat' → *bōt* in some American dialects and the other without in contradiction to (5) as in OE *hælp* → *hæalp* → ME *hælp* → *halp* 'he helped,' OE *wiht* → *wioht* → *wiht* 'person' (by Palatal Umlaut), even though both types have the identical underlying structure of the form (7b) as the underscored forms show. The difference between them seems to lie in the fact that the postvocalic glide segments in the second type are epenthetically derived, whereas those in the first type are not. Empty *w*-nodes can also be pruned if syllables lack morphological or syntactic stress, as in OE *ūtan* → *ūta* 'from outside', OE *ic* → *i*.

Now if monophthongization and [–stress] forms are to be accounted for as ENC phenomena, (5) should then appear as (14), with two added constraints, one of which is global.

(14) Empty Node Convention (final version)

A [–Derived] empty *w*-node in the stressed syllable domain (*σs*) immediately dominated by an *s*-node is to be associated with its sister *s*-segment for compensatory lengthening. All other empty *w*-nodes are to be pruned.

Thus by the global constraint, the empty *w*-nodes in the first type are to be associated with their *s*-sister segments for compensatory lengthening because they are [-Derived], whereas those in the second type are to be simply pruned because they are [+Derived]. Similarly the empty *w*-nodes in [-stress] environments are pruned as they do not occur "in the stressed syllable domain".

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An Explanation of the Japanese Accentuation by the Dual-toneme Scheme

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1

Usually, Japanese Accentuation is described as the opposite (higher or lower) pitch level on every word-forming mora.

Miyata (1927) described it simply by indicating a descending point on a word-form. In 1959, I have proposed that we should base our accentuation on the ultimate pattern, namely so-called level pitch type (*heibansiki*) at first. And in 1964, I have advanced my idea to the syllable from the mora as a seat of accent, in other words accentual nucleus of the word. There I have introduced some accentuation rules as follows.

a. Accentuation Rules for the Tokyo Dialect

- 1) The nucleus gives descending tone to the long (two-morae) syllable, and higher tone to the short (one-mora) syllable.
- 2) The preceding syllables have high level tone, excepting the beginning syllables, which has lower tone.
- 3) The succeeding syllables have low level tone.

b. Accentuation Rules for the Kyoto Dialect

- 0) The higher or lower nucleus is put on a syllable in the word-form.
- 1) The higher nucleus gives descending (or high level) tone to the long syllable, and high level tone to the preceding syllables.
- 2) The lower nucleus gives ascending (or low level) tone to the long syllable, and low level tone to the preceding syllables.
- 3) On the short syllable, the latter half of the descending or ascending tone slurs to the next syllable.
- 3') If the short syllable was in the middle position and succeeded by another short syllable, its tone should not slur; therefore nucleus gives only high level tone to that short syllable.

And, this paper is an attempt to advance my view further to the word-form from the syllable about the accentual placement.

2

It is said that accentuation of the general Okinawan—a cognate language of Japanese—is so simple as it has only two types of the word accent—descending and ascending (or level)—regardless of the length of the word.

Contrary to expectations, we find various types of word accent in the northern dialects of Okinawan, for instance as in the hamlet Kizoka.

Table 1 Accentuation of the Disyllabic Words in the Kizoka dialect (1)

High level tone:	
●● ʔhaʔzi <wind>	●● ʔkúʔri <ice>
●● ʔhuʔyü <winter>	●● ʔkiʔbá <fang>
Descending tone:	
●○ ʔuʔmi <sea>	●○ ʔmatʔci <pine>
Low level or ascending tone:	
○○ ʔmiʔzi <water>	○○ ʔnunʔgi <rainbow>
○○ ʔnaʔci <summer>	○○ ʔúʔnu <ax>
○○ ʔkuʔtú <harp>	
Ascending and descending (circumflex) tone:	
○○ ʔyaʔma <mountain>	○○ ʔuiʔbi <finger>
○○ ʔmaʔyá <cat>	○○ ʔsáʔru <monkey>

*1 ● higher level mora, ○ lower level mora.

*2 ● and ○ have long quantity.

*3 ● and ○ means each descending and ascending tone on a mora.

*4 ʔ and ʔ means each high and low level tone.

*5 ʔ and ʔ means each descending and ascending tone.

*6 ʔ the very short syllable is high and the succeeding syllable becomes lower.

*7 ʔ the very short syllable is low and the succeeding syllable becomes higher.

That is as complicated as the accentuation patterns of the classic Japanese in the Heian period (Old Kyôto dialect).

Table 2 Accentuation of Disyllabic Words in the Classic Japanese according to Kindaiti H. and Komatu H.

High level tone:	
1 ●● ʔkaʔze <wind>	
6 ●● ʔmiʔdu <water>	
Descending tone:	
2 ●○ ʔfuʔyu <winter>	7 ●○ ʔkiʔba <fang>
Low level (or ascending) tone:	
3 ○○ ʔyaʔma <mountain>	8 ●● ʔfeʔbi <serpent>
Ascending (and descending) tone:	
4 ○● ʔuʔmi <sea>	9 ●○ ʔfaʔgi <leg>
5 ○● ʔsaʔru <monkey>	

We can, therefore, describe both the classic Japanese and the northern Okinawan accentuation according to the above-mentioned rules; at that time expected forms of *mizi* 'water', *unu* 'ax', and *nungi* 'rainbow' are ʔmiʔzi,

ʃuʃnu, and ʃnunʒi. However they realize their forms as ʃmiʒi, ʃuʃnu, and ʃnunʒi in the Kizoka dialect.

I provide the correlative oppositions between *mizi* and *yama*, between *unu* and *uibi*, and between *nungi* and *saru* as the key-note through those examples. Then we can evaluate them as in the table 3(a). And *naci* 'summer', it is probably equivalent to *nungi* 'rainbow'.

Table 3(a) Accentuation of Disyllabic Words in the Kizoka Dialect (2)

	High nucleus		Low nucleus	
	on final	on initial	on final	on initial
pyrrhic	˘˘ ʃhaʒzi <wind>	˘˘ ʃuʃmi <sea>	˘˘ ʃmiʒzi <water>	˘˘ ʃyaʒma <mountain>
trochaic	˘˘ ʃkúʒri <ice>	˘˘ ʃmatʒci <pine>	˘˘ ʃuʃnu <ax>	˘˘ ʃuiʒbi <finger>
iambic	˘˘ ʃhuʒyú <winter>	˘˘ — <—>	˘˘ kuʒtú <harp>	˘˘ ʃmaʒyá <cat>
spondaic	˘˘ ʃkiʒbá <fang>	˘˘ — <—>	˘˘ ʃnunʒi <rainbow>	˘˘ ʃsáʒrú <monkey>

*0 There is another pattern: ʃnaʒci <summer>.

*1 ˘ and ˘ means each higher and lower nucleus.

*2 — and ˘ designates each long and short syllable.

And then we shall arrange the classic Japanese accentuation as follows.

Table 3(b) Accentuation of Disyllabic Words in the Classic Japanese (2)

	High nucleus		Low nucleus	
	on final	on initial	on final	on initial
pyrrhic	˘˘ ʃkaʒze <wind>	˘˘ ʃfuʒyu <winter>	˘˘ ʃyaʒma <mountain>	˘˘ ʃuʒmi <sea>
trochaic	˘˘ ʃsaʒgi <heron>	˘˘ ʃkiʒba <fang>	˘˘ ʃfeʒbi <serpent>	˘˘ ʃfaʒgi <leg>
iambic	˘˘ ʃmiʒdu <water>	˘˘ — <—>	˘˘ — <—>	˘˘ ʃsaʒru <monkey>
spondaic	˘˘ — <—>	˘˘ (ʃniʒzi) <rainbow>	˘˘ — <—>	˘˘ — <—>

Removing from the syllable quantity, turning our eyes simply on the location and the quality of the nucleus in various patterns, here comes to four patterns of accentuation, based on the dual tonemic scheme, which consists of the dichotomous nuclei, namely high and low tonemes.

Four Basic Patterns of Accentuation

High level tone:

High nucleus (or accent) on the final syllable

Descending tone:

High nucleus on the initial syllable

Low level tone:

Low nucleus (or accent) on the final syllable

Ascending (or circumflex) tone:

Low nucleus on the initial syllable

- Each nucleus can occupy the medial syllable supplementally in the polysyllabic word.

A given dialect adopts some of those patterns. And, even on the monosyllabic words, the four basic patterns are found in a proper dialect.

For instance, the Ada dialect of the northern Okinawan and the Classic Japanese (old Kyôto dialect), they have four patterns of the accentuation of monosyllabic words.

Table 4 Accentuation of Monosyllabic Words

(a) in the Ada Dialect (a Northern Okinawan)			
ʔmi <flesh>	ʔwa <pig>	ʔni <root>	ʔbo <stick>
- ʔ	- ʔ	- ʔ (▼)	- ʔ (▽)
(b) in the Classic Japanese			
ʔmi <flesh>	ʔfa <leaf>	ʔne <root>	ʔfa <tooth>
- ʔ	- ʔ	- ʔ (●) ▼	- ʔ (▽)

* ▼ or ▽ designates each high or low suffixing particle.

Thus, the two tonemes and four patterns scheme dominates all situations of the Japanese accentuation.

We know already the latinism—mora-counting syllabic accentuation—of the modern Japanese (or contemporary Tôkyô dialect). In addition, the chinoiserie—contour pitch-accent, *sisheng* or *shengdiào*—runs through the Japanese accentuations in all ages and areas. And, there would be found the connection between the register and the contour pitch-accent in the dual tonemic scheme.

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A Critical Appraisal of the IPA Cardinal Back Vowels by the X-ray Microbeam System

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0. It is hardly necessary to recall that the Cardinal Vowel System, originally devised by Daniel Jones and later adopted by the International Phonetic Association, is an extremely useful tool for phoneticians to work with in describing vocalic sounds. In its history of over sixty years the Cardinal vowel system has enjoyed a surprisingly wide currency in phonetic studies throughout the world. Ladefoged states that Jones's vowel quadrilateral is the best device ever conceived for representing vowel sounds. And he refers to the accuracy of the CV (Cardinal Vowel) system by saying that the chief merits of the CV scheme lies in the fact that there is a high degree of agreement among the phoneticians trained in the cardinal scheme as to the quality of vowel sounds indicated in the quadrilateral so that a vowel described either verbally or by means of the diagram by a phonetician can be recalled by another with great accuracy (Ladefoged 1956). However, the usefulness of a certain scheme in its actual application is one thing and the validity of the definition on which such a scheme is based and operates is quite another.

1. Are the Cardinal Back Vowels the Backest Possible Vowels?

The question is this: Are the cardinal back vowels the backest possible vowels? That Jones takes the CV to be the most extreme vowel sounds is apparent in his statements about the CV; "The vowels of well-defined quality are chiefly those in which the tongue is remote from such intermediate position, that is to say, they are those in which the tongue is markedly raised in the front or at the back or is quite low down in the mouth. It is from among those vowels which are as remote as possible from 'neutral' position that it has been found convenient to select the eight 'Cardinal Vowels' (Jones 1955)." An even more conclusive statement to the same effect is this: "CV No. 5 combines the greatest degree of 'openness' with the greatest degree of 'backness'. The tongue is incapable of being lower, and if it were retracted further, a frictional noise would be produced by the air issuing through the narrow space between the back of the tongue and the back part of the roof of the mouth and it would be a consonant (Jones 1958). It is quite clear from the statements quoted so far that the cardinal vowels, whether front or back, are defined as the most extreme vowels in their phonetic quality. That is, it amounts to saying that any vowel sound of any language can be indicated on or inside the quadrilateral and not outside. Therefore a vowel situated outside the vowel quadrilateral is not only unthinkable

but absurd in the light of the definition of the cardinal vowels. And naturally, there is no device given in the IPA Principles for dealing with such vowels as may be regarded further retracted than the cardinals.

My own view on this problem is that there are some vowels further retracted than the cardinal back vowels and consequently cannot be indicated on the quadrilateral. When I say that some vowels are further retracted than the CBV (Cardinal Back Vowels), I mean that they are so both in phonetic quality and articulation, i.e., tongue positions. Korean, for one, is a language in which such back vowels of retracted quality are found to occur. For example, the vowels in words like /kkok/ "without fail" and /mwΛ/ "what !" are pronounced by some Koreans as further retracted than the corresponding CVs, [o] and [Λ].

2. Spectrographical Analysis

In an attempt to prove that what I call RCBV (Retracted Cardinal Back Vowels) are in fact further retracted than CBV I have carried out a spectrographical analysis of CBV and RCBV. The main objective of the experiment was to compare the first two formant frequencies of the vowels on the spectrograms to see differences, if any, between the two groups of vowels. The actual material used for the analysis consists of three sets of vowels: a) the 8 primary cardinal vowels taken from Jones's record; b) the CBV [a, ɔ, o, u] pronounced by the writer in imitation of Jones's CBVs; c) the four RCBV also pronounced by the writer. The results of this experiment (which was published in *Le Maître Phonétique* No. 130, 1968, IPA, London) may be summarized as follows: 1) the 4 primary CBVs as pronounced by the writer are practically identical to Jones's CBVs in terms of formant frequencies; 2) the writer's RCBVs are characterized by much lower formant frequencies than the corresponding CBVs as shown by the following acoustic chart where the points on the continuous line represent Jones's CBVs and the four points on the dotted line the writer's RCBVs. It is noted that the dotted line is well outside the continuous line, suggesting that the RCBVs are backer than the corresponding CBVs. It is also noted that the RCBVs are not only further back but also higher in relation to the CBVs with the exception of [u-], which is on the same level as [u].

The results of this experiment seem to justify my point that RCBVs are in fact backer in acoustic terms than the CBVs.

3. Articulatory Analysis by X-ray microbeam system

In an attempt to get a supplementary and more direct evidence concerning the nature of RCBVs in relation to CBVs the writer has conducted an experiment using the X-ray microbeam system. In this experiment conducted at the Institute of Logopedics and Phoniatrics, University of Tokyo with the help of Drs. Sawashima, Kiritani, Hirose and Ito, and with the writer as the subject, three small metal pellets were attached to the surface of the tongue; pellet 1 on the tongue tip, pellet 2 behind the centre of the tongue and pellet 3 near the back of the tongue. In addition, three more pellets were attached as reference

points; pellet 4 on the lower incisors, pellet 5 on the lower lip and pellet 6 on the nose (see Fig. 2). While the subject pronounced each of the 4 CBVs followed by the corresponding RCBV by retracting the tongue further back, the pellet displacements were traced by the x-ray microbeam. An over-all analysis and comparison of the pellet displacements for CVBs and RCBVs reveals that the tongue positions (configurations) for RCBV are backer (further retracted) and lower than for the corresponding RCBVs as shown in fig. 3a-3d. This may be taken as a definite indication that there are some back vowels pronounced further back than the CBVs which can not be incorporated in the IPA Cardinal Vowel System as currently defined and practiced.

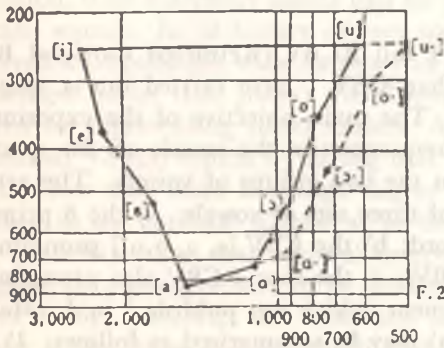


Fig. 1 Acoustic chart of Jones's cardinal points and Lee's retracted back vowels

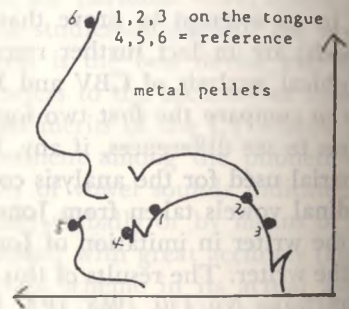


Fig. 2

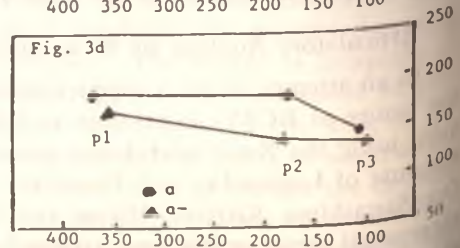
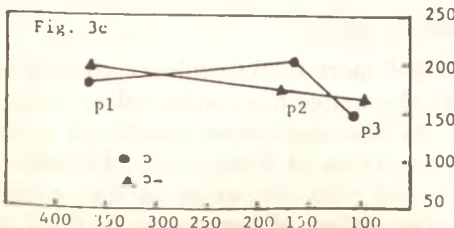
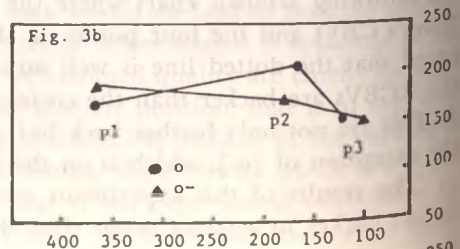
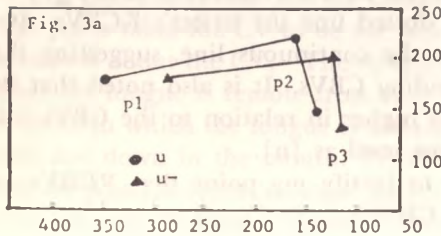


Fig. 3 Tongue positions for CBV and RCBV based on pellet displacements

4. Conclusions and Suggestions

The results of both the acoustic and articulatory experiments seem to prove my point that there are some vowels further retracted than CBVs. And it is certain that a minor revisions is called for in the Cardinal Vowel System, specially with regard to the back vowel area, if CV system is to continue to serve as a universal yardstick for vocalic sounds as it is intended to be.

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Phonetics and phonology: The Example of Intonation

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1. Since intonation has not yet received a definite status in linguistics, phonetic and phonological studies in this field are not very much related to each other. Phonetic research on intonation is very often devoted to a purely empirical description of acoustic features such as fundamental frequency, intensity, syllable duration, rhythm, and the like. Significant results obtained are not linked to a definite linguistic description, but rather to other fields related to speech science, such as physiology or perception. On the other hand, current phonological approaches (e.g. Selkirk, 1980) deal generally with very abstract phonological features (high, mid, low tones for example) on prosodic domain (syllable, foot, phonological phrase and so on), whose phonetic correlates are not obvious, and experience sometimes difficulty to link its results to other linguistic facts such as syntax.

2. The description of the intonation-syntax relationship is a particular example in the field of prosody. It is commonly felt that intonation is in a certain way linked to the syntactic structure, but few studies have actually shown how sentence melody may cue syntax. Indeed, the large number of intonation patterns that can be observed makes this correlation difficult to establish. Discouraged phonologists have often left the field to phoneticians, who either come with claims which are difficult to integrate in any coherent linguistic framework, or simply deny that any relation between intonation and syntax can exist.

3. From a methodological point of view, it is well known that experimental data can only be effectively described and understood when approached through a particular theory. Otherwise, the description of empirical data leads to disorganized information, which can often be difficult to interpret. A trivial and, of course, purely illustrative example of this would be the phonetic description of the relation between intonation and sentence modality in French.

The following three sentences

(1) Est-ce que Pierre est venu?

(2) Pierre est-il venu?

(3) Pierre est venu? (with rising intonation on the final syllable)

are interrogative counterparts of the declarative sentence

(4) Pierre est venu.

Since the interrogative modality in (1) and (2) is indicated by morphological or syntactic markers, rising intonation is redundant and need not necessarily be present in these sentences, whereas in (3) it constitutes the only indication

of an interrogative modality. Looking at a possible correlation between intonation and modality, a naïve phonetic description would group all realizations (1), (2) and (3), and by averaging the falling and rising contours actually observed, would obtain a flat or slightly rising intonation to signal questions, not significantly different from the average contour obtained for declarative sentences. Such a conclusion could of course be avoided if the description started from the linguistic analysis of the interrogative markers in the sentence.

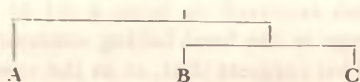
4. Surprisingly enough, this situation prevails for the studies on the intonation-syntax relationship, to the point that some researchers concluded that there is no clear evidence for phrase-level planning of fundamental frequency implementation (see Liberman and Pierrehumbert, 1982). By contrast, a phonosyntactic model such as the one I proposed earlier (Martin, 1975, 1981) appears radically different. Taking a phonological approach, it integrates typical phonetic characteristics, essentially fundamental frequency movements located on stressed vowels, into a language-specific mechanism indicating the prosodic structure of the sentence. It can be summarized as follows.

Utterances have a syntactic structure (SS) and a prosodic structure (PS). These structures are independent and associated to each other in the sentence. As the SS, the prosodic structure is a hierarchical organization in the sentence. It is composed of minimal prosodic units (or prosodic words) where each prosodic word contains exactly one syllable prominence or stress (not an emphatic stress). This stress carries on its corresponding vowel a melodic movement or melodic contour, which can be described by language-specific phonological features.

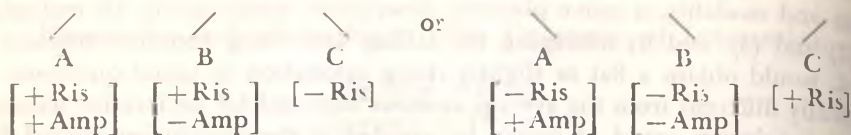
In French, an appropriate set of features is:

- [±Extreme]: the contour attains an extreme low (declarative case) or an extreme high (interrogative case) frequency level as compared to the other contours;
- [±Rising]: when the fundamental frequency (presumably the perceived frequency contour) is rising (or falling);
- [±Ample]: when the melodic variation is large (restrained) as compared to the variation of similarly rising or falling contours.

5. The melodic contours located on stressed vowels are used to indicate the dependency relations existing between the prosodic words, which, in turn, determine the PS. In French, a melodic contour indicates simply to which other prosodic word located at its right it is to be linked in the structure. The mechanism involves a contrast in slope (rising vs. falling), while the amplitude of the melodic variation is used to differentiate between 2 words which depend on a same third prosodic word, but at different levels in the structure. Thus if A, B and C are three prosodic words, organized in the following PS



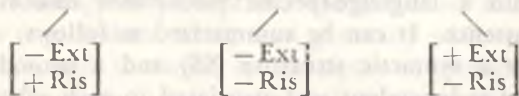
the corresponding melodic contours will be



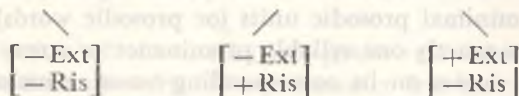
depending if C is falling or rising.

6. The rules of association between PS and SS are fairly complex (see more details in Martin, 1981). Since PS and SS are a priori independent, and have different geometrical constraints, the isomorphism between the two appears more frequently in specific speech acts such as reading. In the case of isomorphism, the simple examples given below would have the following sequences of melodic contours (stressed vowels are underlined):

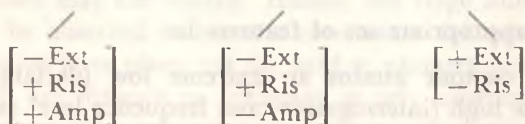
- (5) Les marins, le matin, travaillent.



- (6) Le président du syndicat a demissionne.



- (7) Arthur est champion de tennis.



7. Looking at the phonetic realizations of these contours for read sentences, it can be shown (Martin, 1982) that final contours always attain a lower intensity and frequency levels than the other contours (in the declarative case). As for the non-final contours, only direction and amplitude of melodic variations and, to a lesser degree, contour duration, play a significant role, whereas other phonetic features such as intensity or frequency level appear to be irrelevant.

Such a phonetic description of French intonation could only be obtained thanks to a preliminary phonological analysis of the problem (which was only sketched here). By contrast, many empirical studies found in the literature provide overall phonetic informations which are generally very difficult to integrate in any phonological description of intonation.

Obviously, if sentences of the syntactic types (5), (6), (7) were mixed and their intonative realizations analyzed to form a set of prosodic data, the only invariance that could emerge is the final falling contour, together with the well known declination line. This suggests that, as in the case of the acoustic analysis

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of vowels, for example, the phonological analysis should precede the phonetic description of intonation. Otherwise, there is little hope that phonetic studies can bring any convincing information on the intonation-syntax relationship.

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Stress and Quantity in Middle English Loanwords from Old French and Norman French

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Vowel quantity and stress have been closely related throughout the history of English. In Middle English (ME), short stressed vowels in open syllables were lengthened in native words but not necessarily in words borrowed from Old French (OF) and Norman French (NF).

OF/NF vowels in stressed syllables were borrowed into ME as long vowels as long as they kept the original stress: *cāpe*, *plāte(s)*, *māten*, *cāve*; *degrē* "degree", *cēte* "whale", *chēre* "cheer"; *delīten* "delight", *quīten* "requite", *arīve(d)* "arrive"; *nōte*, *rōbe*, *trōne*; *dūte* "doubt", *rūte* "route", *spūse* "espouse"; *spūtin* "dispute", *escūsen* "excuse", *ūse(d)* "use". This type of borrowing is stated as follows: (I) OF CVCV(C) > ME CVCV(C). This lengthening is regarded as a rule of ME loan phonology from OF/NF and requires no sound change in OF/NF or in ME.

When the stress shifted in the course of borrowing into ME, vowels in derivationally stressed syllables were perceived short or long according to their contexts: *chāpel*, *Lātin*, *chātel* "chattel"; *nāture*, *nātion*, *bāsin*, *lābour*, *mētal*, *mēsure*, *bēril* "beryl"; *trēte* "treaty", *dēver* "duty", *jēlos* "jealous"; *cīte* "city", *pīte* "pity", *licur* "liquor"; *silence*; *prōphet*, *hōnour*, *pōtel* "pottle"; *devōtion*, *ōcean*, *nōtory*; *sūmmon*, *glūton* "glutton", *mūton* "mutton"; *cūrat* "curate"; *stūdie* "study", *cūischin*, *dūchesse* "duchess"; *mūsike* "music", *hūmour*. The above instances indicate that the non-high vowels in penultimate syllables remained short before *r*, *l*, *n* and words ending *r*, *l*, *n* or *t*; otherwise they were lengthened. The high vowels *i*, *ū*, *u* generally remained short. These borrowings are stated as follows: (IIa) OF CVCV(C) > ME CVCV(C) and (IIb) OF CVCV(C) > ME CVCV(C). The limitation of lengthening to non-high vowels in Midland and Southern dialects parallels ME Open Syllable Lengthening (ME-OSL), which applied to all stressed vowels in Northern dialects but only to non-high ones in Midland and Southern dialects. In Northern dialects even the short high vowel *i* of OF/NF loanwords was lengthened as *ē* by ME-OSL: *cēte* "city", *pēte* "pity", *rēver* "river", *prēsoun* "prison", *mērouer* "mirror", *dēner* "dinner". The word-medial liquids *r*, *l* and nasal *n* did not block lengthening of the originally stressed vowels: *māle*, *clēre* "clear", *sīre*, *nīle*, *fīn* "fine", *trōne*. Nor did the word-medial liquids *r*, *l* prevent ME-OSL from lengthening the stressed vowels of native words: *cāre*, *bāre*, *bēren* (OE *beran*), *dāle*, *smāle*, *mēle*.

The phonetics of a loaning language must be modified when the borrowing language possesses a different phonetic system. Unsuppressed natural processes then produce nativized outputs. The borrowing language must bear a burden when nativizing foreign words. The quantity and quality of this burden do not seem to be the same in every period. The amount of the burden in earlier periods is greater than in later periods. Late borrowings retain the French stress on the last syllable, contrary to the English manner of throwing the stress back. *Façade* [fəsá:d, fə-; F. *fasad*], *potade* [pəmá:d, pə-; F. *pommade* pɔmad]; *ballet* [bæléi; F. *bale*], *bouquet* [bu(:)kéi; F. *buke*]; *fascine* [fæsi:n, fə-; F. *fasin*], *elite* [eilít; F. *elit*]; *chaconne* [ʃa:kó(:)n, -koun; F. *ʃækon*], *bureau* [bjúərou, -----; F. *byro*]; *debouch* [dibáuts, -bú:f; F. *deboucher* debuʃe], *recoup* [rikú:p; F. *recouper* rəkupe]; *perdu* [pə:djú; F. *perdy*]. The originally short stressed vowels of Late French words are perceived as long in Modern English. The correspondence between ModF and ModE vowels is shown below: F *i* > E *i*, F *ü* > E *jü*, F *ü* > E *ü*, F *ë/ê* > E *ei*, F *ö/õ* > E *ou*, F *ä* > E *ä*. Thus, the borrowing is stated as follows: (III) ModF CVCV(C) > ModE CVCV(C).

For certain words there exist variants of (III) in American and especially British English in which stress-shift is allowed to operate: *éclat* [A. *eiklá*; — —/B. — —; F. *ekla*], *croquet* [A. *kroukéi*/B. *kráukei*; F. *krøke*], *chateau* [A. *ʃætóu*/B. *ʃætəu*, *ʃá*-; F. *ʃato*], *trousseau* [A. *trú:sou*, — —/B. *trú:səu*; F. *truso*], *ragout* [A. *rægú*/B. *rægu*; F. *ragoût* ragu]. This manner of borrowing is characterized as follows: (IV) ModF CVCV(C) > ModE CVCV(C).

When we consider rules of loan phonology in terms of suppression of processes in the borrowing language, the relations between the rules (I), (IIa), (IIb), (III), and (IV) are clear: (I) allows (i.e. fails to suppress) vowel-lengthening. Stress-shift is left unsuppressed in (IIa) and (IIb), with vowel-lengthening operating in (IIa) but suppressed in (IIb). The more recent rule of loan phonology (III) differs from the older forms (IIa) and (IIb) in that stress-shift is suppressed. However, this suppression is not total in that, at least for certain words, the variant rule (IV) is also possible.

It should be noted that the outputs of (I) and (IIa) differ in stability because the latter creates a greater burden than the former in that it suppresses the process of stress-shift. Our analysis predicts a possible output in the next period which is a less nativized and more frenchified form after suppression of a certain process. Once a set of unsuppressed natural processes is specified as a rule, it affects inputs simultaneously without any false steps or segments. Thus, our loan phonology analysis provides a happy solution for description of borrowing sounds from a foreign language.

There remains the question of why the rules of loan phonology have taken place in that order, or of whether it is possible that these rules could have applied in a different order. The existence of ME-OSL motivated (I) and (IIa) to produce long stressed vowels as common outputs even if the rule (IIa) contains the process of stress-shift. The rule (IIb) interestingly may have motivated the application of Back Shortening which is one of the native rules: *hamer*,

feter, coper, sadel, hovel, seven, troden. Later, Modern French long stressed vowels in word-final position were borrowed as long vowels in Modern English. This manner of borrowing motivated the rule (III). The stress-shift in (IIa, b) on one hand and the word-final long vowel in (III) on the other lead to the rule (IV). The process of stress-shift was suppressed with the introduction of the long stressed vowels in word-final position and, thus, the range of variation was broadened.

Finally, is there any possibility that the rules (I) and (IIa) would have arisen before ME-OSL or that the rule (III) would have appeared before the ModE borrowing of long stressed vowels in word-final position? Before the advent of ME-OSL, there was no motivation for lengthening borrowed vowels in open syllables. The burden of borrowing would have been much greater because the lengthening was highly marked at that stage. Similarly, before the adoption of word-final long vowels, there was no motivation for lengthening stressed vowels in word-final position and the borrowing burden of (III) would have become greater as stressed word-final vowels were only adopted with distinct markedness. In this way, suppression of certain processes has extended the scope of English phonology. Unmarked suppression of processes then follows easily once marked suppression enters the phonological component of English. Could the rule (IIa) have occurred after the long stressed vowels in word-final position appeared in Modern English? I answer in the positive because the rule (IV) is quite similar to (IIa) but produces less nativized and more frenchified forms.

In conclusion, Old French/Norman French loanwords in Middle English are characterized in terms of loan phonology not in terms of English or French native phonology. The rules of this loan phonology are interpreted in terms of the degree of suppression of natural processes. The suppression of marked processes invites drastic change in borrowed forms while unmarked processes are more easily suppressed.

La (dé)nasalisation en français: phonologie ou morphologie?*

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Le problème auquel nous nous adressons est le problème classique du statut synchronique des alternances entre VN (voyelle nasale) et les suites VO+CN (voyelle orale et consonne nasale) telles qu'on les observe par exemple dans *bonifier, bonnement, bonne, bonté, bon*, où la terminaison *3* alterne avec /ɔn/. Nous proposerons que ces alternances sont le résultat d'allomorphies limitées à un nombre relativement réduit d'affixes de statut morphologique particulier que nous appellerons thèmes ou terminaisons thématiques. Nous nous limiterons dans cette discussion aux cas des alternances entre masculins et féminins des noms et adjectifs.

1. La solution de Tranel pour les alternances masculin-féminin.

1.1 Tranel (1981) suggère que les masculins et féminins des noms et adjectifs tels que *bon-bonne* ont une représentation phonologique unique avec VN et que le féminin est dérivé par des règles morphophonologiques. Il étendra cette analyse aux alternances masculin-féminin se manifestant phonétiquement par la présence d'une consonne supplémentaire au féminin.

1.2 Tranel postule une forme phonologique unique pour les deux genres pour expliquer entre autre l'absence de glissement sémantique entre les deux genres. Il apparaît cependant que des glissements sont possibles même s'ils sont relativement peu fréquents.

1.3 Il propose un mécanisme de correspondance entre le timbre des voyelles des masculins et des féminins relativement arbitraire, en ce qu'il exploite des dénasalisations historiques qui ne s'observent pas dans tous les dialectes du français soumis aux mêmes types de conditionnement. De plus ce mécanisme ne s'étend pas aux correspondances (phonologiquement imprévisibles dans son analyse) de timbre des alternances du type *idiot-idiote* /idjo/-/idjot/, ni aux cas où le féminin s'obtient par ajout (ou par modification) d'une consonne finale. Une solution plus adéquate, dans l'esprit de l'analyse de Tranel, consisterait à indiquer dans le lexique comment on doit "changer" la terminaison du masculin pour former le féminin. Cette nouvelle analyse implique des possibilités de correspondance arbitraire. En pratique, celles-ci sont limitées à un ensemble relativement petit dont nous avons des exemples ci-dessous:

* La recherche rapportée dans cet article ainsi que la participation de l'auteur au 13e Congrès international des linguistes à Tokyo ont été subventionnées en partie par le Conseil de recherches en sciences humaines du Canada.

(1) exemples de correspondances entre terminaisons du masculin et du féminin

masc.	fem.	masc.	fem.	masc.	fem.
/ɛ/	/in/	/ø/	/øz/	/i/	/i/
/jɛ/	/jen/	/œr/	/øz/	/if/	/iv/
/ɔ/	/ɔn/	/je/	/jer/	/wa/	/waz/
/ā/	/an/	/e/	/e/	/o/	/ot/

Nous proposerons que les correspondances du type (1) ont un statut précis dans la langue et constituent en quelque sorte un stock ou un inventaire connu du sujet parlant qui lui permet d'une part une économie dans l'organisation de son lexique—car il minimise le degré d'arbitraire des correspondances (en augmentant la redondance interne du lexique)—et qui lui offre un modèle pour la formation des masculins ou des féminins à partir de la forme de l'autre genre. Plus précisément, nous donnerons un statut morphologique précis à chacune de ces correspondances que nous appellerons TERMINAISONS THÉMATIQUES ou plus simplement THÈMES.

2. Une analyse morphologique des alternances masculin-féminin.

2.1 Suivant, entre autre Aronoff (1976), nous définissons les morphèmes comme des unités minimales d'organisation de la deuxième articulation. Une suite de phonèmes peut avoir un statut particulier dans la langue dans la mesure où l'on peut associer à cette suite des caractéristiques propres indépendantes de sa forme phonique, ou pour rappeler un terme plus évocateur, des caractéristiques arbitraires.

2.2 Il est clair que chacune de ces correspondances définit un morphème au sens que nous venons de rappeler. Par exemple, la "règle morphophonologique" qui fait correspondre /ot/ (fem.) à /o/ (masc.) est tout à fait arbitraire: l'ajout de la consonne /t/ (plutôt que /n/, par exemple) n'est pas prévisible à partir d'autres propriétés de la langue, ni, de la même manière, le timbre /o/ de la voyelle correspondante (on aurait pu avoir /o/ comme dans la correspondance *haut-haute* /o/-/ot/).

Le statut des alternances VN/VO+CN et plus généralement des alternances par modification de la finale cesse d'être (morpho)phonologique pour devenir morphologique avec des allomorphies limitées à un nombre relativement réduit de thèmes de la langue. Dans un grand nombre de cas, cette allomorphie semble être de nature supplétive. Il n'est pas exclu cependant que dans certains cas il y ait une relation morphophonologique entre les deux allomorphes d'un même thème.

2.3 La solution morphologique présentée ici, propose donc que les formes phonologiques sous-jacentes associées aux masculin et féminin des noms et adjectifs sont différentes, mais que les entrées ont un découpage morphologique qui met en évidence une racine commune et leur thème. Ceci pourrait avoir la forme (2):

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- (2) cochon $[[k\alpha f]_r + [\beta]_{th_1}]$
 cochonne $[[k\alpha f]_r + [\alpha n]_{th_1}]$

Dans ces représentations th_1 est un thème de genre. Les thèmes de genre sont certainement distincts des thèmes notés dans les dérivations, comme *cochonner* ou *cochonnet*, où le morphe $/\beta/$ ici sera associé à un th_2 ayant un autre conditionnement morphologique:

- (3) $th_1 = \{/\beta/ \text{ (masc.)}, /\alpha n/ \text{ (fem.)}\}$
 $th_2 = \{/\beta/ \text{ (f. libre)}, /\alpha n/ \text{ (f. liée)}\}$

Une terminaison dans un découpage morphologique pourra donc appartenir à plusieurs thèmes différents; ce qui veut dire que l'appartenance à un thème donné fera appel à un ensemble de traits—nous n'avons pas encore exploré les conséquences de cette possibilité). Ceci permet d'expliquer l'organisation lexicale dans laquelle *maison* peut avoir un dérivé *maisonnette*, donc un découpage morphologique avec un th_2 sans avoir tendance à prendre la forme **maisonne* que présupposait l'analyse de Tranel. Le th_1 doit d'ailleurs certainement contenir des informations sémantiques ou syntaxiques limitant son usage à des cas où l'opposition de genre est pertinente, ce qui expliquerait pourquoi les pressions analogiques ont pu agir sur les adjectifs *marron* ou *châtain*, les noms *cochon* et *colon* qui peuvent donner les féminins *marronne*, *châtaine*, *cochonne* et (au Québec) *colonne*, sans affecter des noms féminins comme *maison*.

Finalement, cette analyse n'interdit plus le glissement sémantique entre les deux genres.

3. Conclusion.

Dans cette réanalyse des alternances entre VN et VO+CN nous avons accepté une grande partie des arguments de Tranel pour rejeter l'analyse générative traditionnelle. L'analyse précise et bien documentée de Tranel ne laisse aucun doute quant au conditionnement essentiellement morphologique de ces alternances et quant à la nécessité d'un traitement distinct pour les différents types de conditionnement. La solution de Tranel—qui explique les alternances du masculin-féminin en postulant une forme phonologique commune aux deux formes (celle du masculin) et des règles morphophonologiques définies par des schémas et des tableaux de correspondance qui préfigurent déjà nos thèmes—est à son tour incapable de rendre compte des données qu'il cherche à décrire. C'est que ces alternances sont le fait d'un nombre relativement réduit de correspondances entre certaines terminaisons.

Notre solution propose que ces alternances sont le résultat de règles d'allomorphies affectant une certaine classe de thèmes. La même solution s'étend aux autres cas d'alternances entre VN et VO+CN. Les allomorphies elles-mêmes sont peut-être dans certains cas le résultat de règles morphophonologiques, mais correspondent plus généralement à des supplétions. On pourrait reprocher à notre solution son faible pouvoir explicatif, étant donné

les grandes possibilités d'analyses qu'elle permet, et par suite, la difficulté de réfutations possibles. C'est effectivement un problème: le concept de thème, tel qu'il est défini ici reste encore assez allusif; de plus nous n'avons que peu de moyens pour découvrir comment les sujets parlants organisent leur grammaire, et les moyens indirects (acquisition de la langue, changement historique) ne sont pas précis. Mais même avec son indétermination présente, nous pensons que le concept de thème joue un rôle important dans l'organisation lexicale des langues que d'autres études viendront préciser (cf. Morin 1982).

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Acoustic and Perceptual Correlates of Stress in Hindi

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Based on impressionistic studies it has been claimed that Indo-Aryan languages such as Hindi lack stress entirely (Bloch 1930) or at least do not have distinctive stress (Kelkar 1968). These claims if true, have important consequences both theoretically, e.g., in discussions of universals of accent, and practically, e.g., in second language teaching and machine recognition of Hindi. Therefore experimental evidence is desirable on this point. Instrumental studies have shown that pitch contour, segmental and syllable duration, intensity and a few other lesser phonetic features are the primary cues of stress in other languages. Are there then, any systematic variations in these parameters, especially pitch and duration, that cannot be predicted from the overall sentence intonation or from the syllables and segments that make up utterances in Hindi?

To discover whether there is a correlation between duration or pitch variations and the syllable identified as stressed I conducted the following two studies.

Duration: A list of 36 polysyllabic Hindi words was compiled and the predicted location of stress identified according to Kelkar's algorithm (for the majority of these words other writers' algorithms for stress placement would give the same results).¹ A recording was made of an adult native speaker pronouncing these words in isolation and in a sentence frame. The duration of the "stressed" syllable was measured by hand from oscillographic tracings made from this recording.² The duration of what was predicted to be the stressed syllable was not consistently longer than phonetically comparable unstressed syllables.³ This finding is in accord with Hyman's (1977) claim that there is a universal tendency against the use of duration as a primary cue for stress by languages (such as Hindi) which use length contrastively on segments.

1) See M. Ohala (1977) for details on stress placement algorithms of Dixit (1963), Mehrotra (1965), Kelkar, and Sharma (1969). Most of these use the concept of syllable weight and are overall very similar. E.g., when all syllables are of equal weight they assign stress to the penult, e.g. [pakistāni] 'Pakistani', [təmbāku] 'tobacco'. But in other cases different predictions are given, e.g., Kelkar: [kʰūbsurət] 'pretty', others: [kʰubsūrət]; Mehrotra [əmita] 'a name', others: [əmitā]; Sharma: [ašīrvād] 'blessing', others: [āršivād]; Dixit: [pakīstan] 'Pakistan', others: [pakistān].

2) The vowel and the coda nonsyllabics were measured as it was these portions that Kelkar claimed would be longer in stressed syllables.

3) Although I have not been able to verify the claim that variations in durations are a significant correlate of stress, I would prefer to be cautious about this since not all possible duration measurements have been made.

Pitch: A list of 38 polysyllabic words (including some used in the above-mentioned study) was compiled, spoken, and recorded, as before by 4 adult native speakers. Oscillographic tracings (including a pitch signal) were obtained from the recordings of 3 of the speakers and narrow-band spectrograms from the fourth. The results showed that the "stressed" syllables were characterized by a high or rising pitch and/or by a low or falling pitch on the syllable following (see Figure 1a). Sometimes this syllable did not have a rising pitch on it but the following syllable did have a falling pitch (see Figure 1b). There were a few words that had ambiguous pitch patterns.

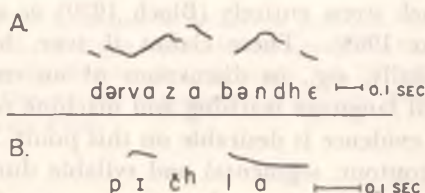


Figure 1. Pitch curves for two Hindi utterances.

Auditory analysis: As a prelude to more rigorous investigations involving speech synthesis, an experiment involving simple auditory analysis was also conducted in order to find out whether stress placement would vary depending on the context in which the word appeared.

Five native speakers were asked to read 40 words (which were interesting for stress placement) in isolation, and in the frames: [apne kəha] 'you said', [apne dekʰa] 'you saw'. I noted the syllable I heard as stressed. The words [kəha] and [dekʰa] were used to provide a difference in the sentence frame since in [kəha] the first syllable is light and in [dekʰa] it is heavy. Finally, I pronounced the words in these two frames varying the stress and asked the subjects for their preference. For example: [apne dərva za kəha] 'you said door', [apne dərva za kəha], [apne dərva za kəha]. The results showed that:

(a) Stress may indeed vary between the word in isolation and when it was in a sentence. For example, in isolation 2 out of 5 subjects preferred stress on the first syllable of [pučkar] 'cooing sound', however in sentence frame all the subjects preferred the stress on the second syllable.

(b) In words of VCVCV structure the short vowel is never stressed. Thus words such as [pətʰrila] 'stony', [təmbaku] 'tobacco', [dərva za] 'door', could not receive initial stress.

(c) The heaviest syllable doesn't always receive the stress. All stress assignment rules which have been proposed previously place the stress on the first syllable of [māmla] 'affair'. However, 4 out of 5 subjects preferred [māmla] with the stress on the second syllable.

(d) In sentence frame if the final syllable is heaviest, i.e., VC, then it receives the stress. Thus: [rozgār] 'job', [kəmzór] 'weak', [əkʰrót] 'walnut'.

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[pučkar] 'cooing sound'.

Conclusion: Stress placement patterns in Hindi support two previous claims about phonological universals of stress: (a) stress placement is governed by syllable weight which correlates roughly with intrinsic duration, and (b) stress is placed with reference to a word boundary, in this case the word boundary to the right. The implication of this second claim is that stress serves at least a culminative function, and possibly a delimitative one (Trubetzkoy 1969, Hyman 1977), i.e., it provides the listener with cues for the location of word boundaries in the stream of speech.

Hyman claims that syllable weight is a common factor in stress assignment rules since the speaker needs time to manifest a pitch contour on a syllable—the longer a syllable is (i.e., the heavier it is), the more it lends itself to this.

It may even be possible to elaborate on his model in a way that would account for my finding of the shifting stress seen from the word in isolation condition to the word in sentence context. Overall one could say that the optimal environment for the manifestation of stress is, first, a heavy (i.e., long) syllable on which to put a pitch rise and, second, a syllable following on which to put a pitch fall (this is in harmony with Bolinger's (1980) claims). Thus given the word [pučkar] 'cooing sound' spoken in isolation, scanning the word from right to left one would find that stress (and the pitch rise) would best be placed on the initial syllable if one had to reserve the last syllable for the pitch fall. However when this word is spoken in a sentence context such as [apne pučkar kəha], the pitch rise can be placed on the last syllable because the syllable following that ([kə]) can take the pitch fall. Thus in isolation the word may have initial stress, but in mid-sentence context, final stress.

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Phonological Diversity & Sound Change in Shanghai

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The Chinese languages have recently been cited as an important source of data in support of a general theory of linguistic change which seriously challenges the traditional neo-grammarians view of sound change. This theory, called *lexical diffusion* by William S-Y. Wang, who coined the term in 1969, proposes that sound change is phonetically abrupt yet lexically gradual. Since this proposal counters the orthodox view it has fostered some controversy in the subsequent years since Wang first suggested the term. Moreover it has had the important secondary consequence of putting data from the Chinese languages solidly into the public domain of general linguistic scholarly debate, removing, one would hope, once and for all, the non-specialist's view that evidence from Chinese—and perhaps from other Oriental languages as well—is somehow too inaccessible, too exotic, or too far removed from the mainstream of Western man's intellectual development to contribute significantly to our understanding of the nature of language.

Whatever one's individual view on the mechanism of linguistic change, those of us who work on Chinese need to be aware of the impact of this contribution to our field of study, and it behooves us to make available as we come across it new evidence by which this theory may be tested. This is by no means to imply that the theory of lexical diffusion is wholly or even substantially dependent on data from Chinese. It is just that the Chinese evidence appears particularly cogent.¹⁾ This is due in part both to the nature of the information we have on, and the actual historical developments which are apparent in, sound change within the Chinese languages.

The internal evidence we have on sound change in Chinese comes primarily from two sources: dialect data available in studies of the various languages now spoken on the Chinese mainland used as the basis for internal reconstruction of earlier stages of Chinese; and the written records of traditional Chinese phonologists which take us back as far as the early centuries of the first millennium. In gross terms, what we see happening over the past 1500 years or so is an overall simplification of the phonological structure of the Chinese syllable. Of particular interest in the general study of the mechanism of sound change is the complex process of collapsing and splitting by which the earlier contrasts in the laryngeal features of voicing, glottalization, tone contour, and tone

1) For an interpretation of Chinese data which challenges the view proposed by Wang see Chan 1982 and Egerod 1982.

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register have been reduced, compacted, or eliminated in the modern reflexes in the Chinese languages.

The *Wu* dialects of southeastern China are especially helpful in this research since in many respects they are still phonologically rather complex, represent relatively conservative strains, and hence point towards a stage of development on the continuum of phonological change less recent than that evidenced by standard Mandarin Chinese, for example. Historically, the speech of the city of Suzhou was taken to be representative of the *Wu* region. In modern times though, if we are concerned with investigating the speech of the center of human activity in that part of the world, we must look to Shanghai. However, choosing Shanghai as the representative of the *Wu* dialect region (which is China's second largest language family, after Mandarin, in terms of number of speakers) poses several interesting problems. The foremost of these, according to recent linguistic reports, is that it seems virtually impossible for linguistic investigators to agree on a synchronic description of the modern Shanghai phonological or lexico-grammatical systems, irrespective of whether one speaks in terms of what used to be called the *common core* or *overall pattern*. To capture the phonological structure of Shanghai as it is being spoken in the metropolitan area at this time, it is apparently necessary to describe several strata of the language, each successively more complex in its phonology, more or less in accordance with the advancing age of the speaker in question, and each co-existing and being in daily use synchronously within a definable linguistic community.

The gap between the least complex Shanghai phonological inventory (in terms of segmental contrasts, tones, and tone sandhi patterns) of the youngest speakers and the phonologically most complex speech of the eldest speakers is so great as to suggest that there would exist severe communication difficulties between the two extremes were there not enough redundant contextual information in the flow of the utterance. Between these two extremes of speech it is possible, and apparently necessary, to describe intermediate, discrete levels of phonological complexity, i.e. quantified or abrupt phonological changes in the language. Furthermore, recent investigations have found it necessary in describing tone sandhi patterns in Shanghai to posit two or more types of "lexical usage" for Shanghai speakers, cutting across age levels. These types of usage determine whether or not certain types of tone sandhi will occur, and it appears that the range of this occurrence varies from speaker to speaker and, for a single speaker, from lexical item to lexical item, i.e. a gradual or diffuse spreading across the lexicon of a quantified or abrupt phonological change.

This linguistic situation in Shanghai is an impressively unstable one, of course, with fairly clear historical and socio-political origins; most probably it will not last another generation. What is of paramount interest to the phonologist and the historical linguist here is that in many senses we have a microcosm of sound change observable in process within the city of Shanghai, and that it is possible—in the eye of the investigator at any rate—to discern discrete strata of phonological change which have already ossified and which co-exist

Phonological change observed in *A Lexical Survey of the Shanghai Dialect* (1982)

	Nature of Change	Degree of Change Observed	Comments
0	Merger of voiced & voiceless initial consonants	unobserved stable	
0	Merger of closed syllable finals with [-V?] and open syllable finals [-V]	unobserved stable	
0	Merger of historical lower register (阴) tones with upper register (阳) tones	unobserved stable	
1	Merger of syllable finals [-i] & [-i]	incipient stable	Still in complete phonological contrast with only a few attested irregular examples of merger, all lexically specific.
1	Merger of syllable finals [-o?] & [-o?]	incipient stable	Very high phonemic load. Few attested examples of non-contrasting merger, all lexically specific.
1	Merger of syllable finals [-i-i?] & [-i-i?] after initial affricate [ʃʈ-]	incipient stable	Very high phonemic load. Few attested examples of non-contrasting merger, all lexically specific.
1	Merger of syllable finals [-ə], [-e], & [-e]	incipient unstable	No true merger in slow, careful speech but phonological contrast can be blurred in normal discourse. Lexically limited.
2	Merger of syllable finals [-ə] & [-e]	moderately advanced stable	Still in complete phonological contrast, but with frequent examples of merger, all lexically specific.
2	Merger of syllable finals [-e] & [-e]	moderately advanced unstable	High phonemic load; most examples of merger are lexically specific though there appears to be some lexically non-specific free variation.
3	Merger of primary Shanghai syllabic isolation tones /ʔ/ [ʔ] & /ʔ/ [ʔ]	in flux unstable	Phonemic load difficult to assess.
4	Merger of labio-dental and guttural fricative initials [f-] & [h-] and [v-] & [ʃ-]; appearance of bilabial [p-] and [b-] before /uw/.	well advanced unstable	Appears to be in free variation with no discernible lexical conditioning.
4	Merger of glottalized [ʔ-] and murmured [ʔ-] initial resonants [m, n, ŋ, l, w, y, (zero)] in non-initial position within phonological word	well advanced unstable	Underlying form is often non-retrievable in shape corresponding to historical cognate.
5	Merger of apical (homorganic) vowels [-ɹ] & [-ɹ]	reaching completion unstable	Light phonemic load though still in phonological contrast. Only a few examples of non-merged contrasting relics remain, all lexically specific.
6	Merger of phonemic syllabic isolation tones in non-initial position within phonological word	virtually complete unstable	Phonemic load nil. Underlying form is usually non-retrievable in shape corresponding to historical cognate.
6	Merger of palatal and alveolar fricative initials [ɹ-] & [ʃ-] and [ɹ-] & [ʃ-] before high front vowels	virtually complete stable	Not in phonological contrast in normal speech. Only a handful of non-merged contrasting relics remain, all lexically specific.
6	Merger of syllable finals [-ian] & [-ian]	virtually complete stable	In phonological contrast only in reading pronunciation, not in normal speech. Even in reading pronunciation only a handful of non-merged contrasting relics remain, all lexically specific.
7	Merger of voiced affricate & sibilant initials [dz-] & [z-]	complete stable	
7	Merger of syllable finals [-io?] & [-io?]	complete stable	
7	Merger of syllable final [-n] & [-n]	complete stable	
7	Merger of syllable final [-m], [-n], & [-n]	complete stable	
7	Merger of syllable final [-p], [-t], & [-k]	complete stable	
7	Merger of historical upper rising (阴上) tone and upper going (阴去) tone	complete stable	
7	Merger of historical lower rising (阳上) tone, lower rising (阳上) tone, and lower going (阳去) tone	complete stable	

and compete in the same linguistic community with both earlier and later strata; and yet these strata are not homogeneous throughout nor are they without gaps across the lexicon of a single idiolect. Thus one can observe points separated by only brief spans of linguistic time along the curve of phonological change in Shanghai, even though the mathematics of the curve itself may as yet be elusive.

Ideally a thorough comparative treatment would involve the study of a number of contrasting idiolects representative of the various states of perceptible change in modern colloquial Shanghai which would provide data of use to scholars working on the broader issues of linguistic change.²⁾ I made one such study of the speech of a single Shanghai speaker who is representative of a conservative strain of the dialect in *A Linguistic Survey of the Shanghai Dialect* (Sherard 1982b). The material in the preceding table, which is based on the informant's speech recorded in this survey, indicates some of the main directions of sound change evident in this single idiolect at points where such change has been traditionally described for various versions of Shanghai or the other major Chinese dialects. Twenty-three points of phonological change, either attested or shown to be historically probable for Chinese, are shown here; these I have tentatively organized into eight groups, numbered 0-7, thus roughly displaying the information in the table in the order of the degree of progression of change actually observed in the speech of this informant.

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2) Hu 1978 provides an excellent overview of the general state of change observed in Shanghai.

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A Study on the Synthesis and Perception of /r/ and /l/

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0. Introduction

The study on the perception of /r/ and /l/ has been of considerable interest among researchers since /r/-/l/ contrast has often been a choice to study the effect of linguistic experience, and /r/ and /l/ have unique articulatory and acoustic characteristics which may be defined as an intermediate between stop consonant and vowel. There have been a number of reports on the experiments in the perception of /r/ and /l/ using natural and synthetic speech sounds. Especially, the cross-linguistic studies reveal that the speakers of different languages, e.g., Japanese and English speakers, have different degree of performance in the perception of /r/ and /l/, since /r-l/ contrast is phonemic in English, but not in Japanese. Miyawaki et al. (1975) studied the effect of linguistic experience of American English and Japanese, and mentioned that the difference of linguistic experience is specific to perception of speech mode. Furthermore, Price (1981) and Mochizuki (1981) examined the cross-linguistic effect on the perception of /r/ and /l/ using natural and synthetic speech, and significant remarks have been made on the perception of these sounds. However, the studies on the contextual effects on the perception of /r/ and /l/ are still limited, and the relation between acoustic characteristics and the perception of /r/ and /l/ has not yet been fully examined. Under such circumstances, the present study aims at examining how contextual differences affect the perception, what acoustic parameters are responsible for /r-l/ contrast, and how Japanese and American English speakers perceive synthetic continuum of /ra-la/.

1. Experiments on the perception of /r/ and /l/ in natural speech

The experiments using natural speech consist of (1) identification test of /r-l/ contrast in 50 minimal pairs (Type 1), (2) identification test of /r-l/ contrast in 40 words embedded in a carrier sentence "I said the word ____" (Type 2), and (3) identification test of /r-l/ contrast in 40 words embedded in a carrier sentence "____ is my favorite word." (Type 3). One male native speaker of American English recorded the materials in a satisfactory condition. 32 Japanese college students took part in the experiments of /r-l/ contrast. The results can be shown in Tables 1-3.

Table 1 shows the overall performance for three types of listening materials.

Table 1

Type of Listening Materials	Percentage Correctly Identified
Type 1	85.0%
Type 2	72.6
Type 3	70.7

From Table 1, it can be said that /r-l/ contrast in minimal pairs are more correctly identified than those contained in the carrier sentences. There is no significant difference in the performance whether a key-word was contained in sentence-initial position or sentence-final position.

Next, Table 2 shows the effect of position within a word for the perception of /r-l/ contrast.

Table 2

Position within a Word	Percentage Correctly Identified
Word-final	92.2%
Word-initial	88.4
Intervocalic	77.9
Consonant cluster	68.0

Table 2 indicates that /r-l/ contrast in the word-final and word-initial positions are better perceived than in intervocalic position and in consonant cluster. These results agree with Gillette (1980) and partly with Mochizuki (1981). The weakened performance in the intervocalic position and in consonant cluster may be attributed to the coarticulatory effects of neighboring segments.

It is known that the performance in identification tests is influenced by neighboring phonetic circumstances. Table 3 indicates the effect of vowel environments.

Table 3

	Type 1	Type 2	Type 3
Word-initial # $\begin{bmatrix} -\text{back} \\ -\text{low} \end{bmatrix}$	94.8%	92.6%	78.1%
# $\begin{bmatrix} +\text{back} \\ +\text{round} \\ -\text{low} \end{bmatrix}$	87.0	82.8	67.7
Word-final $\begin{bmatrix} -\text{back} \\ -\text{low} \end{bmatrix}$ #	94.5%	84.4%	95.4%
$\begin{bmatrix} +\text{back} \\ +\text{round} \\ -\text{low} \end{bmatrix}$ #	89.1	61.0	92.2

From Table 3, it can be said that /r-l/ contrast is better perceived in the environments of non-low, front vowels than those of back rounded vowels. That

is, the environments of front vowels facilitate the perception of /r-l/ contrast, while the ones of back rounded vowels may inhibit it. It can be considered that the anticipatory rounding caused by roundness of back vowels may influence the perception of /r-l/ contrast.

Furthermore, spectrographic analysis was made for the pairs with a higher percentage of identification and the ones with a lower percentage to examine the acoustic characteristics on the distinction between /r/ and /l/. A substantial difference is found in the formant patterns of F2 and F3; in word-initial position, /r/ shows a rising transition of both F2 and F3, while /l/ shows a rising or level transition of F2 and a falling one of F3. These formant patterns seem to be characteristic to each segment, and a clear manifestation of these patterns contributes to a higher percentage in identification.

2 Experiments on the perception of /r/ and /l/ in synthetic speech

In order to examine how Japanese and English speakers perceive the synthetic /r-l/ continuum, a series of experiments was conducted. The stimulus materials were prepared at Haskins Laboratories on the OVE III synthesizer. The 10-step /ra-la/ stimuli differed in the frequency values of F2 and F3 within the initial steady-state portions and the transition portions. F2 frequency value varied in equal step from 951 to 1404 Hz, and the one of F3 varied from 1488 to 3246 Hz. The frequency value of F1 was fixed for 10 stimuli. Two types of test were prepared; an identification and an oddity discrimination test. Subjects were 7 native speakers of American English and 22 native speakers of Japanese. Fig. 1 and Fig. 2 indicate the results of identification and discrimination test for two groups.

Fig. 1 shows the results of the pooled identification for Japanese and American English subjects. The two subject groups showed a completely different pat-

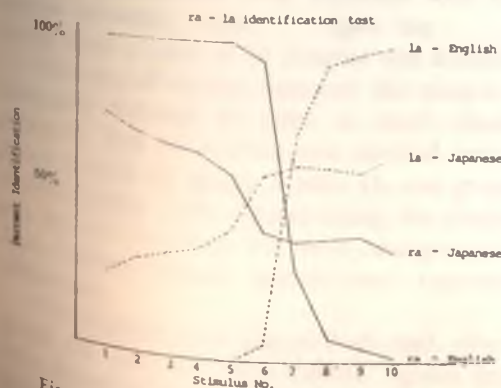


Fig. 1 Pooled identification of Japanese and English-speaking subjects

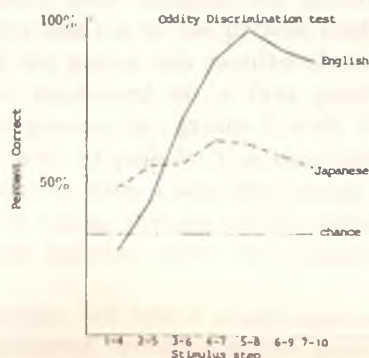


Fig. 2 Pooled discrimination of Japanese and English-speaking subjects

terns in the perception of /r-l/ continuum. Japanese subjects identified stimuli 1, 2, and 3 as /ra/ with 60-75% of identification rate, and the identification curve showed a gradual shift as the stimulus shifts from 3 to 7. On the other hand, American English subjects identified stimuli 1 to 5 as /ra/ and stimuli 9 and 10 as /la/ with an almost 100% of identification rate. The boundary lies between stimuli 6 and 7. The difference in the identification test can be substantiated in the discrimination test. In case of Japanese subjects, the accuracy was in the range from 50 to 65% across the continuum, and there is no remarkable change in accuracy in the stimulus pairs of 1-4 and 4-7. In case of American English subjects, however, the accuracy was below the chance at 1-4 pair, but rose to near perfect accuracy at 5-8 pair. The discrimination peak occurred at the stimuli which corresponded to their identification boundary. From these considerations, it can be said that Japanese subjects perceive the /r-l/ continuum continuously, while American English subjects do the continuum categorically. This difference of perceptual mode is due to the one of the linguistic function of /r/ and /l/ in the two languages.

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An Examination of the Area of Tongue-Palate Contact in Swedish Dental Stop Production

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Tense or fortis stops are opposed to lax or lenis stops by a longer duration of articulatory closure. According to Jakobson and Halle, this "length as a component of the tenseness feature is intrinsically connected with the other, qualitative manifestations of the given feature within the same phoneme."¹⁾ Then, what are the other, qualitative manifestations? Are they related to any physiologic parameter such as the tension of the tongue, for instance? In order to answer the question, we examined the area of contact between the tongue and the palate during the articulation of Swedish dental stops.

Method

The corpus of utterance samples to be discussed here was chosen among those we obtained with a native male speaker of the Stockholm dialect by dynamic palatography. It pertained to short /t, d/ in initial or medial position and their long versions /tt, dd/ in medial position. The consonants were combined with vowels /i/ and /a/ to form 32 disyllabic phoneme sequences such as VtV or dVddV, where V stood for the vowel. The subject, one of the authors, pronounced each sequence with the acute accent, embedding it in a carrier sentence: *säga igen* 'say again'. Each sentence was uttered twice; thus, a total of 64 samples was available for analysis in the present study.

The on/off-contact between the tongue and the palate was monitored continuously through 64 pairs of small electrodes implanted on a thin plastic palate.²⁾ The electrodes were divided into four groups, i.e., group 1 with 19, group 2 with 17, group 3 with 15, and group 4 with 13 pairs.³⁾ The electrodes in each group were aligned along the dental ridge in such a way that group 1 was located closest to the teeth and group 4 in a higher portion of the palate. The contact output signals were tape-recorded together with the subject's utterances.

The signals were played back and, after mixture, fed into a sound spectrograph for analysis. The contact signals from different locations of the palate were shown as horizontal lines in the upper part of the spectrogram paper. In the lower-frequency range of the same spectrogram, the audio signal was displayed by use of the wide-band filter.³⁾

Initial /t, d/

The numbers of on-contact electrodes represented the size of the tongue contact. Thus, we assumed their maximum value found for the dental stop articulation to be a measure of the tenseness feature. The interval from contact formation (first *on*) to contact break (first *off*) was measured, instead of the duration of stop closure, for the electrode immediately to the left side of the front midsagittal point;³⁾ the two measures were found, by inspection, to coincide well with each other. In initial position, /t/ usually showed a wider contact on the palate than /d/ (maximum number: mean of 35 (with a range of 30-39) for /t/; 31 (27-36) for /d/), and stayed closed over a slightly longer period (about 120 ms for /t/; 110 ms for /d/).

Medial /t, d/

The most apparent articulatory difference between the medial /t/ and /d/ lay in the duration of stop closure. It was always longer in /t/ than in /d/ in the same phonetic context. When the dental in question was preceded by the low vowel /a/, the closure phase lasted for about 190 ms for /t/ and 100 ms for /d/; it was about 280 ms long for /t/ and 200 ms for /d/ when the high vowel /i/ preceded the dental.

The medial /t/ after /a/ accompanied a larger area of tongue contact than the medial /d/ in the same context (maximum number: 36 (32-43) for /t/; 30 (25-36) for /d/). But in the phonetic context where the dental was preceded by /i/, there was no significant difference in contact area between /t/ and /d/ (maximum number: 54 (49-59) for /t/; 53 (50-57) for /d/). The absence of difference in contact area may be ascribed to some physical constraint such as the distance between the tongue body and the palate.

Medial Long /tt, dd/

The long dentals were not always characterized by larger areas of tongue contact than their short counterpart. On the contrary, a dramatic decrease in contact area was observed whenever the vowel /i/ occurred as the preceding sound. For example, the maximum number of on-contact electrodes was reduced from a range between 50 and 57 for /d/ to a range between 24 and 39 for /dd/. The closure duration, however, remained rather unchanged throughout both long and short lenis dentals (about 200 ms). A similar shift of contact patterns was seen for the fortis pairs /t/ and /tt/, too.

By contrast, the long dentals following the vowel /a/ were characterized by longer closure durations, i.e., about 260 ms vs. 190 ms for /tt/ vs. /t/ and 200 ms vs. 100 ms for /dd/ vs. /d/. And the lengthening simultaneously showed a trend to enlarge contact areas, as was given by the maximum numbers 40 (34-49) vs. 36 (32-43) for /tt/ vs. /t/ and 33 (29-38) vs. 30 (25-36) for /dd/ vs. /d/.

Was the tenseness, then, preserved in the articulation of the long dentals in spite of the peculiar tongue gesture in the /i_/_/ context? Comparison in the

same phonetic context showed that the answer was affirmative. The fortis were always distinct in terms of both longer closure durations (about 260 ms for /tt/; 200 ms for /dd/) and more on-contact electrodes of the maximum patterns (maximum number: 41 (34-49) for /tt/; 33 (24-39) for /dd/).

The results obtained with the long dentals show that the duration of closure and the size of contact are controllable independently in the process of the dental stop articulation. Nevertheless, these two variables are always correlated as to the fortis/lenis distinction. A similar observation concerning the qualitative manifestation of consonantal tenseness has been reported in the case of Japanese /t/ vs. /d/.⁴⁾ Our observation, of course, is quite limited in the number of subjects and vowel environment, but it clearly shows that /t/ is opposed to /d/ also by a larger area of tongue contact.

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A Complex Acoustic-phonetic Description of Word Stress

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The stress of English words, such as the noun and verb forms of "subject", is complexly related to the fundamental frequency contours and ratios of the durations and peak amplitudes associated with the first and the second vowels in the word (Fry, 1955; Lehiste, 1970). However, not all words with the same stress pattern have the same ratios or contour, nor can the contrasting patterns always be distinguished by these physical dimensions. This suggests that a more complex physical dimension may be better correlated with the perceived word stress.

The ratio of the power (duration \times intensity) of each syllable has been observed with speech (Lieberman, 1960) and non-speech stimuli (Stevens, Sandel, and House, 1962), but there are complex effects associated with this dimension also. If listeners used the power of each component to judge stress patterns, they did not use a simple ratio evaluated as greater or less than one (or 0 dB) to make their reports. This suggests either 1) that a different method of evaluating the ratio (including a different critical point) is required, or 2) that a different physical dimension would be more appropriate.

These issues were studied by analyzing productions of words for which it was known that the duration ratios and the peak amplitude ratios would not distinguish the stress patterns. In such words, a phonetically long vowel /ɜ:/ was paired with a phonetically short vowel /ɪ/, so that the vocalic power and duration were influenced by vowel quality, syllable position, stress pattern and speech rate.

The stimuli were four nonsense words formed by inserting the two pairings of the vowels /ɪ/ and /ɜ:/ in the consonantal frame /s__ds__d/ and assigning stress either to the first or the second syllable. An adult native speaker of American English repeated each word several times at a tempo of one token every 750 milliseconds. The duration and sum of the squares of the amplitudes of the vocalic portions of the signals were determined from the waveforms of eight tokens of each word sampled at 10 kHz with the PCM system at Haskins Laboratories. Mean acoustic measurements of each word as a function of stress pattern and stressed vowel are given in Table 1.

The duration ratios for each segmental sequence are not distinct as a function of stress ($p > .3$); rather, the differences reflect the sequence of vowels. This is consistent with Fry's findings.

The total power of the vowels in the word is independent of the stress pattern ($p > .1$) and of the segmental sequence ($p > .7$). However, the total

Table 1. Mean acoustic measurements (N=8)

Stimulus;	Duration Ratio (V1/V2)	Total Power (dB)	Power Ratio (dB)	Intensity Ratio (dB)	Normalized Intensity Ratio* ($\times 10^{*-5}$)
/sɪdsɜːl/	.52	87.9	— .88	2.16	2.66
/sɪl'sɜːl/	.41	87.6	-6.60	-2.81	.09
/sɜːldsɪl/	1.14	87.7	8.10	7.45	.96
/sɜːl'sɪl/	.94	88.4	-1.39	-1.04	.11

* The unit is an inverse power scale, such as 1/watts.

power does reflect the quality of the stressed vowel ($p < .05$). This power is partitioned between the vowels such that the ratio is greater for stress on the first syllable than for stress on the second syllable for a given segmental sequence. However, the stressed vowel is not always the vowel with the greater power. Comparing the power ratio to 0 dB is not useful, as the results of Stevens, et al., had suggested.

The ratios based on the intensity (average power) of the vowels are more useful. They show a significant effect of stress pattern ($p < .05$). Further, the intensity ratios reflect the quality of the stressed vowel when the first syllable is stressed ($p < .001$), but not when the second syllable is stressed ($p > .6$). The stressed vowel always has a greater average power than the unstressed vowel, regardless of syllable position. Position has a greater effect on the relative intensity of a phonetically long stressed vowel than on that of a phonetically short stressed vowel.

The differences in the intensity ratios are associated with the effects of both the stress patterns and the vowels. To eliminate some of the variance associated with the different vowels, a normalized intensity ratio was tested. This is the ratio of the intensities normalized for the total power of both vowels. Since the total power is associated with the quality of the stressed vowel, independent of position, such a normalizing term should make the intensity ratio reflect the stress pattern more clearly. This is the effect that the normalization has on the utterances produced at this tempo. Stress patterns are significantly different ($p < .01$) and, as with the intensity ratios, there is no effect of the stressed vowel quality on the ratio of words stressed on the second syllable. ($p > .5$). (Since the normalized expression also reflects the effects of the vowels, an improved normalizing term is being sought.)

As an alternative normalizing term, the total duration of the vowels in each word was used with the intensity ratio. (This new ratio reflects the rate of change in the average power ratio.) However, this expression is not different from the simple ratio of duration for distinguishing the stress patterns. It is related to the order of the vowels in the word.

Thus, there is a useful acoustic description of stress patterns that is based on how the power in the signal is partitioned among the component vowels and

modified by the stress pattern. The normalized term is not to be evaluated against a particular set point, but against a language-specific critical point for stressed disyllables. The same results are seen with rate-governed utterances with additional vowels at other tempos by other speakers. From that work, it appears that the normalizing term must be modified to accommodate a wide range of vocal effort (including differences in rate and loudness) and the differences in inherent length and intensity of the vowels.

In this work, I have explored answers to four questions with reference to stress: 1) what linguistic units should guide the analyses of the speech signal; 2) what acoustic dimensions should be examined; 3) what expression the measurements should fit into; and 4) how the expression should be evaluated. The results of this acoustic study point to the use of vowels to locate the portions of the signal whose average power is to be assessed, normalized and expressed as a ratio. This ratio is to be evaluated relative to a critical point. In this scheme, words with different segmental composition but the same stress pattern are related by similar ratios relative to the critical point. The contrasting stress patterns of any segmental sequence have ratios on opposite sides of the critical point. The variation in the observed ratios is related to the phonetic quality of the component vowels. Such a view of the acoustic-phonetic description of English word stress gives phonetic reality to the claims in metrical and generative phonological theories of the importance of segmental and relational information in the description of word stress.

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An Electro-Palatographic Study of Japanese Intervocalic /r/

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Introduction

It is generally said that Japanese /r/ in initial position is a sort of weak stop, while intervocalic /r/ is realized as a tap or flap. In addition to this flap-like quality, it has been pointed out that vowel environments exert a great influence on the phonetic quality of this consonant. These observations have been mainly based on subjective introspection. In the field of experimental phonetics, however, there have been a few studies on the articulation of /r/, such as Shibata (1968) and Miyawaki (1972), which employ electro-palatography. These experimental studies are one step toward the investigation of the articulation of /r/, though they only study some /r/s in limited vowel environments. Therefore, we decided to investigate the dynamics of the palato-lingual contact during the articulation of Japanese intervocalic /r/ using electro-palatography (Kiritani et al., 1977).

Method

The linguistic materials used in this study consisted of intervocalic /r/ in all the twenty-five possible vowel environments of Japanese. Meaningless words of the form $/V_1rV_2V_1rV_2/$ ($V_1, V_2 = i, e, a, o, u$) were embedded in a carrier sentence of the form /Sorewa desu/, or "It is".

Two native Japanese speakers of Tokyo dialect served as subjects. Subject 1 was a 26-year-old female; Subject 2 was a 54-year-old male. Neither of them reported any speaking disabilities.

The subjects were told to produce the sentences at a comfortable speaking rate and the key words with a flat accent. They repeated the text ten times. For each subject, twenty tokens of /r/ in each vowel environment were selected and analyzed.

The artificial palates used in this study had sixty-three electrodes. The data was stored in a computer connected to a portable palatograph unit. When the subjects read a text and pushed the control button after each utterance, data of a one-second duration was stored in the computer.

Results

In most articulations of /r/, the contact proceeds postero-anteriorly along the teeth ridge. Figure 1 shows examples of the contact pattern sequences ob-

tained during the articulation of /ere/ and /ara/ for Subject 1. In this figure, the upper part of each pattern corresponds to the anterior portion of the palate, and the lower part to the posterior portion. The small dots show the positions of the sixty-three electrodes, and the blackened dots represent on-contact electrodes. The number beside the upper right-hand corner of each pattern gives the frame number. In the example of /ere/ shown in the upper half of Figure 1, the contact proceeds posterio-anteriorly and reaches the maximum contact pattern in frame 12, which shows the largest number of on-electrodes during the articulation of /r/. This type of contact can be seen in the case of stop consonants.

In addition to the type of contact pattern of /r/ described above, there is another type in which the contact first occurs at the anterior portion of the palate. The lower half of Figure 1 shows an example of the contact pattern obtained during the articulation of /ara/ for the same subject. In this example, the contact occurs at the anterior portion as shown in the pattern of frame 15, and frame 16 shows the appearance of the contact at the lateral edge. This second type of contact pattern was observed especially in an /a/ or /o/ environment. There were fourteen /ara/s of this type out of twenty repetitions and thirteen /aro/s. Also, /are/, /ora/, /oro/ and /aru/ showed more than 25% occurrences of this type of contact pattern.

The utterances of Subject 2 also show both types of contact pattern explained above. In Subject 2's utterances, the frequency of the occurrence of the second type of contact pattern showing a kind of lateral articulation was lower than in those of Subject 1. However, /ara/, /ari/, /ori/, /uri/ and /oro/ showed more than 25% occurrences of this type of contact pattern for Subject 2.

Thus, there are two types of contact pattern in the production of /r/. In both cases, however, the contact first disappears from the central region in the

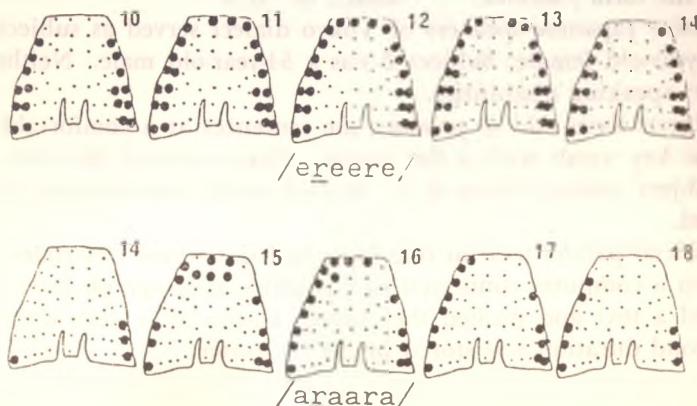


Fig. 1 Examples of contact pattern sequences obtained during the articulation of /ere/ and /are/ (Subject 1).

anterior portion of the palate, and continues to disappear in a backward direction.

Table 1 shows the average number of on-electrodes in the maximum contact patterns of twenty tokens for all the vowel environments. The /r/s which were preceded and/or followed by a high-front vowel /i/ took the widest area, while the environment of the low-back vowel /a/ resulted in a narrowing of the area. Examination of the contact pattern of individual tokens revealed that in some cases the electrodes at the lateral edge showed off-contact, while those in the anterior portion of the palate showed on-contact. The pattern with on-contact electrodes in the anterior portion and an absence of those at the lateral ridge was observed in the second type of contact pattern.

So far we have seen a significant difference in the contact patterns resulting from different vowel environments. Table 2 shows the frequency of the occurrence of complete closure during the articulation of /r/ in all the vowel environments. The presence of complete closure is defined as either when all the

Table 1 The average number of on-electrodes in the maximum contact patterns of 20 tokens for each vowel environment. In the table, V_1 is the preceding vowel and V_2 the following vowel.

(Subj. 1)

(Subj. 2)

V_1	V_2					Total	V_1	V_2					Total
	i	e	a	o	u			i	e	a	o	u	
i	39	35	33	33	37	177	i	35	29	22	21	27	134
e	31	24	23	24	24	126	e	27	20	14	15	20	96
a	29	22	17	19	22	109	a	25	18	16	17	19	95
u	31	27	21	25	28	132	o	25	19	14	17	19	94
o	35	30	25	27	31	148	u	27	21	14	18	20	100
Total	165	138	119	128	142	692	Total	139	107	80	88	105	519

Table 2 The frequency of the occurrence of complete closure for 20 tokens.

(Subj. 1)

(Subj. 2)

V_1	V_2					Total	V_1	V_2					Total
	i	e	a	o	u			i	e	a	o	u	
i	18	17	20	19	16	90	i	15	13	14	8	12	62
e	15	14	16	12	15	72	e	11	8	0	0	9	28
a	19	19	2	6	19	65	a	6	4	6	5	10	31
o	19	20	9	15	20	83	o	5	8	0	3	7	23
u	12	16	12	14	16	70	u	9	10	0	10	14	43
Total	83	86	59	66	86	380	Total	46	43	20	26	52	187

electrodes of any one line along the teeth ridge show contact or when the corresponding electrode of a neighboring line shows contact in spite of the presence of an off-contact electrode in a certain line. As shown in the table, closure is not always attained during the articulation of /r/. Especially in the /a/ and /o/ environments, the frequency of the occurrence of closure is low; while /r/s preceded or followed by /i/ attain closure most frequently.

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On the Chu People, the Chu Dialect and the Chu Phonemic System

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There exist in the history of China and the Chinese history of literature the so-called Chu people, the Chu dialect and the Chu phonemic system of which neither head nor tail has ever been made. However, from our recent investigations into the origin, the pedigree and migration of the Chu people, it has been established that they have long since merged with the many branches of the Han people, once also called the Hua (Xia) people, and become part of them. And, judging from the descriptions of the Chu dialect made in ancient historical records and literature written in Chinese, such as "Semantics and Etymology of the Chinese Language", "Huainanzi" and "On Dialects", it is now known that the Chu dialect had the Chinese structure as its absolutely dominant component part which formed the surface layer. At the same time, part of the Chu vocabulary was either identical with or similar to the languages of the Zhuang and Tong or Miao and Yao nationalities which formed the base of the Chu dialect. It can therefore be asserted that the Chu people were originally an independent tribe from southern China living on the vast expanse of fertile land of the Yangtze and Han River as their base. In the early days, the valleys of the Yellow River, the Luo, the Yangtze and the Han Rivers were the sites where the culture of the Xia and Chu people got merged. The passage through which they communicated with each other was from the Wei River to Xiangyang via the Wu Pass and along the Han River. The Chu people believed in witchcraft which is an indication that it had absorbed the culture of the Shang Dynasty (c. 16th-11th century B.C.). On the other hand, the migration of the Zhou people had also reached the northeast of the Han River. Therefore, it can be said that the Chu people "had benefited from the three dynasties". The so-called Chu people and their dialect have long since blended with the various branches of the Han people and refined Chinese. As for "The Poems and Folk-songs from Chu" described in the history of the Chinese literature, they are nothing but poems and folksongs written in ancient Chinese and the Chu dialect. The Chu language used by the literati and the officialdom of the Eastern Jin Dynasty (317-420 A.D.) and the Southern Dynasties (420-589 A.D.), namely, the Song Dynasty (420-479 A.D.), the Qi Dynasty (479-502 A.D.), the Liang Dynasty (502-557 A.D.) and the Chen Dynasty (557-589 A.D.) was only a dialect different

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from the refined language spoken in the Central Plains (comprising the middle and lower reaches of the Yellow River). With respect to the Chu phonemic system, it was but a system of phonology different from that treated in "Qieyun". It should be pointed out that the so-called Chu dialect mentioned in modern dialects of Hubei Province has nothing in common with the ancient Chu dialect since the inhabitants of Hubei are mostly from Shaanxi, Henan, Shanxi, Anhui, Gansu and Sichuan provinces. Both the people and their language are different from those here discussed and it would be fantastic talk mixing them up.

Aposteriori Consonants in Inner Speech

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1. Introduction

Experimental attempts which try to answer the question whether human speech is perceived in terms of distinctive features, and if so which features in what relation can be demonstrated, have been very numerous, (cf. van den Broecke, 1976). They consist mostly of nonsense stimuli presented under adverse conditions.

The features discussed in most studies may be of two different types:

1. a priori features. The experimenter decides which features (e.g. voice, nasality) he wants to examine. The conclusions obtained will inform him what the relation between the F's is in term of perceptual salience. A typical study of this kind is Miller and Nicely (1955).
2. a posteriori features. The experimenter makes no a priori claims about the number and nature of the features supposedly operative in the process of perception, but performs a multidimensional scaling technique on the confusions obtained, which will yield a representation of the stimuli in an n-dimensional space. Subsequently, an attempt is made at reduction of the number of dimensions involved, and at an interpretation in phonetic terms of the dimensions finally obtained. The features thus obtained are called aposteriori features.

It is evident that the second method, if leading to interpretable results, is superior to the first method since it does not require specific assumption except the assumption on the part of the experimenter that some form of feature analysis may take place in the process of speech perception.

Objections may be raised against the classical way of obtaining confusions which often consist of reducing the signal to noise ratio of the stimulus presented. Predictably, speech sounds of a noisy spectral composition, such as voiceless fricatives will be more affected by a spectrally similar masker such as white noise than e.g. nasals or liquids. If confusions between stimuli could be obtained that did not suffer from this drawback, a more realistic insight into the psychological reality of features, and therefore about their degree of independence could be obtained. The experiment reported here tries to achieve this aim.

The hypothesis can be formulated that similarity judgments on visually presented and internally generated speech sounds (cf. Sokolov's 'inner speech',

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1968), in our case Dutch consonants, are based on a number of perceptual dimensions which is smaller than the number of consonants presented. These dimensions will be called perceptual F's and are expected to have some correspondence with F's arrived at by other means.

The above question has been answered in van den Broecke (1976) using a nonmetric scaling technique. The purpose of the present experiment, in which the same data were subjected to a different, viz. metric scaling technique is, to determine whether this different technique is more suitable than the nonmetric one to reveal structure in the available data.

2. Method

To test the above hypothesis, visual stimuli, needed to generate internal sounds, were composed of pairs of the following 16 Dutch consonants: /p d t d k v s x l r h m n ŋ w/. Unfamiliar symbols (viz. η) were explained to 80 naive Ss who participated in the experiment.

All 128 possible pairs plus 32 repeats were given to the Ss in random order, both as regards the sequence within the pair and the composition of the list. Ss were asked to rate each pair on a 10-point scale in terms of similarity between the members of each pair, as internally generated. This resulted in 12800 judgments, expressed as a number between 1 and 10.

These data were subjected to two types of multidimensional scaling techniques, viz. Minissa (nonmetric, c.f. Roskam and Lingoes, 1970) and SMACOF-IA (metric, c.f. Stoop, Heiser and de Leeuw, 1981).

2.1. Metric vs. nonmetric scaling

Though metric scaling imposes much more severe restrictions, the configurations resulting from metric and nonmetric scaling are often similar. The stress of the latter, informally defined as the degree of mismatch between the actual differences between the objects and their distances in the spatial configuration found in the analysis, will, however, be much smaller.

3. Results

The results are displayed for the first 3 dimensions in Fig. 1 and 2 (metric) and Fig. 3 and 4 (nonmetric). It will be noted that the two solutions in Figs. 1 and 3 have a great deal in common, and are both very well interpretable in terms of phonetic categories: the nasals /m, n, η / form a clear subgroup, as do the liquids /l, r/ in the upper and lower left region respectively. In both figures the fricatives /v, x, w, s, z/ form a group in the centre. The plosives /p, t, k, b, d/ cluster in the top right corner, and the back consonants /h, k, x/ are grouped at the top centre. The labials /b, p, v, w/ are somewhat problematic, especially in the nonmetric configuration, where the /d/ intrudes. In the metric solution, the very close proximity of Dutch /v/ and /w/ seems convincing.

The voiced/voiceless opposition manifests itself in the nonmetric solution as a clear and constant distance along the first dimension. In the metric solution, this opposition cannot be retrieved. Summarizing, a comparison between

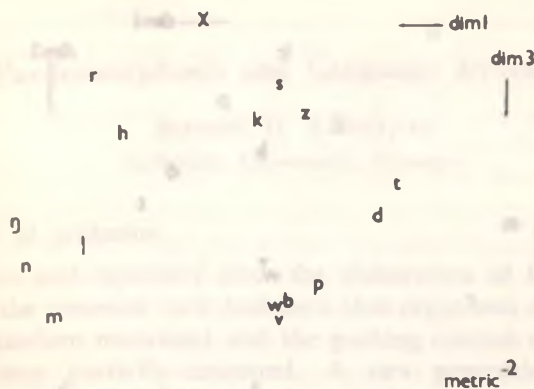


Figure 1. Dimensions 1 and 2, metric solution.

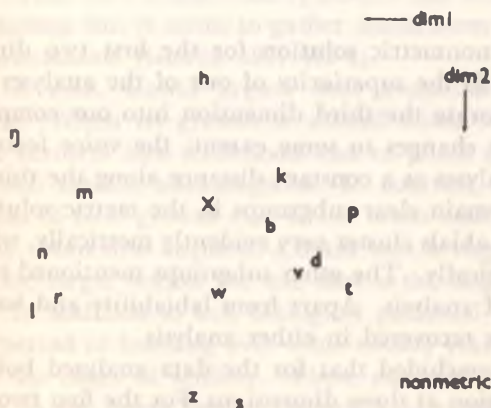


Figure 2. Dimensions 1 and 3, metric solution.

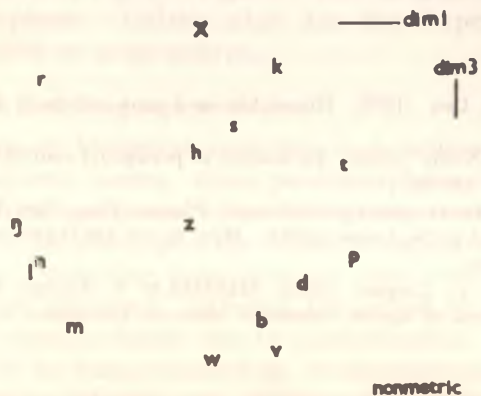


Figure 3. Dimensions 1 and 2, nonmetric solution.



Figure 4. Dimensions 1 and 3, nonmetric solution.

the metric and the nonmetric solution for the first two dimensions is rather inconclusive as regards the superiority of one of the analyses over the other.

When we incorporate the third dimension into our comparison, cf. Figures 2 and 4, the picture changes to some extent: the voice feature is now clearly captured in both analyses as a constant distance along the third dimension. The nasals and liquids remain clear subgroups in the metric solution, but merge in the nonmetric one. Labials cluster very evidently metrically, whereas they cannot be retrieved nonmetrically. The other subgroups mentioned remain distinguishable in both types of analysis. Apart from labiability and backness, no further place features can be recovered in either analysis.

Thus it can be concluded that for the data analyzed both types of scaling find an optimal solution at three dimensions. For the first two dimensions, there is no clear preference for either approach, but when the third dimension is included, the metric solution seems somewhat better interpretable than the nonmetric one.

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Paedomorphosis and Language Evolution

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1. The theory of evolution

Since Darwin and especially since the elaboration of the synthetic theory in the thirties, the common view had been that organisms evolved through the interaction of random mutations and the guiding control of natural selection. This view is now partially contested. A new generation of evolutionary biologists is arguing in favor of the existence of "a tendency within organisms to change in the direction of greater complexity and greater adaptability" (Henig, 1982:17). This view is not entirely new; it has been voiced throughout this century and before, but it seems to gather momentum and genetic evidence is beginning to accumulate (Gould, 1977:405-8).

In the case of man, Gould argues that evolution is the result of a retardation process. When its growth is slowed down, a creature is able to retain its juvenile features into adulthood and to phase out the later-acquired characteristics, which are less advantageous. In concrete terms, this essentially means the retention of fetal growth rate of the brain and the expansion of our period of plasticity. Man thus acquired a brain that is large enough in absolute size, and, among the larger brains, the one that is the largest in relation to the size of the body. In addition to this biological potential for intelligence, man has extended his period of learning over some twenty years and, at a somewhat different rate, to the onset of senility. These essential human characteristics are not, according to the latest theory, the result of random mutations but of a continuous slowing down of our maturation process. Biologically this is achieved by the action of regulatory genes. The retention of juvenile features is called paedomorphosis. Today's adult has the shape—not the size—his ancestor had as a child or as an embryo.

2. Paedomorphosis and language evolution

The new theory of biological evolution is I believe important for the explanation of linguistic events. Since paedomorphosis is the result of the action of our regulatory genes, and since these genes are said to hold back our somatic development, we can expect the action of regulatory genes to affect all our structural genes, including those that are responsible for our ability to speak and to acquire languages. Although the period of possible observation is shorter, human language should also be paedomorphic.

Now, let there be no misunderstanding. Paedomorphosis is not infantilism. Modern man is not an infantile ape. Humans have retained, developed and capitalized on the juvenile features of their ancestors. Hence the size of our

brain, which is our best asset. Likewise, modern languages are not Indo-European babytalk, but it will be argued that these languages have evolved by retaining and expanding the earlier-acquired features and by eliminating those that appeared in a later stage of linguistic development.

2.1. The evolution of Indo-European languages

When we take an overall view of the evolution of Indo-European languages, we realize that, although they all evolved in their own particular ways, and at their own individual rates, the general direction was always the same. Here are a few changes that illustrate that direction.

Phonologically, languages have displayed a general propensity to eliminate complex sounds. The satem languages eliminated the labio-velars, and the centum branch, with the exception of a few Romance languages, has also reached that end result. Laryngeals, and glottalized stops if there were, have completely disappeared, and aspirates are confined to the modern Indo-Aryan languages and Eastern Armenian. The elimination of phonemic vowel length has been gradual, but the trend is unmistakable.

The evolution of morphology is also proceeding in one general direction. The dual has practically disappeared, and declensions are steadily losing ground. Aspect is confined to the Slavic languages. The optative survives in isolated cases, while the subjunctive is being absorbed by the newly-created conditional.

In syntax, there are two overriding evolutionary changes. Within a clause, word order is taking away from flexional markers the task of indicating grammatical function, and in complex sentences subordination has come to replace correlation.

As the exceptions listed above indicate, these changes are not truly universal within the Indo-European languages—nor do they need to be—yet it cannot be denied that these languages have all evolved in the general direction charted by these changes. Geographic expansion does speed up the rate, while closed-in environments stifle it. In periods of political stability and blossoming culture, attempts are made to stop the clock and even turn it back. (In the sixteenth century, French grammarians, for instance, tried to endow their language with a “classical” morphology and prosodists urged the introduction of vowel length.) Conversely, periods of social upheaval unleash the natural forces. These are, nevertheless, modalities, and they should not distract from the essential fact that all Indo-European languages have evolved and are evolving in the same general direction.

2.2. Explaining the evolution of Indo-European languages

The direction taken by Indo-European languages is commonly said to be from a synthetic to an analytical model. Such a statement has no explanatory power; it is just a description, and a partial one at that since it does not apply to the phonological data. Let us see if the evolutionary changes indicated above

can be explained, and not simply given a generic descriptive label.

2.2.1. The existing theories

The changes listed above could be explained by claiming that languages move towards greater simplicity. The thought is appealing, but it remains specious until we can define *simplicity* and discover what triggers the change. Economy and the interplay between man's communication needs and physical inertia provide some explanation for the onset of the process, but they also stumble on the difficulty, not to say the impossibility, of ranking linguistic features on a scale of physical strain. The pursuit of equilibrium seems also intellectually satisfying, but it is difficult to reconcile it with the observation that languages never stop evolving, and even if we accept the existence of a situation like that of Sisyphus, language itself would have to seek its own equilibrium, and that is at best an empty metaphor. Of course we could conclude with Lass (1980) that explanations are not possible in linguistics, and devote ourselves to taxonomy, but I believe an adequate explanation can be found.

2.2.2. Paedomorphosis and the evolution of Indo-European languages

If we consider the chronology of the acquisition of the linguistic features that have by and large disappeared and compare it to that of the features that have survived or were created to replace the outgoing ones, we immediately see that the ones that have been phased out would have been acquired late, and the ones that have been created are derived from features that appear early in child language.

Vowel quality is acquired before vowel length. Palatal affricates appear early; labio-velars are produced very late. Voiced consonants appear earlier than glottalized stops. Fricatives are mastered before aspirates. Demonstratives and their begotten articles become part of child language long before declensions are assimilated. The future and the conditional clearly precede the subjunctive and the optative. I do not know when Russian children make a systematic distinction between perfective and imperfective verbs, but the French imperfect appears in the course of the third year and is mastered at the beginning of the next. We cannot tell of course when a Roman child was able to use constructions such as *UT sementem feceris*, *ITA metes*. Was it later than the acquisition of subordinate clauses? Perhaps. The observation of children who acquire modern languages where correlatives still exist should give us some day a definitive answer. What is sure is that the use of word order to indicate grammatical function precedes the lengthy process of acquiring declensions (For the chronology of the acquisition of these features, see Burling 1973, Chao 1973, van Ginneken 1917, Grégoire 1937-47, Leopold 1947, Rûke-Draviņa 1973, Slobin 1966 and Velten 1943).

While information concerning the acquisition of some Indo-European features that have generally disappeared remains wanting, the data available

at the present time clearly show that languages have evolved through a process whereby the early-acquired features have expanded and gradually replaced those that required more time to master. This process, which is characteristic of human evolution as a whole is the direct result of the retardation affected by our regulatory genes.

2.2.3. The child's role

In the paedomorphic conception of language evolution, the child is not portrayed as an imperfect learner of adult language, but as one who creates as he learns (cf. Jakobson, 1968:14). Confronted with the difficulty of learning *k* and *k^w*, the child will probably first pronounce a *t* instead of the *k*. Later he will gradually try to velarize that *t*, which he knows stands for a *k*. That slightly posterior *t* will come to take the shape of a dental or palatal affricate (Sikorskiy, 1899:155). At that time, or at an ulterior stage he might try to produce a *k^w*. If his efforts yield a plain velar, he will have created in his intermediate language an opposition between a tentative dental or palatal affricate and a somewhat delabialized labio-velar. Later he will acquire the adult opposition, but, with the regulatory genes slowing down the language acquisition process, children of successive generations will have more and more time to consolidate the dental or palatal affricate versus plain velar opposition and less and less time to acquire the set of plain and labialized velars. Eventually the latter set will be excluded from the acquisition process, and the language will have evolved.

Now, it is a known fact since Jakobson (1968), that features are universally acquired at about the same time. Therefore children experience the same difficulties, but the substitutions they make can vary. Confronted with the acquisition of *k* and *k^w* a child may replace *k^w* not by a delabialized velar but by a develarized labial. Such a substitution would eventually lead to the opposition *k* vs *p*. This evolution took place in Osco-Umbrian, while the process described above explains the changes that occurred in French.

The child plays, therefore, an important role. He creates easier, though equally adequate, alternatives for features that are more difficult to acquire. (The difficulty parameter is provided by the chronology of acquisition.) But because children have a certain margin for choosing their alternatives, and because they give themselves the liberty of tackling difficulties in the order they wish, the late-acquired linguistic features evolve in a different way and at a different rate. This explains why all Indo-European languages are not alike and at the same time why they evolve in the same general direction.

3. A remaining question

The evolution of language is therefore a genetically controlled process. Paedomorphosis will occur as long as our regulatory genes exert a retardation action on the activity of our structural genes, which are responsible for our somatic development and our acquisition of language. For language this will

mean a continuing process of expanding earlier-acquired features and phasing out the late comers. The evidence shows that this process has been at work in the Indo-European languages. It remains to be seen whether a parallel exists in the other families of human languages. If such a parallel is found, I believe the theory of paedomorphosis will provide the historical linguist with an efficient research paradigm because it can explain.

- 1) why languages evolve
- 2) why languages evolve the way they do
- 3) how evolutionary changes are initiated.

Furthermore, this theory integrates the evolution of language with the evolution of Man.

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The Tocharian Accent

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From the point of view of accent Tocharian (both A and B) has a strong intensive accent on the penult. But there are some remainders of the Indo-European quantity.

The results of a stress accent are everywhere the same: the stressed syllable (or rather vowel) becomes long and is articulated with a particular energy while the other vowels (both pretonic and post-tonic) weaken and frequently disappear. English is a very good example of this.

I shall give some examples of the accent on the penult. The Sanskrit words which have entered into Tocharian frequently lengthen the penult, which proves that the Tocharian accent fell on that syllable (in Sanskrit the syllable was short):

Sanskrit <i>Gangā</i> :	Toch. A B <i>Gāṅg</i>
Sanskrit <i>artha</i> :	Toch. B <i>ārth</i>
Sanskrit <i>Kundala</i> :	Toch. B <i>kontāl</i>
Sanskrit <i>śraddha</i> :	Toch. A <i>śrāddhe</i>
Sanskrit <i>cakra</i> :	Toch. A <i>čäkrä</i> , B <i>čäkkär</i>

As can be seen, the final vowel usually falls, because of the intensive stress on the penult.

Sometimes the lengthening of the penult can be discovered only by comparing the word with other Indo-European languages. While we have (as it is natural) the Indo-European length preserved in words like A *māčar* 'mother', B *māčar*, we have A *pāčar* 'father', B *pāčar*, A *čkāčar* 'daughter', B *čkāčar* where all other Indo-European languages have a short penult: Greek *pater*, Lat. *pāter*, English *father*, Indo-Aryan *pitā* (with *a); Gr. *θυγάτηρ*, Indo-Aryan *duhitā* (in the other languages the short *a falls: English *daughter*). A good example of the fall of both the pretonic and the post-tonic vowels is

Toch. A *špāl*: Gr. *κεφαλῆ*

For the protonic syllable cf. e.g. *śatwārah*

In some cases (both in the penult and in the other syllables) the Tocharian long vowel is an I.E. heritage: so e.g. the first sing. A-*mār* which corresponds (with the addition of an -r at the end) to Greek *ἐμενόμεν* (where an -v is added, as in other cases); Toch. A B *mā*: Gr. *μή*; B *kā*=Lt. *quā*. In cases like B *weñäre*=Lat. *uenēre* we do not know whether the long *ā* is an I.E. heritage or a Tocharian innovation. The same is true for the verbal suffix -*nā* (B *kārśnāmar* etc.) which may well correspond to Gr. *δάμνασι*.

Morphological *n*-Epenthesis in Sanskrit

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0. When a phonological change or sequence of changes gives rise to a situation in which zero in semantically basic or unmarked morphological categories alternates with a non-null element X in non-basic categories, the typical outcome is a morphologically conditioned epenthesis rule. Such an alternation between zero and X will be handled by an epenthesis rule rather than a deletion rule because of the established tendency for speakers to take as morphophonemically basic the allomorph of an alternating morpheme that occurs in the most basic or unmarked combination of morphological categories (Kurylowicz 1968, Vennemann 1972). The epenthesis rule will in general be morphologically conditioned simply because in general there will be no purely phonological characterization of that set of zeros that alternate with X.

This paper will discuss three morphological *n*-epenthesis rules of the Sanskrit declensional system. The historical origins of the rules will be considered first, but more attention will be devoted to what they suggest concerning the relative advantages of a proportion-based and a rule-based approach to morphological change. On the one hand, a rule-based account will clarify a situation that has resisted explanation in terms of a purely proportional approach. On the other hand, it will be argued that morphological rules must be allowed to be conditioned not only by properties of stem forms but by properties of surface inflected forms—in the case considered here, by properties of the semantically unmarked nominative singular. This surface conditioning has the effect of incorporating into rule-based accounts of morphological change the notion 'point of contact between paradigms' that is central to traditional proportional accounts of morphological change. It is claimed, then, that a rapprochement between the two approaches is indicated.

1. Before vowel-initial endings, Sanskrit neuter *i*- and *u*-stem nouns display an intrusive *n* that is present only rarely in the earliest texts, but comes to be the rule in the classical period. The source of the rule that inserts this *n* can be found in the paradigm of the subclass of neuter *n*-stems that, because of the operation of Sievers' Law (see, e.g., Debrunner and Wackernagel 1930:268), displays an apparent full grade of the *n*-suffix throughout the paradigm. Below are the singular paradigms of *brāhma(n)*- 'prayer' and *mādhv-* 'honey', with both earlier forms (without *n*) and later forms (with *n*) shown for the latter:

N.A. *brāhma*

1. *brāhmanā*

mādhvā

mādhv

mādhunā

D.	brāhmaṇe	mādhve	mādhune
Ab.G.	brāhmaṇas	mādhvas	mādhunas
L.	brāhmaṇi	mādho/mādhavi/mādhāu	mādhuni
V.	brāhmaṇ/brāhma		mādhu/mādho

In the paradigm of *brāhma(n)-*, phonologically conditioned variation in the realization of syllabic *n* led to a situation in which zero in the semantically basic nominative singular alternated with *n* in oblique cases. Subsequently, the semantically basic form was taken as morphophonemically basic, and the *n* of the oblique cases, formerly stem-final, was reinterpreted as epenthetic. At the point in time when the epenthesis rule applied only in (former) *n*-stem paradigms like that of *brāhma(n)-*, it could be written as (1) below:

$$(1) \quad \phi \rightarrow n / \left[\begin{array}{l} +\text{neut} \\ +\text{syll} \\ +\text{low} \end{array} \right] \text{---} + [\text{VX}] \text{ Case}$$

Generalization of rule (1) takes the form of loss of the feature specification [+low]. After generalization, *n* is inserted not only at the end of neuter stems ending in *a*, but also at the end of stems ending in *i*, *u*, and *r*. Rule (1) as written has a defect, however. It will incorrectly insert *n* into certain forms of neuter thematic stems. The dative singular of *āsyām* 'mouth', for instance, is *āsyāya*, presumably from /āsyā+ya/, and the locative singular is *āsyē*, clearly from /āsyā+i/. We will not attempt to remedy this defect in rule (1) at this time, but will return to it below in section 2.

The two remaining *n*-epenthesis rules we will discuss are rules (2) and (3) below:

$$(2) \quad \phi \rightarrow n / \text{---} (s) + [i] \text{ Case}$$

$$(3) \quad \phi \rightarrow n / \text{---} (C) + [X] \text{ Strong Case (in participles and comparatives)}$$

Rule (2) supplies the *n* that appears from the Vedic period on in the nominative-accusative plural of all varieties of neuters except for those with stop-final stems—specifically, in thematic neuters and neuter *s*-stems as well as in *n*-stems and *i*- and *u*-stems. Like rule (1), rule (2) originates in the *n*-stem paradigm; we will not discuss here the reason for its divergence from rule (1). Rule (3), on the other hand, supplies the *n* that appears in all strong cases of the paradigm of the perfect participle in *-vas-* and of comparatives in *-yas-*; this rule has its origin in the paradigm of adjectival and specifically participial formations in *-nt-*, where the alternation of zero in the weak stem with *n* in the strong stem is the phonologically regular reflex of the older alternation of *en*. Finally, we must mention a lengthening rule that applies in strong cases before stem-final *n* or *n* followed by stem-final *s*; in particular, this rule applies to all forms that receive *n* by rules (2) and (3):

$$(4) \quad V \rightarrow [+ \text{long}] / \text{---} n(s) + [X] \text{ Strong Case}$$

2. Having introduced the three *n*-epenthesis rules (1)-(3) and the lengthening rule (4) and briefly considered the historical origins of the *n*-epenthesis rules, let us now turn to the implications of these rules concerning the relative advantages of proportion-based and rule-based accounts of morphological change. The first point to be made concerns the perfect participle in *-vas-* and the comparatives in *-yas-* and the fact that accounting for the nasal that appears in their paradigm by rule (3) and for the long vowel that accompanies that nasal by rule (4) offers a way out of a problem that has worried earlier writers on the subject. Trying to find an analogical model that would account for both the nasal and the long vowel of these paradigms at once, Debrunner and Wackernagel (1930:294), for instance, were led reluctantly to conclude that the only possible candidate was the adjective *mahat-* ~ *mahānt-* 'great', whose strong stem shows both the nasal and the vowel length: the proportion they set up was *mahat-* : *mahānt-* :: *-yas-* : *-yāns-*. This situation constitutes a clear case in which a rule-based model of morphological change is necessary for an adequate understanding of the facts and in which exclusive reliance on a proportional model will preclude that understanding.

Proponents of the proportion as a descriptive device, however, have emphasized its value in calling attention to the pre-existing similarity or 'point of contact' between one paradigm and another that often provides at least a partial explanation for why a rule appropriate to the first paradigm has been generalized to the second (see, e.g., Jeffers 1975). Let us consider this question of the motivation for generalization of a morphological rule with particular attention to rule (1) above.

Rule (1), it will be remembered, provides the *n*'s of the paradigms of *brāhma* and *mādhū*. A proportion-based description of the extension of epenthetic *n* from the first paradigm to the second would write, for instance, *brāhma* : *brāhmanā* :: *mādhū* : *mādhunā* and would point explicitly or implicitly to the coincidence of zero ending in the nominative-accusative singular as a point of contact between the two paradigms and thus as a factor encouraging the generalization of rule (1) from one to the other (see, e.g., Debrunner and Wackernagel 1930:133). In fact, it would seem that without consideration of this point of contact, it is impossible to understand why rule (1) was generalized to neuter *i-* and *u-*stems but not to neuter thematic stems. Why should the paradigm of *mādhū-* have been remodeled after the paradigm of *brāhma-* while the paradigm of *āsyā-* remained unaffected unless it was precisely because the nominative singular of *āsyā-*, i.e. *āsyām*, has a non-zero desinence? Remembering now that the version of rule (1) given above was defective precisely in applying incorrectly to thematic neuters, we must conclude that the only way to revise it is by allowing it to be conditioned by the form of the nominative-accusative singular rather than by the stem form. The original version cannot distinguish between *brāhma* and *āsyām*, since it makes reference only to the stem, and the stem in both cases ends in *a*. Instead of rule (1), then, we need rule (1').

(1') $\phi \rightarrow n / ___ + [VX]_{\text{Case}}$

Condition: unmarked case-form has the shape $\# Y \begin{bmatrix} +\text{syll} \\ -\text{long} \\ +\text{low} \end{bmatrix} \#$

A final observation that must be made with respect to rule (1') is that the spread of this rule is a classic illustration of the dictum that a morphological change is typically the outcome of multiple pressures rather than the effect of a single cause. Rule (2), which is older than rule (1'), overlaps in effect with the latter, and was plausibly a factor in encouraging its spread (see Debrunner and Wackernagel 1930:132; the same may be said of the relationship of rule (2) to rule (3)). The pre-existing presence of epenthetic *n* in the genitive plural of *i*- and *u*-stems, which dates from Indo-Iranian times, is another factor that may have encouraged the spread of (1'). A third factor of this sort concerns the instrumental singular. Apart from the genitive plural, in which epenthetic *n* appears in all three genders, the instrumental singular of *i*-stems and *u*-stems is unique in displaying epenthetic *n* in masculine as well as in neuter forms. Further, these *n*-forms appear early, comprising about half of the relevant instrumental singulars in the Rig-Veda (Debrunner and Wackernagel 1930:147), and do not favor neuters over masculines. Influence from the thematic instrumental singular *-ena*, even better established in the Rig-Veda (eight out of every nine cases, according to Whitney (1889:112)), seems unmistakable, perhaps in conjunction with influence from the pronominal declension that is generally acknowledged to be the source of that thematic ending. Rule (1') is the natural outcome of a situation in which zero in the semantically basic nominative-accusative singular alternates with *n* in oblique cases with vowel-initial endings. The generalization of rule (1'), however, must be seen as the outcome of multiple pressures.

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Towards a History of the Sulawesi Languages

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The language groupings of Sulawesi can be stated as follows:

1. The Bugic Subfamily in the southwest peninsula with these members: Makassarese, Buginese, Mandar, and Sa'dan.

2. The Bareic grouping, probably a subfamily, which includes a large number of speech types immediately related to Bare'e, to be found in the central bulge of Sulawesi, in its central eastern peninsula, in its southeastern peninsula and the islands of Muna and Buton, and in the Togian Islands in Tomini Bay.

3. The Loinangic grouping consisting of Bobongko in the Togian Islands; Loinang and Balantak in the eastern peninsula, and Banggai in the Banggai Islands.

4. The Tominic languages or speech types in the western portion of the northern peninsula.

5. The so-called Philippinic languages of Sulawesi in the eastern portion of the northern peninsula. These consist of the following groupings: Gorontaloese, Bolaang-Mongondow, Minahasic, and Sangiric.

Mills (1975) has given the evidence for the Bugic subfamily under the name of South Sulawesi. Sneddon (1970) has given the evidence for Minahasic. J. Noorduyn (1981) proposed that Gorontaloese and Bolaang-Mongondow constitute a discrete subgroup which I call Gorontaloic. The available evidence supports treating the Bareic members as constituting a separate subgroup. The position of the Tominic languages must be left indeterminate for lack of evidence. The Loinangic languages seem to go more closely together with each other and with Bareic, but the interrelationships can not be further specified as yet.

In this paper I will address two problems. The first is the relationship between the Philippinic languages and the Philippine languages. The second is the relationship between all of the languages of Sulawesi with the languages of the Philippines and those of western Indonesia. There is reason to propose a hypothesis that the Sulawesi languages belong together at the same level as the one that unites the West Indonesian languages with the Philippine languages. This hypothesis implies that there was a proto-language—here called Proto-Hesperonesian—which gave rise not only to all of the languages of the island, but also to some extra-Sulawesi language groups. Such a classification would make it difficult to avoid concluding that Sulawesi had been the center of distribution for the West Indonesian and Philippine languages.

Lexicostatistically there appears to be a chain from the Philippine lan-

guages to the West Indonesian languages that goes through Sulawesi. The links in this chain are as follows:

Gorontalic—Philippine	about 30%
Philippine—Sangir	about 30%
Sangir—Tontemboan	27.7%
Tontemboan—Bare'e	26.0%
Bare'e—Bugic	about 30%
Bugic—Malay	about 30%

The weakest links appear to be between Tontemboan and Sangir and between Tontemboan and Bare'e, though the weakness appears to be minor. Furthermore the weakness of the Bare'e link to the north may ultimately be dissipated when the Loinangic languages are introduced into comparison. On the other hand it is worth noting that Bare'e appears to form part of the northern boundary of a number of traits which it shares with Bugic and/or West Indonesian languages.

These traits are (1) the use of immediately proclitic pronouns with the verb, not found (with one minor exception) in North Sulawesi or Philippine languages, though it is found in East Indonesian languages and elsewhere; (2) the *-ka* and *-aka* suffixes which seem to correspond to Malay *-kan*, not found in Bugic, the North Sulawesi languages, or the Philippine languages; (3) the merger of the third person singular enclitic pronoun with the plural, a change which can hardly be dissociated from the rise of the so-called *di*-passive of Malay and Javanese, the *ri*-passive of Buginese, the *i*-passive of Ngaju, and perhaps also the *ni*-passive of Makassarese.

Loinang, Balantak, and Banggai of the Loinangic languages exhibit cognates of Malay *-kan*, and Loinang and Banggai have proclitic pronouns. These features suggest a closer relationship with Bare'e and Bugic rather than with any North Sulawesi language, thus agreeing with generally held hypotheses.

Furthermore the cognate set for 'nine' presents an obvious tie between Philippine languages and the North Sulawesi languages. Certain Manobo speech types exhibit a metathesis of PAN *siwa* (to **siaw*: Ilianen, Western Bukidnon *siyaw*, Kalamansig Cotabato *siyow* (Reid 110). This metathesis also appears in Tontemboan, Bolaang-mongondow *siow* and Sangil *siaw*.

There are thus two family-tree diagrams for Hesperonesian that have to be considered. One reflects the chain and the other divides the languages into two branches: (1) Gorontalic, Philippine, Sangiric, Minahasic and (2) Bareic, Bugic, West Indonesian. In either classification the Loinangic and Tominic languages remain to be fitted in when sufficient evidence becomes available.

Evidence for the chain hypothesis can be found in the cognate sets that are exclusively distributed over languages that would belong to different branches in the two-branch hypothesis. Many such cognate sets can be presented, but I will present here, because of limitations of space, samples of only two types.

both characterized by lacking a Philippine cognate as far as I know. The first sample has 17 cognate pairs from about 55 with a Tontemboan cognate and a Bareic cognate, the Tontemboan cognate being cited first and followed by the Bareic cognate:

wangkir, Bar(e'e) bangke 'big'; wowoq, Bar bobo 'mute'; wurotok 'flea', Bar wuroto, buroto 'k. of mosquito'; əqmba, Bar omba 'burn'; ensong, Bar encu 'push away'; inta 'let's go', Bar inta 'come on!'; langkaq, Bar mo-langka 'drink from a tilted vessel'; lener 'calm, flat (of sea)', Bar lene 'flat, level'; noqo, Bar noo 'dirt from cleaning (house, land)'; ngongo, Bar -ngongo 'pull lips crooked'; palən 'hinder, block', Bar palo 'stopper, plug, door'; rangkang, Bar rangka 'dry branches'; rica 'to nauseate', Bar rika 'nauseated'; sawəl, Bar sawa 'substitute'; tələs 'to buy', Bar tolo 'to exchange'; tuqul 'to name', Bar toqo 'name'; rangan, Bar janga 'span'.

Similarly I list 7 pairs from about 30 cognate sets with a Tontemboan cognate (cited first) and a West Indonesian cognate (immediately following). In this group care was taken to avoid pairs that might be suspected of resulting from a borrowing from Malay:

kambil 'grasp, seize', Mal(ay) ambil 'take'; woqso, Mal bocor 'leaky'; kuniq 'yellow', Mal kuñit 'turmeric'; rere, Mal lidi 'frond midrib'; sangir, Old Javanese sangir 'to whet'; sowor 'grow well', Mal subur 'rapid growth, fertile'; waya 'only, all', Javanese Wae 'only'.

Similar collections, varying in size, can be made for Sangirese and Gorontalic. Because of the restrictions in space I can not present the evidence here, but will do so in a later publication.

If the numbers in such cognate lists are significantly larger than is to be expected under the two-branch hypothesis which unites Tontemboan with the Philippine languages, then those numbers militate against the two-branch hypothesis. Unfortunately there is no way of determining the size of a critical number, so that the decision can only be made impressionistically.

This method was first applied by Meillet (1922; cf. ch. 1). It is not without risks, for the addition of a new cognate to a set can nullify the value of the set. The method is best used in a well-worked field like Indo-European; its use in Austronesian where not all of the languages are well-studied entails a greater risk that the collection will suffer significant attrition. It should not however be overlooked that additional conforming sets may yet appear.

Furthermore when the method is used in conjunction with lexicostatistics, there is the possible disadvantage—hardly noticeable here—that the same evidence may be being used twice for the same purpose. Despite the drawbacks involved I regard the evidence of these cognate sets as (provisionally) tipping the balance in favor of the chain hypothesis.

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Towards a Diachronic Theory of Ijo Gender

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One of the interesting features of Ijo, a Niger-Congo language cluster, is its possession of a gender system that involves both animateness and sex. The system, which shows considerable dialectal variation, distinguishes, in its least developed form, the categories human/non-human, on the one hand, and feminine/non-feminine, on the other; and in its most developed form the categories human/non-human (or animate/inanimate, with animate distinguishing human and non-human) and feminine/masculine/neuter. Animate nouns are nouns which refer to human beings, animals and supernatural beings; inanimate nouns include all other entities. Feminine nouns are nouns which refer to female human beings; masculine nouns are those which refer to male human beings and all animals (including the female); neuter includes nouns which refer to inanimate entities or human beings whose sex is unknown or ignored. In some varieties of Ijo, the system involves a 'dual-dichotomy' system of classification (i.e., it starts off with two primary dichotomies) such that certain entities that are distinguished in one part of the system are grouped together in the other. The gender distinctions manifest themselves in pronominal reference, (definite) article agreement, and demonstrative agreement.

These facts about Ijo gender are often inaccurately reported in the current linguistic literature. For example, Gregersen (Language in Africa: An Introductory Survey, 1977:52) speaks of Ijo as a language with only three gender classes: masculine-feminine-inanimate; and Greenberg ('How Does a Language Acquire Gender Markers?', 1978:77) reports that Brass (=Nembe) Ijo has 'articles which distinguish masculine, feminine and neuter in the singular and with the same distinction in pronominal reference, but without agreement in the noun phrase'. Thus, the paper aims, first, to present an adequate synchronic description of gender in Ijo. Beyond this, and with greater focus, the paper investigates the historical evolution of the phenomenon in the language cluster, and makes, inter alia, the following points.

1. The gender system is essentially a natural one (with a high degree of correlation between the actual linguistic classification of the nouns of the language and a classification based on semantic properties), which in certain respects has evolved into a grammatical system.
2. There is a general tendency towards masculinization in the system.
3. Ijo gender occurs in its most conservative and elaborate form in the Nembe dialect, although part of the complexity of the Nembe system is the result of innovation. A graphic representation of the system (comprising five

gender classes) is given in Fig. 1, and the exponents in the following table.

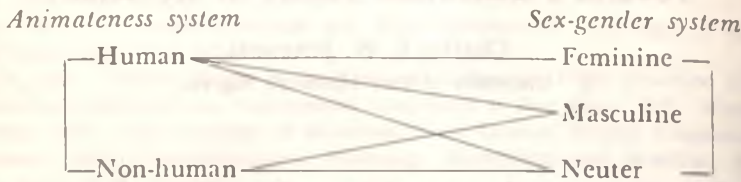


Fig. 1 Gender classification in Nembe

(In both Figs. 1 and 2 the linking lines show the only possible combinations of features between the two systems.)

Exponents of the Nembe gender system

SINGULAR							PLURAL							
	Pronous: root		Independent	Demonstratives: Distal		Proximal	Article	Semi-pronoun	Pronous: root	Independent	Demonstratives: Distal	Proximal	Article	Semi-pronoun
Human feminine	a		ará		má	-ma	ara, bó							
masculine	ó		orí		ɓeí	-bei	owej bó		ein		ení	mcin	-mein	ongu
neuter	ibó			anj	mí	-mí	bó							
Non-human masculine	ó		ori		ɓeí	-bei	wei		á		ará	mamá	-ma	yaj
neuter	ain			anj	mí	-mí	ye					má		

It is to be noted that in Ijò the demonstratives are diachronically related to the articles and personal pronouns.

4. There are other Ijò, dialects, besides Nembe, that have five gender classes. Of particular interest is Kalabari. In this dialect, the system based on animateness makes a primary distinction between animate and inanimate, with animate dividing into human and non-human, as in Fig. 2. It is proposed that the animate/inanimate distinction found in Kalabari (and some other dialects) is an innovation.

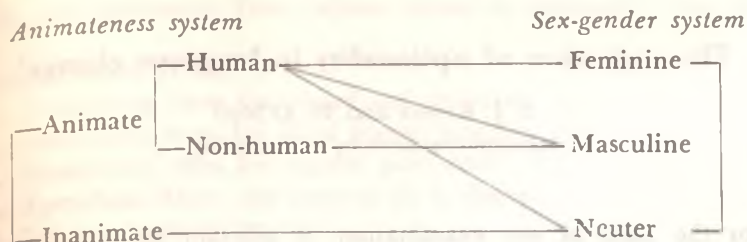


Fig. 2 Gender classification in Kalabari

5. Some of the syntactic changes in Ijo gender appear to be minimization of allomorphy and phonetic merging, and this is clearly illustrated in *Bumɔ*. *Bumɔ* has a masc/fem/neut distinction in the articles (*beí/mɔ ~ mɔ ~ mɔ/mɔ*), masc/fem/neut in the pronouns (*erí, ɔ-/ara, anɪ ~ a/aɪn ~ anɪ, ɔbɔ, ámɪnɪ*), masc/non-masc in the distal demonstratives (*erí/anɪ*) and in the proximal demonstratives (*beí/mɪ*).

6. Some of the gender markers perform other interesting functions in the morphosyntax of Ijo. For example, in Kalabari four kinship concepts: 'father', 'mother', 'husband', and 'wife' are each expressed by a short form and a long form, the latter being composed of the short form plus *bɔ* 'person, one (human)'. The short and long forms of these kinship expressions are used in different contexts: the short form occurs immediately after a singular possessive qualifier which refers to a specific individual; the long form occurs elsewhere.

The reduction of optionality in language change

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1. On the basis of our examination of relevant data, we are able to motivate three successive rule systems which characterize the early modern English plural morphology; namely, those of John Hart, Richard Hodges, and Mather Flint.² We present those systems here—it being the earliest, Hart's system first:

(1) Hart's system (ca. 1550–1570)

1. Underlying form for plural, genitive, and third person singular: /s/
2. Underlying form for regular past tense and participle: /d/
3. Epenthesis Rule

$$\emptyset \rightarrow i / \left[\begin{array}{c} + \text{cor} \\ \propto \text{strid} \\ \propto \text{cont} \\ + \text{high} \\ + \text{syll} \end{array} \right] \langle a \rangle \left\{ \begin{array}{c} + \text{cor} \\ \propto \text{cont} \end{array} \right\} \#$$

If $\langle a \rangle$, then obligatory; otherwise optional

4. Plural Voicing Rule (optional)

$$[- \text{voice}] \rightarrow [+ \text{voice}] / [+ \text{voice}] \# \text{---} \# \text{noun/verb}$$

5. Past Tense Voicing Assimilation Rule

$$[+ \text{voice}] \rightarrow [- \text{voice}] / [- \text{voice}] \# \text{---} \# \text{verb}$$

The next system to summarize is that of Richard Hodges:

(2) Hodges' system (ca. 1630–1650)

1. Underlying form for plural and genitive: /s/
2. Underlying form for regular past tense and participle: /d/; underlying form for third person singular: /z/
3. Epenthesis Rule

$$\emptyset \rightarrow i / \left[\begin{array}{c} + \text{cor} \\ \propto \text{strid} \\ \propto \text{cont} \end{array} \right] \text{---} \# \left[\begin{array}{c} + \text{cor} \\ \propto \text{cont} \end{array} \right] \#$$

4. Plural Voicing Rule (optional)

$$[- \text{voice}] \rightarrow [+ \text{voice}] / [+ \text{voice}] \# \text{---} \# \text{noun}$$

5. Verb Voicing Assimilation Rule

$$[+ \text{voice}] \rightarrow [- \text{voice}] / [- \text{voice}] \# \text{---} \# \text{verb}$$

1) The following material is abstracted from Keyser and O'Neil (ms.), Chapter 3.

2) For a summary of the relevant data, see Keyser and O'Neil (1980) and (ms.), Chapter 3. There are differences between the analyses given in these two places because of some suggestions of Lauren Cowles, who brought to our attention the systematicity in Hart's orthographic practices with respect to the various plural endings found in his work.

Finally, we summarize Flint, whose system is essentially that of modern English:

(3) Flint's system (1706)

1. Underlying form for plural and genitive: /z/;
underlying form for third person singular: /z/
2. Underlying form for regular past tense: /d/
3. Epenthesis Rule: the same as (2) 3, above
4. Voicing Assimilation
[+voice] → [-voice] / [-voice] # — #

2. Historical Development of the Three Systems.

We now consider a comparison of these three rule systems in order to understand the historical progression which they follow. In particular, we show that each subsequent system exhibits modifications consistent with the view that phonological change takes place in such a fashion as to minimize optionality and maximize simplicity. Indeed, the historical changes which we observe suggest that an appropriate evaluation metric would view the elimination of optionality as a contribution to a phonology's simplification just as the elimination of features from rules contributes to simplification.

To begin with, Hart's morphology makes a division among plural, genitive, third person singular, and past tense morphemes in a way which distinguishes it from modern English. Consequently, a single rule of voicing assimilation affecting all of these morphemes is not possible in Hart. In order to treat the voicing phenomena as an example of ordinary assimilation, it is necessary to specify the nominal and third person singular morphemes as underlyingly voiceless while the past tense morpheme is underlyingly voiced. The optional Plural Voicing Rule in Hart's grammar would, then, appear to be a likely place for simplification, given the theoretical framework being developed here.³ It may not be immediately obvious that simplification has taken place in Hodges' grammar at this point. Such is, in fact, the case. Note that in Hodges the voicing assimilation rules are divided in a way which differs from that in Hart. In particular, Hodges' grammar re-categorizes the third person singular morpheme so that it as well as the past tense morpheme is underlyingly voiced. This allows the statement of the voicing assimilation rules to be simplified by reducing the morphological information required; namely, the rules are stated in Hodges as applying in one case only to nouns and in the other only to verbs. This was not possible in Hart where the Plural Voicing rule applied to nouns and verbs while the Voicing Assimilation rule was restricted to verbs.

How might that change have come about? Recall that in Hart the Plural

3) Notice that Hart's grammar is also simplified through the loss of an optional part of the Epenthesis Rule according to which the syllabic form of the plural ending could be optionally added to such words as *copy*, *enemy*, etc. (Note that here and throughout this paper, we often use the word 'plural' to refer to the full set of inflectional phenomena under discussion: plural, genitive, third person singular, past tense and past participial forms.)

Voicing rule is, in fact, a collapsed rule which contains two subparts. The first subpart affects assimilation in nominal forms and the second subpart in verbal forms. Because the featural characterization of the rules is identical for both morphological classes, the rule must be collapsed as in (1.4) above. Let us suppose, however, that the optional status of each of these subparts is subject to independent change of status. In particular, let us suppose that the optional label attached to that subpart of rule (1.4) which applies to verbs is changed to obligatory. The effect of this will be to produce a grammar in which the third person singular morpheme will no longer be subject to free variation. Its voicing property is totally predictable in terms of the preceding segment.

We represent this change as follows:

- (4) Post-Hart Assimilation Rules
- a. Plural Voicing Rule (optional)
 $[-\text{voice}] \rightarrow [+ \text{voice}] / [+ \text{voice}] \# \text{ — } \#]_{\text{noun}}$
 - b. Plural Voicing Rule
 $[-\text{voice}] \rightarrow [+ \text{voice}] / [+ \text{voice}] \# \text{ — } \#]_{\text{verb}}$
 - c. Past Tense Voicing Assimilation Rule
 $[+ \text{voice}] \rightarrow [- \text{voice}] / [- \text{voice}] \# \text{ — } \#]_{\text{verb}}$

We take (4) to represent an adult innovation in Hart's grammar. However, (4) is not an optimal grammar and a child faced with constructing a grammar on the basis of the output of (4) would, in conformity with principles of simplicity, acquire the grammar represented in (2); i.e. Hodges' grammar. Let us consider why this is so.

Notice that Hart's Past Tense Voicing Assimilation Rule (repeated in (4c) above) is an obligatory rule which is restricted to verbal environments. Given this rule, however, it is possible to eliminate (4b) from the Post-Hart grammar and still derive the same output. The mechanism for achieving this simplification is to reanalyze the third person singular ending /s/ as /z/.

Once again we find that the shift of a rule from optional to obligatory status has resulted in the simplification of a grammar. Note, however, that Hodges' grammar still contains an optional remnant of Hart's grammar; viz., the possibility of having a voiceless plural ending attached to a noun that ends in a voiced segment. We might expect that this would be the next point of change if reduction of optionality were highly valued in historical change. Assume that such innovation was made. This would give us the following set of assimilation rules, once again representing an adult innovation:

- (5) Post-Hodges Assimilation Rules
- a. Plural Voicing Rule
 $[-\text{voice}] \rightarrow [+ \text{voice}] / [+ \text{voice}] \# \text{ — } \#]_{\text{noun}}$
 - b. Verb Voicing Assimilation Rule
 $[+ \text{voice}] \rightarrow [- \text{voice}] / [- \text{voice}] \# \text{ — } \#]_{\text{verb}}$

As before, this grammar is not an optimal one. That is, it is possible to simplify it by eliminating (5a). The mechanism for doing is the same as before; name-

ly, the underlying plural morpheme /s/ is reanalyzed as /z/. Thus, the Verb Voicing Assimilation Rule is generalized. When we come to Flint, we see that just this grammar has arisen. The underlying plural morpheme has been collapsed with the third person singular morpheme so that all three morphemes are underlyingly voiced and therefore subject to the same rule of voicing assimilation without exception. In the grammar of Flint, therefore, the set of assimilation rules has been reduced to one and the set of underlying morphological shapes has been reduced to two, both voiced.

3. Conclusion

In previously published research, we have shown that the Epenthesis Rule of Modern English came into the language from Old French through Anglo-Norman. We saw that as a result of the contact between the two languages, English spread its genitive to the Anglo-Norman vocabulary while the latter spread inorganic *e* to English, thereby paving the way for the generalization of the Epenthesis Rule throughout the entire language.

A consequence of this merger of two systems was the large scale introduction of optionality into the grammar. This is particularly clear in Chaucer whose verse shows a great deal of free variation in the application of the Epenthesis Rule. The facts of the merger provided a ready mechanism for the elimination of this optionality. Thus, from the very beginning the noun plural and the regular past tense morphology overlapped at the point of the Epenthesis Rule. These morphemes were kept apart because of differences in voicing which required different voicing assimilation rules. However, the highly valued character associated with minimizing optionality and the inherent similarity of the assimilation rules set off the chain of processes reviewed above.

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Irregular Sound Change Due to Frequency

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The notion of irregular sound change due to frequency is not new. It would be difficult for me to say who was the first to use this term, but in any case it was used as early as 1846 by Diez (1846:12), the founder of the comparative grammar of Romance languages, who considered Fr. *sire* < Lat. *senior* as 'durch häufigen Gebrauch verkürzt'. Some years later, the famous etymologist Pott (1852:315) stated that It. *andare*, Sp. *andar*, and Fr. *aller* derive from Lat. *ambulāre* 'mit zwar ungewöhnlichen, aber durch die Häufigkeit des Gebrauchs von diesem Worte gerechtfertigten Bubstabenwechseln'. Other linguists followed them. There is, however, an essential difference between the opinions of my predecessors and mine on this subject. Until now, irregular sound change due to frequency has been considered as something sporadic, affecting only the vocabulary, whereas, to my mind, irregular sound change due to frequency, which concerns also reductions in morphemes, especially in inflectional ones (which are even more frequently used than words), is the third essential factor of linguistic evolution, in addition to regular sound change and analogical development: in any text of any language, more or less one third of the words show an irregular sound change due to frequency. Here is the fragment of a fable by La Fontaine, where all words which have undergone irregular sound change due to frequency are printed in italics:

*Le Renard s'en saisit, et dit: Mon bon Monsieur,
Apprenez que tout flatteur
Vit aux dépens de celui qui l'écoute,
Cette leçon vaut bien un fromage, sans doute.
Le Corbeau honteux et confus
Jura, mais un peu tard, qu'on ne l'y prendrait plus.*

Obviously, my explanation of irregularities in the words printed in italics differs from the traditional. Among others, I think it is erroneous to explain these irregularities by the lack of stress. Here is a small list where, on the left, there are the existing forms and, on the right, the forms which should be awaited if they were really unstressed:

<i>illum mūrū</i> > <i>le mur</i>	* <i>illummūrū</i> > * <i>elmur</i> > * <i>eumur</i>
<i>illam mātrem</i> > <i>la mère</i>	* <i>illammātrem</i> > * <i>ellemère</i>
<i>illōrum filius</i> > <i>leur fils</i>	* <i>illōrumfilius</i> > * <i>ellourfils</i>
<i>casam mē</i> > <i>chez moi</i>	* <i>casammē</i> > * <i>chèsemoi</i>
<i>nōn cantat</i> > <i>ne chante</i>	* <i>nōncantat</i> > * <i>nonchante</i>

meam mātrem > *ma mère*
nostrōs patrēs > *nos pères*
ego tremulō > *je tremble*
dē illum mūrū > *du mur*
ecce istum mūrū > *ce mur*

**meammātrem* > **memère*
 **nostrōspatrēs* > **nôtrepères*
 **egotremulō* > **étremble*
 **dēillummūrū* > **delmur* > **deumur*
 **ecceistummūrū* > **écémur*

For more details, see Mańczak 1978.

In brief, the theory of irregular sound change due to frequency can be presented as follows. There is a synchronic law according to which the linguistic elements which are more often used are smaller than those which are less often used. There is a kind of balance between the size of linguistic elements and their frequency. Anyhow, the size of linguistic elements is not stable. As a result of regular sound change, the size of words may change considerably as the comparison of some Old and New High German words shows:

OHG *ūf* (2 phonemes) > NHG *auf* (3 phonemes)—increase of 50%;
lēra (4) > *Lehre* (4)—no change;
hros (4) > *Ross* (3)—decrease of 25%;
skōni (5) > *schön* (3)—decrease of 40%.

Since the frequency of words is not stable either, it may happen that the balance between the size of a word, or of a morpheme, and its frequency is disturbed. If a word or a morpheme becomes too short in relation to its frequency it is replaced by a longer one. But if a linguistic element (i.e., a morpheme, word, or group of words) becomes too long in relation to its frequency, it must be shortened, and then there are two possibilities: either a mechanical shortening (*autobus* > *bus*) or an irregular sound change due to frequency (*master* > *mister, you are* > *you're*, Lat. *cantābat* > Fr. *chantait*).

If irregular sound change due to frequency is far advanced, it consists of the decay of one or more phonemes, e.g. *God be with you* > *good-bye*. However, if the development due to frequency has just started, it may only consist in a partial reduction of a phoneme, e.g.

(a) the long vowel undergoes a reduction: Goth. *sunus* shows a short vocalism although the vowel was long in Proto-Indo-European, cf. OI *sūnú-*, Lith. *sūnis*, OCS *synŭ*.

(b) the degree of the vowel opening is subject to a reduction (*a* > *e* > *i* or *a* > *o* > *u*), e.g. in OHG *nēmamēs* > *nēmumēs*, the vowel *a* narrowed to *u*, in OHG *stān* > *stēn*, to *e*;

(c) the full vowel is changed into a reduced one: *shall* may be pronounced with an [ə];

(d) the nasal vowel is subject to denasalization: Pol. *będzie* 'will be' may be pronounced with *e* instead of *ɛ*

(e) the palatal consonant undergoes a depalatalization: the Russian reflexive pronoun *-sja* may be pronounced as [sa];

(f) the voiceless consonant becomes voiced, the pronunciation of voiced consonant seeming to be easier: an irregular voicing occurs in the ending -s (plural, genitive, and 3rd pers. sing.) and in some very frequent words like *as*, *of*, or *the*.

There are five criteria which allow us to recognize that irregular sound change due to frequency is involved:

(1) If a frequency dictionary for a given language and for a given epoch exists, we may use it, since the majority of words showing an irregular change due to frequency belong to the thousand words most frequently used in the given language. E.g. in French, the distribution of these words is as follows:

First thousand	99
Second thousand	9
Third thousand	4
Fourth thousand	2
Fifth thousand	1
Sixth thousand	0

For more details, see Mańczak 1969:20. There can be no doubt that there is a connection between the irregularities in question and frequency, since $\chi^2 = 409,55 > 11,07$.

(2) In addition to irregular sound change due to frequency, there are other irregular sound changes, namely assimilations, dissimilations, metatheses, and expressive or overcorrect forms. These irregular sound changes are characterized by the fact that they occur in different words in different languages, e.g. an assimilation took place in Fr. *chercher* < *cercher*, a dissimilation in Fr. *faible* < *flëbilem*, a metathesis in Fr. *troubler* < **turbuläre*, while *h* in Fr. *herse* is expressive and *s* in Fr. *chaise*, overcorrect. However, in other Indo-European languages, it would be difficult to find a word meaning 'to look for' with an assimilation, a word meaning 'weak' with a dissimilation, a word meaning 'to trouble' with a metathesis, a word meaning 'harrow' with an expressive phoneme and a word meaning 'chair' whose form would be overcorrect. In other words there is no parallelism between the words showing irregular changes as assimilations, dissimilations, etc. in different languages, whereas irregular sound change due to frequency occurs in various languages more or less in parallel which is explained by the fact that the most frequently used words are nearly the same in all languages. Some examples follow.

Ger. *Herr* shows an irregular reduction. The same is true for *mister* < *master* or *sir* (which derives from Fr. *sire*, also irregularly developed from Lat. *senior*), Fr. *monsieur* < *monseigneur*, or Sp. *don*, being used alongside the more but not quite regular *dueño*. Although the opinions on the etymology of Pol. *pan* or Czech *pán* are not unanimous, it is unquestionable that these words derive from a longer form, as it is the case with Russ. *barin*, which has developed from *bojarin*.

Fr. *frère* < *frātre*m (against *pierre* < *petram*) shows an irregular reduction

of the group *tr*. Also irregular are Sp. *fray*, *frey*, Port. *frei*, It. *frat'*, *fra*. Common Slavic **bratrŭ* lost its *r* in the majority of Slavic languages, cf. Russ. *brat*. Swed. *bror* and Lith. *brolis* are not regular either.

Lat. *ambulāre* undergoes a reduction in Romance languages, cf. Fr. *aller*, It. *andare*, Sp. *andar*, Prov. *ana*, etc. The same applies to Common Germ. **gangan*, cf. go, Ger. *gehen*, Dutch *gaan*, Dan. *gå*, etc. In the same way, Common Slav. **ŕidlŭ* (> Russ. *šel*, Pol. *szedł*, etc.) exhibits an irregular reduction, namely the occurrence of *i* instead of *e*.

Verbs denoting the act of speech often show an irregular sound change due to frequency, cf. *says*, *said*, Lat. *ājō*, *inquam*, Fr. *parler*, It. *parlare*, Roum. *vorbi*, Pol. *mówić*, etc. As a parallel to the disappearance of *r* in *speak* or OHG *spēhhan*, one may quote the irregular development of Russ. *govorit* 'speaks', which, in dialects, is reduced to *gryt* > *gyt*.

(3) If in a given language, a morpheme, word, or group of words occurs in a double form (regular and irregular), irregular sound change due to frequency is characterized by the fact that the irregular form is usually used more often than the regular one, e.g. the suffix *-isk* developed regularly into *-ish* and irregularly into *-sh*, and *Wel-sh*, *Scot-ch*, *Fren-ch*, *Dut-ch*, *Man-x* are used more frequently than *Swed-ish* or *Dan-ish*. This is similar in other West Germanic languages, cf. Ger. *deut-sch* but *französ-isch*, *italien-isch*, Dutch *Vlaam-s*, *Fran-s* but *Macedon-isch*. An identical situation is found in the Scandinavian languages, cf. Sw. *sven-sk*, *ty-sk* but *jon-isk*, *arab-isk*, Dan. *dan-sk*, *nor-sk* but *bulgar-isk*. The Germanic *-isk* also reached French, where, similarly, it had a double development: the irregular one in more frequently used forms of the type *franç-ais* and the regular one in less common forms of the type *Franç-ois*.

In the fragment of a piece by Molière, the frequency of regular and irregular forms of the same origin is as follows:

<i>aux</i> < <i>ad illōs</i>	4	<i>à eux</i>	—
<i>aux</i> < <i>ad illās</i>	1	<i>à elles</i>	—
<i>des</i> < <i>dē illās</i>	14	<i>d'elles</i>	1
<i>des</i> < <i>dē illōs</i>	8	<i>d'eux</i>	—
<i>la</i> < <i>illam</i>	52	<i>elle</i>	3
<i>les</i> < <i>illōs</i>	22	<i>eux</i>	1
<i>les</i> < <i>illās</i>	30	<i>elles</i>	12
<i>me</i> , <i>m'</i> < <i>mē</i>	46	<i>moi</i>	19
<i>messieurs</i> < <i>meōs seniōrēs</i>	4	<i>messeigneurs</i>	—
<i>monsieur</i> < <i>meum seniōrem</i>	6	<i>monseigneur</i>	—

As one sees, irregular forms are more frequent than their regular equivalents (for the rest of this list, see Mańczak 1969:22).

(4) If irregular sound change due to frequency occurs within a paradigm or a word family, it may be recognized by the fact that only the more commonly used forms are subject to it, while the forms used less frequently remain regular.

When we consider Ger. *haben*, it appears that the forms in the sing. pres. ind., which are more often used, are shortened (*hast, hat*), whereas the plural forms *haben, habt* are regular. As far as the relation of this verb to its compound forms is concerned, one may compare the irregular E *has, have, had* to the regular *behaves, behave, behaved*. In French, *ai, as, a, ont* are irregular, while the less frequently used forms *av-ons, av-ez* are regular. A similar situation is observed in other Romance languages.

In Gothic and in Old High German, the declension of the *ā*-stem nouns was of the following shape:

			Gothic	Old High German
Sing.	Nom.	*-ā	<i>giba+</i>	<i>geba+</i>
	Acc.	*-ām	<i>giba+</i>	<i>geba+</i>
	Gen.	*-ās	<i>gibōs</i>	<i>geba, gebu+</i>
	Dat.	*-āi	<i>gibai</i>	<i>geba, gebu+</i>
Plur.	Nom.	*-ās	<i>gibōs</i>	<i>gebā+</i>
	Acc.	*-āns	<i>gibōs</i>	<i>gebā+</i>
	Gen.	*-ōm	<i>gibō</i>	<i>gebōno</i>
	Dat.	*-āmīs	<i>gibōm</i>	<i>gebōm</i>

As one knows, Indo-European **ā* and **ō* result in **ō* in Proto-Germanic. Therefore, the development of the forms not marked by crosses is regular. In historical grammars, this double development is accounted for by the existence of the acute and the circumflex intonations in Proto-Indo-European. This explanation gives the impression of an *ad hoc* explanation for two reasons: (a) the distribution of regular and irregular endings in Gothic differs considerably from that of regular and irregular endings in Old High German; (b) everything indicates that the Balto-Slavic intonation arose independently of the Greek; therefore, there is no evidence that any intonation existed in Proto-Indo-European. For these reasons, the irregular endings of Gothic and OHG *ā*-stem nouns are to be accounted for on the basis of their frequency, which is proved by the fact that, both in Gothic and in Old High German, the irregular development occurs in the more frequently used endings, since it is known that (a) the singular is used more often than the plural; (b) the nominative and the accusative are used more often than the dative and the genitive. As a parallel, the fact may be cited that, in the frequently used Latin nominative singular of the type *tabul-a*, the final vowel underwent an irregular shortening, whereas, in the less frequently used ablative singular *tabul-ā*, the old length was preserved. For more examples, see Mańczak 1978 a.

(5) If one compares two irregular sound changes due to frequency in a linguistic atlas, the area of the more frequent form is larger than that of the less frequent one. In different languages, the infinitive suffix undergoes an irregular reduction, e.g. in English, where *give* is shortened whereas Ger. *geben* is regular. The same applies to Romance, Slavic, and Baltic languages (Mańczak 1977:299). In a French text, the infinitive in *-er* occurs 101, that in

-ir 36, and that in -oir 18 times. In the *Atlas linguistique de France*, I found that *r* is not pronounced in the infinitives in -er in 291 villages, in the infinitives in -ir in 188 villages, and in the infinitives in -oir in eight villages. In other words, the area of dropping *r* in the frequent verbs of the type *aller* is larger than that of dropping *r* in the less frequent verbs of the type *dormir*, while the area of dropping *r* in the rare verbs of the type *avoir* is the smallest.

The great advantage of the theory of irregular sound change due to frequency consists in the fact that its verification is very easy: a few days are enough for collecting necessary statistical data.

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The Indo-European Lexicon and its Usage as a Problem in Reconstruction

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It is a well-known fact that it is difficult, and in many cases impossible, to establish beyond doubt the IE terms for common concepts of the real and abstract world. This is so largely because the IE languages, which are the material basis for our attempts at reconstruction, only too often have different terms for the same concept, or feature variants whose original semantic distinctions we can no longer discern. Those parts of the universal IE vocabulary which it is possible to reconstruct beyond doubt are usually elements of the basic vocabulary, i.e. terms for concepts from man's natural sphere, his environment and living conditions, which express fundamental experiences and insights, and whose "meaning" is constant for all members of the speech community. This includes terms for man himself with respect to sex, kinship and age, terms for body parts and bodily functions, unmistakable phenomena and impressions of inanimate nature such as "heaven" and "earth"; "sun", "moon" and "stars"; "fire" and "water"; "wind", "rain", "it is snowing", "winter", "cold", "warm" etc. To a certain extent it also includes elements of animate nature, such as the names of wild and domesticated animals, and wild and cultivated plants, though here changes in ecological and economic conditions may impair the constancy and universal validity of this section of the vocabulary. But "fish" and "bird" at least were fixed concepts in the minds of the Indo-Europeans. Further, the vocabulary which has survived proves that they knew and named the wolf and the bear, and that they had domesticated cattle, sheep and pigs. Less certain, however, is whether the early Indo-Europeans, or all the later Indo-Europeans after the migrations, knew the beech, which does not occur east of a line from Königsberg (Kaliningrad) to Odessa.

The question of the name of the beech leads us to the temporal and spatial aspect. This, in conjunction with the sociological aspect (socio-cultural differences, e.g. so-called class differences which are reflected in usage), is responsible for the greater part of language variation (i.e. variants on the synchronic level, language change on the diachronic axis). What the IE languages have preserved of this original diasystem, is, as a result of a variety of accidents of history, but a limited selection. To attempt to reconstruct the original reference system out of these isolated reflexes would be an impossible task.

The objective of IE linguistics is to reconstruct Proto-Indo-European, and thereby also to throw light on the prehistory of the individual IE languages. "To reconstruct Proto-Indo-European" is, however, easier said than done.

Behind the optimistic and sometimes naïve view that it is possible to reconstruct "Indo-European"—or at least its basic features—lies the illusion of the unified character of IE. This illusion is fostered by the methods of comparison and linear reconstruction, whereby we compare only what is alike or very similar, which is then filtered out, assigned a hypothetical pre-form and projected back into the proto-language, thus giving it a semblance of unity. But when we include the differences, rather than explaining them away, we get alternative reconstructions. And these diverging results of linear reconstructions indicate the existence of synchronic irregularities and variations, or diachronic processes within the proto-language.

Everything we know about language tells us that irregularities can, and indeed must have existed at all times. And this is precisely what makes the reconstruction of IE (or of a branch of it, such as Germanic) so difficult, and virtually impossible. For the proto-language was not the ideal unified system which we aim at, but a system of constants and variables, in which even the constants (phonemic, morphemic, lexemic etc.) have only an ideal validity, and variation or variability dominates in language behaviour, as a result of the ability of speakers to use language creatively. This built-in variability makes it impossible to reconstruct a system which is unequivocal in all details, and claims that this can be done are illusory. There is no "pure method" by means of which one can objectively arrive at a "pure system". What can be extrapolated with some degree of certainty is basic typological patterns and trends of development, morpho-semantic categories, paradigmatic classes and similar broad structures; the small-scale pattern cannot be determined in detail, precisely because it was by nature variable. We can do no more than to reconstruct set-pieces of a diasystem. To group and arrange these into individual systems which mirror the spatial and temporal continuum which was Proto-Indo-European is an attractive task, which however can only be tackled, by means of "internal reconstruction", when the first stage of work has been completed. But we are still a considerable distance away from such a step. A breakaway from the rigid reconstruction of the "Standard Model" of Indo-European, largely dominated by Greek and Indo-Iranian, and a breakthrough to new insights about a differentiated picture of the proto-language can be expected when we start with Hittite, which can increasingly be seen to be one of the key languages for the reconstruction of IE.

To return to the lexicon: The lexicon reflects the outer world through the mirror of a world view which is conditioned by knowledge and experience and is thus subject to change. To reconstruct a prehistoric lexicon is thus not merely difficult in itself, it also implies at the same time a hypothesis about the 'Weltbild' which it expresses, that is its "semantics". Today, for example, a hammer is usually a tool made of iron. But an etymological analysis reveals that the word *hammer* has survived a cultural change and that the original context was the Stone Age, in which the hammer was a stone tool (Slav. *kamen-stone*).

The greater part of the lexicon does not remain constant over a long period of time, but is subject to variation, in form or in content. This holds for the proto-language too, which cannot be thought of as a temporal and spatial unity, for in it all kinds of sociological differences must have influenced both the formation of concepts and their expression. In the daughter languages the repertoire of the proto-language is further selected, modified, replenished and renewed, often by means of building units (roots, morphemes) inherited from the proto-language. The result is that the dividing lines between inherited forms and substances, and new formations from this material, but shaped by a different spirit, are often blurred when perceived from a later point in time.

One can assume a relatively unified, though not completely uniform, use of the lexicon (and language use in general) at the most for Early IE, and even then only if this was a linguistic unit small in number of speakers and territorial extension, within which synchronic communication was in principle possible between any two speakers. In practice this will rarely have been the case, as public life is carried on in large groups, private life in small groups, which furthermore have an intimate sphere about which no communication takes place with the outside world. Intimate life, therefore, is not represented by a common norm of language use, but by linguistic habits of a special kind (jargons and group vocabulary, idiolectisms, individual use of language).

Communication in the broadest sense takes place only about important things, and this alone leads to the establishment of a language norm, which, precisely by virtue of the important function it fulfills for the community, is passed on to successive generations. Things which are of purely individual interest and of no importance for the community, and which are only talked about by a narrow circle of people, are named *ad hoc*. These names are formed on the spur of the moment, with the means at hand, and within the context of the given pragmatic situation. Linguistic traditions resulting from such formations are usually short-lived and limited to a small radius; norms and traditions of general validity do not derive from linguistic intercourse in the intimate sphere. This is why the vocabulary of "unimportant" things is, to all intents and purposes, inaccessible to "reconstruction", for these are being constantly renamed everywhere and at all times. Many things have, in fact, no fixed name, but are named as the need arises. For example, the many varieties of insects, grasses and herbs have no fixed nomenclature in the general language, for the very reason that most of them are unimportant and thus not subjects for communication. To the extent that they can be important (for example, medicinal herbs), knowledge of them is restricted to experts, who strive to keep this very knowledge secret and communicate about it only among themselves. This source too has contributed nothing or very little to the common linguistic tradition. Thus, wherever the individual IE languages have preserved words from the intimate, family sphere, which have the signs of being very old, it is likely that they derive from some group language or dialect. When such material allows us to reconstruct forms, these are not to be con-

sidered valid for the common language, but for some unspecified dialect or sociolect of it.

In the field of sexual affairs, privacy and intimacy are effectively protected by the principle of non-communication with outsiders. The appropriate language, usually of an affective nature, protected by taboo, thus remains within the intimate and confidential sphere and does not penetrate into the general stream of communication. It is only through "indiscretion", or covertly, that it can spread, and then only to a limited extent. Although this "intimate" vocabulary belongs largely to the category "unimportant", or, rather, unsuitable, uninteresting for communication, there are a number of exceptions: These are some terms of a general status for sexual organs and functions. The importance of these organs and functions for the life of each individual and above all for the propagation of society is generally recognized, so that communication about these biologically and sociologically relevant aspects of the sexual sphere is necessary and does indeed take place. This explains why it is precisely the basic terms of this sphere that have been inherited and can thus be reconstructed: IE **ǵen-* "beget, give birth", **eibh-* "futuere", **pesos* "penis", **ǵ^{we}e/olbhos* "womb", *(*w*)*ers-* "inseminate" (adj. "male"), **dhē-* "suck, give suck" (adj. "female") and others. As the relevant organs and functions are basically the same for man and the animals, the requirements of animal husbandry also contributed to the preservation of this vocabulary.

To conclude, let us say a word about the interdependence of the semantic structure of the vocabulary and word formation.

When we consider the lexical system of a language as a whole, it becomes clear that productive word formation and its products play a subordinate role. They serve to fill in and fill up the system *en détail*, also to replenish and restore it, whereas the fundamental positions of the system are filled by words which, synchronically at least, are unmotivated, i.e. not dependent on any other word, but which themselves form the basis of derivatives.

From an historical point of view, the "lexicon" of a language consists of inherited words and loanwords, as well as recent formations, which are new coinages (or replacements for old words) made of the language's formal resources. The inherited words and loanwords are fully lexicalized elements of the vocabulary, whereas the products of productive word formation are ambivalent: As long as they are potentially capable of being formed, they need not be considered fully-fledged members of the lexicon. However, they frequently acquire—even if only temporarily—the status of a "coined" word which can be called on at will after repeated spontaneous formation and use, and can thus be considered lexicalized. The creations of word formation can be misinterpreted as inherited words when the formal elements of which they are built are inherited; however, they are only in substance or in type inherited, and not as individual lexemes. This must be kept in mind when languages are compared: Skt. *bhṛ-ti* "carrying, support", Lat. *fors, fortis* "chance" and OHG *gi-burt* "birth" are not "inherited words", but rather independent new forma-

tions based on the inherited productivity of *ti*-formations in the individual languages. In other words, just as **bhṛ-ti* was a possible formation in IE, so it still was in the individual IE languages, as is indicated by the varying meanings, in each case based on the verb. Only at some later stage the word became lexicalized in the individual languages, either because the method of formation was no longer productive (e.g. German *Geburt*), or because the connection with the verb is no longer transparent (Lat. *fors*). For IE, however, a word **bhṛ-ti* itself cannot be postulated, but only the possibility of its formation; that it actually was formed, is proven by the fact that the process is repeated into the period of the individual languages.

In future investigations of the vocabulary of IE it will be necessary not just to reconstruct the lexemes of the proto-language on the basis of the words attested in the individual IE languages, but also to establish, by observing attested productive mechanisms, what words *could* have been formed, and were thus *potentially* available.

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Die Funktion des Optativs in abhängigen Aussagesätzen

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E. Schwyzer (*Griechische Grammatik* II. 334) vertritt folgende allgemein verbreitete Ansicht über den optativus "obliquus": "Man gewöhnte sich nach präteritiven Formen an den (im Gegensatz zum Indikativ—R.N.) subjektiveren Optativ (bes. Potential), der dann in Aussagesätzen und in der oratio obliqua mehr und mehr zu einem formellen Ersatz des Indikativs wurde..." Kühner-Gerth (*Griechische Grammatik* I.254s.) stellen fest, dass der Optativ andere modi verträte, "wenn die Behauptung ausdrücklich als *Gedanke des Subjektes im Hauptsatz* bezeichnet werden soll", und K. W. Krüger (*Griechische Sprachlehre* 54.6) meint, dass der Indikativ "in ideell abhängigen Sätzen" nach einem Präteritum "in den Optativ übergehen (kann)". Im Gegensatz zu Schwyzer bemühen sich mithin die drei letztgenannten Gelehrten, dem Optativ auch in Objektsätzen wenigstens eine gewisse Eigenfunktion zuzuschreiben. Diesem Prinzip folgend wurden die von verba dicendi et sentiendi abhängigen *ὅτι*—und *ὥς*—Sätze bei Herodot und Thukydides neuerdings untersucht und für jedes Verb einzeln die Sätze, die Indikativ, bzw. Optativ aufweisen, miteinander verglichen. Es handelt sich um folgende Verben:

1. Verba dicendi: ἀγγέλλω, δείκνυμι, δηλώω, διδάσκω, κατηγορεῖμαι, ἐπικαλέω, λέγω, μέμφομαι, φημί, φράζω.
2. Verba sentiendi: αἰσθάνομαι, ἀκούω, γινώσκω, ἐπίσταμαι, λογίζομαι, μανθάνω, πυνθάνομαι.

Das Ergebnis: der Inhalt der im *Indikativ* abgefassten Nebensätze bestätigt Vorwissen oder wird als Neuigkeit (Tatsache) schlechthin apperzipiert. Der *Optativ* hingegen gibt zu erkennen, dass (im Falle eines v. dicendi) der Angesprochene, bzw. (im Falle eines v. sentiendi) das Subjekt des Hauptsatzes der Nachricht (bzw. Erfahrung) mit Spannung entgegensah oder eine vorgefasste Meinung hatte, die dem Botschaftsinhalt widerspricht. Dieses Resultat soll nun an einigen Satzepaaren demonstriert werden.*

1. Verba dicendi

ἀγγέλλω a) ind. Hdt. II. 152. 4 ἀγγέλλει τῶν τις Αἰγυπτίων ἐς τὰ ἔλεα ἀπικόμενος τῷ Ψαμμητίχῳ, ὥς οὐκ ἰδὼν πρότερον χαλκῷ ἄνδρας ὀπλισθέντας, ὥς χάλκεοι ἄνδρες ἀπερχόμενοι ἀπὸ θαλάσσης λεηλατεῦσι τὸ πεδίον diese Botschaft bestätigt die Information, die Psammetich ib. 152. 3 erhalten hatte

b) opt. Hdt. III. 36. 6 ἐπόθησε . . . ὁ Καμβύσης τὸν Κρείσαν . . . καὶ οἱ θεράποντες

... ἐπηγγέλλοντο αὐτῷ ὡς περιεῖη Kambyses hatte Krösus für tot gehalten, da er Befehl gegeben hatte, ihn zu ermorden

λέγω a) ind. Thuc. V. 37. 3 eine Gesandtschaft wird angekündigt οἱ τῶν Ἀργείων ἄνδρες ... ἐλπόντες ὅτι πρέσβεις πέμψουσι, ... ἀπῆλθον

Hdt. II. 150. 1 eine Naturerscheinung: ἔλεγον δὲ οἱ ἐπιχώριοι καὶ ὡς ἐς τὴν Σύρτιν τὴν ἐν Λιβύῃ ἐκδιδόει ἡ λίμνη αὕτη ὑπὸ γῆν cf. Hdt. VIII. 38

b) opt. Hdt. III. 130. 4 παράγοντες ... οἱ εὐνοῦχοι ἔλεγον πρὸς τὰς γυναῖκας ὡς βασιλεῖ οὗτος εἴη ὃς τὴν ψυχὴν ἀπέδωκε der Fremde offenbart sich als der Lebensretter; NB—der Optativ ist Regel, wenn eine Person vorgestellt wird, cf. Hdt. III. 140.3, IV. 145.3, VI. 41.3, VIII. 136.1; V. 31.1—eine Insel wird "vorgestellt": λέγει ... ὡς Νάξος εἴη νῆσος μεγάθι μὲν οὐ μεγάλη κτλ.

Thuc. VII. 83.1 οἱ δὲ Συρακόσιοι τῇ ὑστεραίᾳ καταλαβόντες αὐτὸν (sc. den Nikias) ἔλεγον ὅτι οἱ μετὰ Δημοσθένους παραδεδώκοιεν σφᾶς αὐτοῦς, κελεύοντες κάκεινον τὸ αὐτὸ δρᾶν. ὁ δὲ ἀπιστῶν σπένδεται ἱππέα πέμψαι σκεφόμενον die Botschaft ist so überwältigend, dass Nikias ihre Richtigkeit überprüfen will

δηλώω a) ind. Thuc. VII. 25.9 ἐπεμψαν ... πρέσβεις οἱ Συρακόσιοι ... δηλώσαντας ὅτι ἐν ἐλπίσιν εἶσι zu bezeugen, dass sie guten Mutes sind

b) opt. Hdt. II. 116.1 ("Ομηρος) μετήκε αὐτὸν (sc. τὸν λόγον—'Ελένης ἄπιξιν παρὰ Πρωτέα) δηλώσας ὡς καὶ τοῦτον ἐπίσταίτο τὸν λόγον. δηλον δὲ κατὰ παρεποίησιν ἐν Ἰλιάδι (Z 289–292) die Anspielung auf Paris' und Helenas Verbleib in Sidon impliziert laut Herodot—im Gegensatz zu allgemeiner Auffassung—dass Homer mit Helenas Verbleib in Ägypten vertraut war

2. Verba sentiendi

αἰσθάνομαι a) ind. Thuc. VI. 65.3 Truppenbewegungen der Athener werden entdeckt οἱ ἱππῆς οἱ Συρακοσίῳ ... αἰσθόμενοι ὅτι τὸ στράτευμα ἅπαν ἀνήκται ἀποστρεφόμενοι ἀγγέλλουσι τοῖς πεζοῖς

b) opt. Thuc. VIII. 100.2 αἰσθόμενος ... ὅτι ἐν τῇ Χίῳ εἴη ... σκοποὺς μὲν κατεστήσατο κτλ. Thrasylos hatte gemeint, Mindarus wäre im Hellespont, erfuhr jedoch, dass er in Chius sei

Pl. Prt. 328d ἐπεὶ ... ἡσθόμην ὅτι τῷ ὄντι πεπαυμένος εἴη ... εἶπον Sokrates hatte gemeint, Protagoras hätte seine Rede noch nicht beendet

ἐπίσταμαι a) ind. Hdt. I. 96.2 (Δηϊόκης) ἐπιστάμενος ὅτι τῷ δικαίῳ τὸ ἄδικον πολέμιόν ἐστι sprichwortartige Weisheiten stehen im ind., cf. Hdt. VII. 39.1

b) opt. Hdt. I. 122.2 hier handelt es sich wieder um die Entdeckung einer Identität (Κῦρον) ἐπίστασθαι ... ὡς βουκόλου τοῦ Ἀστυάγεος εἴη παῖς

μαρθάνω opt. Hdt. VIII. 107.2 χρόνῳ δὲ μαθόντες ὅτι οὐ νέες εἶεν ἀλλ' ἄκραι die Schiffer hatten Felsen für Schiffe gehalten

In den nun folgenden Belegen, in denen Indikativ und Optativ nebeneinander aufscheinen, bringt das Verb im Indikativ einen Tatsachenbericht, der im Zuhörer Spannung erregt und daher den Optativ desjenigen Verbs bedingt, das die Befriedigung dieser Neugier zum Inhalt hat.

Hdt. V. 97.1 ἐπειδὴ δὲ ἐπὶ τὸν δῆμον ὁ Ἀρισταγόρης ταῦτά ἔλεγε τὰ καὶ ἐν τῇ

Σπάρτη περὶ τῶν ἀγαθῶν τῶν ἐν τῇ Ἀσίῃ καὶ τοῦ πολέμου τοῦ Περσικοῦ, ὥς οὔτε ἀσπίδα οὔτε δόρυ νομίζουσι εὐπετέες τε χειρωθῆναι εἴησαν

Hdt. IX. 69.1 ἀγγέλλεται τοῖσι ἄλλοις Ἕλλησι τοῖσι τεταγμένοις περὶ τὸ Ἡραίου καὶ ἀπογενομένοις τῆς μάχης, ὅτι μάχῃ τε γέγονε καὶ νικῆεν οἱ μετὰ Πausaniew (cf. Hdt. IX. 38.2)

Hdt. VIII. 26.1s. ἐπυνθάνοντο οἱ Πέρσαι περὶ τῶν Ἑλλήνων τὰ ποίεοιεν. οἱ δὲ σφι εἶπον ὡς Ὀλύμπια ἄγουσι (der ind. ist der v.l. ἄγοιεν vorzuziehen) καὶ θεωρόειεν ἄνωγα γυνικόν καὶ ἱππικόν

cf. Thuc. VII. 25.9 ἐπεμψαν . . . οἱ Συρακόσιοι . . . ἀγγέλλοντας . . . τῆς ναυμαχίας πέρα ὡς οὐ τῇ τῶν πολεμίων ἰσχύι μᾶλλον ἢ τῇ σφετέρᾳ ταραχῇ ἥσσηθείεν, τὰ τε ἄλλα δηλώσαντας ὅτι ἐν ἐλπίσιν εἰσὶ ihre Niederlage wäre erstaunlicherweise nicht der Übermacht der Feinde zuzuschreiben (wie es gewöhnlich der Fall ist), sondern inneren Schwierigkeiten, und so gäbe es keinen Grund zur Besorgnis

Auf Grund der erreichten Erkenntnisse scheinen an zwei Stellen Emdationen erforderlich zu sein:

1) Hdt. VI. 70.1 (Δημάρατος) ἐπορεύετο ἐς Ἥλιν, τῷ λόγῳ φᾶς ὡς ἐς Δελφοῦς χρυσόμενος τῷ χρηστηρίῳ πορεύεται (v.l. πορεύεσθαι). Λακεδαιμόνιοι δὲ ὑποτοπηθέντες κτλ. Der Ind. πορεύεται ist hier nicht haltbar, da die Spartaner dem Demaratus keinen Glauben schenken; cf. id. I. 24.7—Periander überführt die Schiffer ihrer Lügnerzählung betr. Arion: φαμένον ὡς εἴη τε σῶς . . . καὶ μιν εὖ πρήσσοντα λίποιεν ἐν Τάραντι ἐπιφανῆναί σφι τὸν Ἀρίονα κτλ. Daher möchte ich vorschlagen, VI. 70.1 πορεύεσθαι vorzuziehen; zu ὡς mit Infinitiv-Konstruktion nach φημί cf. Hdt. III. 31.3 Αἰγύπτιοι δὲ (φασί) ὡς . . . τὴν γυναικὰ περιτίλαι καὶ ἐπανειρέσθαι κτλ.

2) Thuc. III. 29.1 Πελοποννήσιοι . . . προσμείξαντες . . . τῇ Ἰκάρῳ καὶ Μυκόνῳ πυνθάνονται ὅτι ἡ Μυτιλήνη ἐάλωκεν (v.l. ἐάλωκυῖα εἴη). Die Peloponnesier waren auf dem Wege zu Mytilene, um der Stadt zu Hilfe zu kommen, hörten jedoch, dass die Stadt bereits eingenommen war; ib. 29.2 βουλόμενοι δὲ τὸ σαφές εἶδέναι ist Ausdruck ihrer Ungläubigkeit—daher scheint hier der opt. ἐάλωκυῖα εἴη die richtige La. zu sein; cf. Hdt. I. 83—die Spartaner in ähnlicher Situation: καὶ σφι ἦδη παρεσκευασμένοις . . . ἤλθε ἄλλῃ ἀγγελίῃ ὡς ἡλώκοι τὸ τεῖχος τῶν Λυδῶν καὶ ἔχοιτο Κροῖσος ζωρηθεῖς. οὕτω δὲ οὔτοι μὲν συμφορὴν ποιησάμενοι μεγάλην ἐπέπαντο.

Der Optativ in abhängigen Aussagesätzen erweist sich mithin als Niederschlag einer Überraschung oder Enttäuschung, die im Empfänger der Nachricht ausgelöst wurde (könnte, sollte das wahr sein?). Diese Reaktion kann nur retrospektiv perzipiert werden, daher finden wir den Optativ als Indikator dieses Staunens nur nach einem Präteritum (cf. E. Koschmieder, *Zeitbezug und Sprache*, 11sq.).

Wie ich von Herrn Dr. N. Barri (Universität Jerusalem) erfahre, gibt es im albanischen Hauptsatz den sogen. "Modus admirativus" (cf. S. E. Mann, *Albanian Grammar* 1932, p. 36), der, durch Zufügung des Suffixes *kam* geformt, "mild surprise" ausdrückt. In Anlehnung daran möchte ich vorschlagen, den besprochenen griechischen Optativ—als *Abart des Potentialis*—nunmehr "optativus mirativus" zu benennen.

APPENDIX

• Hier folgen Belege, die im Haupttext nicht zitiert werden konnten. Sätze, in denen das Verb im übergeordneten Satz in einer Hauptzeit gehalten ist, wurden hier nicht mit einbezogen.

I. *Verba dicendi*: ἀγγέλλω—*opt.* Hdt. IV. 153, V. 33.3; Thuc. VII. 31.3, VIII. 6.4, 86.3, 108.1. δείκνυμι—*ind.* Hdt. VII. 172.1, IX. 58.2; *opt.* Hdt. I. 31.3, V. 22.2, VI. 43.3; Pl. Ap. 21c. δηλώω—*ind.* ("beweisen") Hdt. IV. 118.4; *opt.* ("implizieren") Hdt. II. 102.5, VII. 210.2, VIII. 34, 61.2; Thuc. I. 72.1 (Dependenzgrammatik!). διδάσκω—*ind.* Thuc. VIII. 72.1; *opt.* Thuc. III. 97.1, IV. 46.5. ἐπικαλέω—*ind.* Thuc. V. 83.4; *opt.* Thuc. V. 56.2. κατηγορέω—*ind.* Thuc. I. 91.1; *opt.* Thuc. VIII. 85.3. λέγω—*opt.* Hdt. I. 45.1, II. 139.2, III. 32.2, 75.2, V. 92.32, VI. 69.4, VII. 203.1, VIII. 19.1, 74.2, IX. 44.2; Thuc. I. 38.1, 90.4, II. 5.5, 48.2, IV. 11.4, 83.4, 114.3, V. 2.2, 61.2, VI. 25.2, VIII. 24.5, 45.4, 48.2, 92.2; Soph. Ph. 345sq. μέμνημαι—*ind.* Hdt. II. 133.2, III. 11.1, VI. 92.1; *opt.* Hdt. II. 169.3. φημί—*ind.* Hdt. VI. 70.1; *opt.* Hdt. I. 24.7.

II. *Verba sentiendi*. αἰσθάνομαι—*ind.* Thuc. VII. 49.1, 65.1; *opt.* Thuc. IV. 122.3, V. 2.3. γινώσκω—*ind.* Hdt. III. 61.3, IX. 89.2; *opt.* Hdt. VI. 69.5, VII. 194.2. ἐπίσταμαι—*ind.* Hdt. II. 173.2, III. 146.2, VI. 13.1, VII. 218.3; *opt.* Hdt. II. 152.3, III. 71.2, V. 92.31, VII. 18.2; Thuc. VI. 74.1. ἀκούω—*ind.* Hdt. VII. 35.1, 238.1, IX. 95; *opt.* Hdt. VII. 208.1, VIII. 79.2, Soph. Ph. 549sq. μανθάνω—*opt.* Hdt. III. 21.2, 32.2, 64.2, VI. 69.3, VIII. 65.6, 93.1. πυνθάνομαι—*ind.* Hdt. VI. 135.2, VIII. 50.2, 109.1; Thuc. III. 29.1; *opt.* Hdt. I. 96.3, III. 140.1, 154.1, V. 86.4, VI. 41.3, VII. 196.1, 239.1, VIII. 57.1, 136.1; Thuc. II. 57.1.

Ergativity in the History of Persian

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This paper consists of four sections: 1. The problem, 2. The Old Persian tense system, 3. Subject properties, and 4. Conclusion. In this paper I will try to explain the historical role of the Persian genitive-dative case by referring to ergativity.

1. The problem

We begin our discussion by taking a look at the following comparative list of the first person singular free pronouns. (See the next page.)

Avestan first person singular free pronoun has five separate cases: nominative, accusative, dative, ablative and genitive, but Old Persian fused genitive and dative into a single form *mana*.

Among Middle Iranian languages, Khotanese and Sogdian have a separate accusative case *ma* and *t'm'* (and *mn'*), respectively. Middle Persian (and Parthian) has only two cases: the nominative and oblique.

Modern Iranian languages each have one, two or three cases (except Ossetic). As can be seen from the list, in the languages that have more than one case the nominative derives from the Old Iranian nominative, but the oblique derives from the Old Iranian genitive (-dative). Also in languages with only one case, it is the Old Iranian genitive (-dative) *mana* that has survived, and not the nominative *azəm/adam* (e.g. New Persian and Yagnobi). In Ossetic too, which has more than three cases, the cases other than the nominative and the genitive have been built on the genitive *mæn*, e.g. dative *mænæn*, allative *mænma*, *mænma*, ablative *mænæj* (Abaev 1964:23).

The above list shows that the genitive (-dative) has been very important alongside the nominative in the history of Iranian. This is further borne out by the following facts. First, the New Persian plural suffix *-ān* is known to have derived from the Old Persian genitive-dative suffix *-ānām* (e.g. New Persian *mardān* 'men' from Old Persian *martiyānām*, cf. Darmesteter 1883:124). Second, the Middle Persian second person singular pronoun *tō* (written as *tw* in Manichean texts, as *LK* in Pahlavi texts) can be shown to have derived from the Old Persian genitive-dative singular **tava* (unattested in the Old Persian inscriptions, but attested in Avestan), and not from the nominative *tuvam* (Meillet 1900:254). Third, it is suggested that the Taleshi (a modern Iranian language) oblique case suffix *-i* (e.g. *šahar-i pašō*, lit. country-obl. king-nom. 'king of the country') derive from the Old Iranian genitive singular suffix *-ahya* (Oranskij 1977:154).

		nom.	acc.	gen.	dat.
Old Iranian	Avestan	azəm	mām	mana	maibyā
	Old Persian	adam	mām	mana	mana
Middle Iranian	Khotanese	aysu	ma	mamā	mamā
	Sogdian	'zw	t'm', mn'	mn'	mn'
	Mid. Persian	az, an		man	
		nom.	obl.	possessive	
New Iranian	New Persian	man			
	Kurmanji	āz	mbn		
	Semnani	a	mu, mura		
	Taleshi	āz	mъ, mъnī		
	Balochi	man	manā, manārā	m(a)nī	
	Gilaki	mən	məra	mi	
	Mazandarani	men	mere	me	
	Mukri	ambn	mbn		
	Pashto	zə	mā		
	Munji	zā, zə	mən, mun		
	Shugni	wuz	mu		
	Wakhi	(w)uz, wəz	maʃ	ʒə, ʒbl	
	Yognobi	man			
	Ossetic	az	mæn(gen.)		

(Cf. Pirejko et al. 1978: 129 and Efimov et al. 1978: 218)

In the following I will try to explain syntactically why the genitive-dative was generalized as the oblique case to the exclusion of other cases, focusing on the transitional period from Old Persian to Middle Persian.

2. The Old Persian tense system

As was convincingly shown by Cowgill (1968:268), 'Old Persian is essentially on the Middle Iranian level as regards the inherited Indo-Iranian aspect system, with only one set of finite forms, those made from the present stem, in real use' (cf. also Lazard 1976:184f.). That is, in the indicative mood, Old Persian has two tenses: the present and imperfect (past), e.g. *°barāmi* (in *pari-barāmi* 'reward') and *abaram* 'I carried.' Alongside these synthetic forms, there appear such periphrastic perfect constructions as in *ima taya mana kṛtam* 'this is what I have done.' As was shown by Benveniste (1952), this controversial expression cannot be passive (see also Noda 1982), but an active perfect in the form of a possessive construction. In my view this perfect transitive construction together with a perfect intransitive sentence forms an ergative system. (Note that in the following we will use S, A and O for 'intransitive subject', 'transitive subject' and 'transitive object', respectively, cf. Dixon 1979). Consider the following

examples:

- (1) *hammiçyā* *hangmatā* *paraitā* *patiš* *l'wānam* (DB 3.65)
 rebel (S)- assembled- came out- against Virana
 nom.pl.m. nom.pl.m. nom.pl.m.
 'The rebels assembled (and) came out against Vivana.'
- (2) *uta-mai* *anyašci* *vasai* *asti* *kṛtam* (BD 4.46-47)
 and-1sg.gen.(A) other(O)- much is done-
 nom.sg.n. nom.sg.n. nom.sg.n.
 'And I have done much else.'

In 1 the S NP *hammiçyā* is put in the nominative, and the verbs *hangmatā* and *paraitā* agree in gender and number with the S NP. In 2 the A NP *-mai* is put in the genitive-dative, and the O NP is put in the nominative (=accusative). The verb *kṛtam* agrees in gender and number not with the A NP, but with the O NP. The auxiliary *asti* also agrees in number and person with the O NP. Thus, it is clear that as regards nominal morphology and verb agreement the Old Persian perfect shows ergativity, i.e. S and O are treated alike, while A is treated differently from them.

Thus, the Old Persian tense-aspect system in the indicative mood is:

present	° <i>barāmi</i> 'I carry'
imperfect	<i>abaram</i> 'I carried'
perfect	* <i>mana/-mai bṛtam</i> 'I have carried (it)'

It must be noted here that present and imperfect are of accusative type, while perfect is, as we have just seen, of ergative type, i.e. we have here a split conditioned by tense-aspect of sentence as to accusative-ergative dichotomy. Now judging from the situation in Middle Persian (cf. Noda 1980), it seems that with the passage of time the imperfect was abandoned in favor of the perfect, i.e. the perfect became a simple past. Thus the late Old Persian verbal system comprised only two tenses: the present (of accusative type) and the past (of ergative type) as in Middle Persian. E.g.

	Late Old Persian	Middle Persian
present (accusative)	° <i>barāmi</i>	<i>baram</i>
past (ergative)	* <i>mana/-mai bṛtam</i>	<i>man/-m burd</i>

It is on this late Old Persian two-tense system that the following discussions are based.

3. Subject properties

It has been suggested by Keenan (1976) that 'subject' is not a single unified concept, but rather consists of several properties. These properties include coding properties (e.g. nominative case marking, and control of verb agreement)

and behavioral properties (e.g. control of reflexivization, and deletability under coreference). It has further been shown by Ziv (1976) and Cole et al. (1980) that the 'multifactor' concept of subject is valid not only for synchrony but also for diachrony. I will show below that this idea is useful for an understanding of the history of Persian.

First let us look at the present tense.

- (3) *adam xšāyaθya ahmi* (DB 1.12) 'I am king'
 1sg.nom.(S) king-nom. 1sg.
- (4) *ima xšačam dārayāmi* (DB 1.26) 'I hold this kingdom'
 this- kingdom(O)- hold-1sg.
 acc.s.g. acc.sg.

In 3 the S NP *adam* is in the nominative and controls verb agreement. In 4 also verb agreement is with the (here missing) A NP (*adam*). As for behavioral properties consider the following. (Due to the lack of suitable examples in the present tense, we will use imperfect forms. This is justified because the present and imperfect are the same as far as stem formation is concerned.)

- (5) *pasāva adam kāram frāišayam. Vidr̥na nāma Pārsa*
 then I(A) army(O)- sent-1sg.impf. Hydarnes name Persian
 acc.sg.
- mana bandaka avam-šām Ø maθištam akunavam* (DB 2.19–20)
 my subject- 3sg.acc.- (I) chief(O)- made-
 nom.sg. 3pl.gen./dat. acc.sg. 1st.impf.

'Thereupon I sent forth an army. A Persian by name Hydarnes, my subject—him I made chief of them.'

In 5 the second occurrence of *adam* (indicated by Ø) is omitted under coreference with the first *adam*.

Now let us look at the perfect. Examples 1 and 2 are repeated here as 6 and 7.

- (6) *hammičyā hangmatā paraitā patiš Vivānam*
 nom.pl.m. nom.pl.m. nom.pl.m. acc.sg.
 'The rebels assembled (and) came out against Vivana.'
- (7) *uta-mai anyāšci vasai asti kṛtam*
 1st.gen.-dat. nom.sg.n. much 3sg.pres. sg.n.
 'And I have done much else.'

In 6 the S NP *hammičyā* is put in the nominative and controls verb agreement (*hangmatā* and *paraitā* agree with *hammičyā* in gender and number). In 7 the A NP *-mai* is put in the genitive-dative and the O NP *anyā s-* is put in the nominative (=accusative) case. The participle *kṛtam* agrees with the O NP in

gender and number. Similarly, the auxiliary *asti* also agrees with the O NP and is put in the third person singular, and not first person singular. Thus, in a transitive perfect sentence, the subject coding properties, i.e. nominative case marking and the ability to control verb agreement rest on the O NP. Now here is an example of coreferential deletion:

- (8) *uta-mai anyašci vasai asti krtam,*
 and-1sg.gen.-dat. other-nom.sg.n. much is done-sg.n.
ava-Ø ahyāyā dipiyā nai nipištam (DB 4.46-47)
 that(O) this— inscription- not written-sg.n.
 loc.sg. loc.sg.
 'And I have done much else, (but) I have not inscribed (lit.
 written) it (*ava*) in this inscription.'

In 8 the second occurrence of *-mai* (indicated by Ø) is omitted under co-referentiality to the first *-mai*. Thus at least one subject behavioral property (deletability under coreference) is on the A NP. Due to the limitations of the corpus, examples are very scanty, but we can sum up our results tentatively in the following way:

		behavioral property	coding properties
present	intransitive	S	S
	transitive	A	A
perfect	intransitive	S	S
	transitive	A	O

4. Conclusion

As we saw above, subject consists of coding and behavioral properties. However, individual subject properties are not on a par with one another, but rather hierarchically organized (cf. Keenan 1976:324). Thus, among coding properties verb agreement is higher than nominative case marking on the (implicational) hierarchy. That is, if an NP has verb agreement, then it has also nominative case marking, but not vice versa. The same holds true for the relation between coding properties and behavioral properties. As was shown by Cole et al. (1980) in the acquisition of subjecthood, behavioral properties are acquired earlier than coding properties. That is, behavioral properties are higher than coding properties on the implicational hierarchy. Thus, an NP which has behavioral properties can be regarded as more basic, i.e. more subject-like than one with coding properties.

To return to Old Persian, in a present transitive sentence coding properties and behavioral properties converge on the A NP. On the other hand, in a perfect transitive sentence subject properties are distributed on two separate

NP's, behavioral properties being on the A NP and coding properties on the O NP. That is, a late Old Persian transitive sentence has behavioral properties on an A NP both in present and past (perfect) tenses. Accordingly, the nominative which functions as A in the present (accusative type), and the genitive-dative which functions as A in the past/perfect (ergative type) are the two likely candidates for surviving into Middle Persian.

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Word Frequency and Lexical Diffusion in Modern English Shortening*

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In all cases of lexical diffusion, we find leaders and laggards among the words, which raises the issue of what factors determine these schedules. Slowly some of the factors are being sorted out. One such factor is word frequency. In the present paper we will clarify how the interaction between phonetic factors and word frequency determined the schedules of the shortening of ModE long vowels.

The data are based on Dobson (1968) and the frequency counts on Spevack (1973). The shortening of ME \bar{o} was very common before [d], and fairly common before [v] in the 16th century, and before [t, θ, k] shortening became operative in the former half of the 17th century, and within a given consonant, the vowel in the more frequent words and the vowel preceded by [C1] changed first. The shortening of ME \bar{e} was very common before [d] in the 16th century, and then this change gradually spread before [t, θ] in the 17th century, and lastly before [f], and within a given consonant, the vowel in the more frequent words changed earlier than that in the less frequent words. The shortening of ME \bar{e} occurred in the same environments as those of ME \bar{e} , and it corresponded roughly to an increase in word frequency.

Dobson (1968, §24) suggests that shortening is "essentially due to a general tendency to shorten the vowels of closed syllables," though "the consonants differ in their power to cause it." We assume that the first factor that motivated the change is this temporal compensation between the vowel and the following consonant in monosyllabic words. In ModE shortening this temporal compensation took place earlier before dentals than before labials or velars, and before a voiced consonant than before a voiceless consonant. House and Fairbanks (1953) find that English vowels are generally longer before dentals than before labials or velars, and the voicing of a postvocalic consonant strongly affects the duration of a preceding vowel. By assumption, the greater is the deviation from the domain of monosyllabic words, the earlier the temporal compensation takes place. Thus we may suppose that in ModE shortening the temporal compensation in monosyllabic words took place earlier in the longer words, i.e., the vowel between voiced dental consonants or before a voiced dental consonant. Then, within a given consonant, word frequency is an important factor. When we look at the vowel before [d], or [t], and so on, the high

* I wish to thank William S.-Y. Wang for much stimulation and helpful suggestions.

frequency words changed earlier than the low frequency ones.

From the above observation, we may say that the ModE shortening was actuated by the interaction of the temporal compensation, phonetic environment, and word frequency. When all these factors were operative in one word, the long vowel had a very high probability of shortening.

As how early temporal compensation took place depends on the phonetic environment, probability of shortening $P(S)$ can be formalized as the sum of phonetic environment $P_1(E)$ and word frequency $P_2(F)$:

$$P(S) = P_1(E) + P_2(F), \quad 0 \leq P(S) \leq 1$$

where P_1 and P_2 are weight given to each factor. For the verification of this formula, we give the value .4 and .5 to P_1 and P_2 respectively. For E , the value

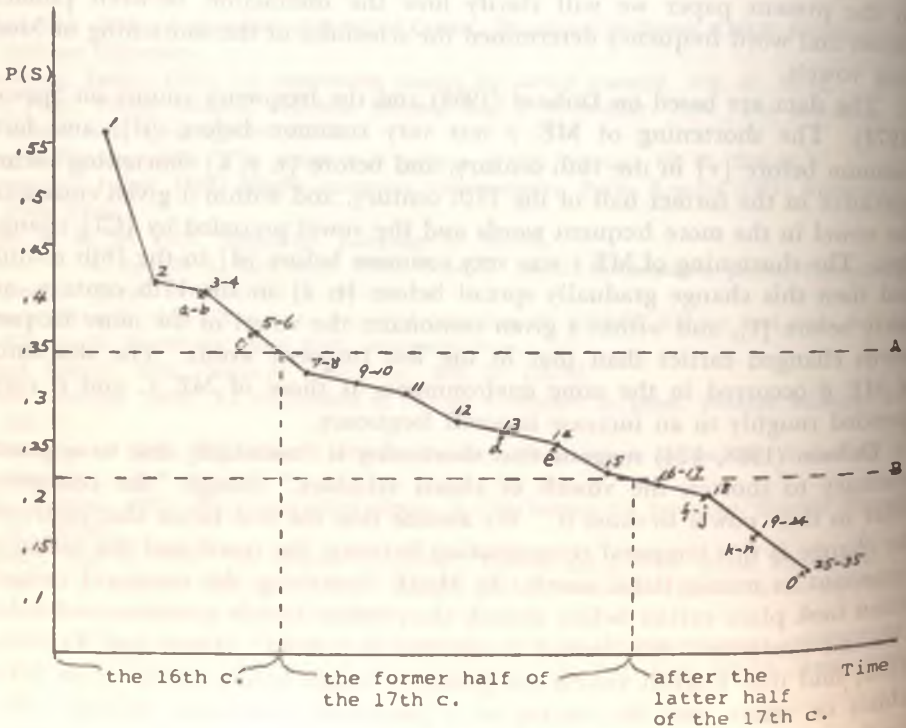


Figure 1 Probability of Shortening of ME \bar{o} and \bar{e} Words

ME \bar{o} words:

- 1 blood, 2 flood, 3 good, 4 fool, 5 (ap)prove, 6 whom, 7 glove, 8 look, 9 foot, 10 (re)move, 11 stood, 12 took, 13 (for)sooth, 14 food, 15 book, 16 root, 17 womb, 18 mood, 19 boot, 20 shoot, 21 soot, 22 tooth, 23 behove, 24 hove, 25 brook, 26 shook, 27 struck, 28 cook, 29 forsook, 30 hook, 31 nook, 32 rook, 33 crook, 34 roof, 35 hoop

ME \bar{e} words:

- a dead, b head, c death, d beat, e tread, f spread, g stead, h bread, i shred, j lead(n), k sweat, l threat, m wheat, n eat(pt), o deaf

is given as follows: dental .2, velar .1, labial .1, voiced .2, voiceless .1. For *F*, the frequency counts are grouped and the value is given as follows: 0-0.0050 .1, 0.0051-0.0100 .2, 0.0101-0.0150 .3, 0.0151-0.0200 .4, over 0.0200 .5. The diagram of the probability of shortening of ME *ō* and *ē* words is given in Figure 1. This diagram shows that the lower the *P(S)*, the later the shortening. The lines A and B show the thresholds of shortening in the former half of the 17th century and the latter half of the 17th century respectively.

Lastly the actual data from the 16th century to the 20th century are compared with the probability of shortening that our formula predicts. Our formula can predict the development of eModE *ū* words 91.1% in the 16th century, 83.3% in the former half of the 17th century, and 90.3% in the latter half of the 17th century, and 91% after the 18th century.

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Graphemischer Figuralismus und die Methoden der historischen Phonologie

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(1) Schreibungsmaterial und historische Phonologie

Die Schreibung ist die Hauptquelle für die synchronische und die diachronisch-vergleichende Phonemanalyse von Texten aller Zeiten; das gilt besonders für Sprachen mit alphabetischem Schreibungssystem, d.h. mit Einzelzeichen (Graphemen) als Zeichen für Laute. Grapheme sind Teile eines Graphemsystems mit graphischen Varianten je nach Stellung, Funktion (Verwendung), Texttype u.dgl. Nach der Theorie römischer Grammatiker vereinigt ein Schriftzeichen *figura* (Gestalt, Form) und *potestas* (Lautwert). Erfassung der *figurae* mit ihrer Variation muß der erste Schritt der Analyse sein, ein schreibungspositivistischer Schritt, der die Erkenntnis des traditionellen Lautwertes miteinschließt. Aber jedes Graphem ist auch Teil eines Schreibungssystems, in dem die gegenseitigen Unterscheidungserfordernisse die Form beeinflussen, ebenso wie die Einzellaute Teile eines Lautsystems oder Phonemsystems sind. Die Auffassung von Einzelzeichen als Teile des Systems bringt eine schreibungsstrukturalistische Einstellung als zweiten Schritt der Schreibungsanalyse, dem auch eine mehr graphophonemische als graphophonetische Deutung entspricht. Figuralismus ist für uns das allzu starke Hervorheben des graphischen Faktors in Einzelzeichen, d.h. auch die Annahme von allzu weitgehender Konstanz des Lautwerts. Daraus ergibt sich synchronisch die Überbewertung von bloß graphischer Variation, diachronisch das Nichterkennen einer Zeichenumwertung. Figuralismus bedeutet die Analyse von Schriftzeichen ohne viel Rücksicht auf die Struktur des Gesamtsystems und der Schreibermotivierung bei der Zeichenwahl.

(2) Graphemwandel und Phonemwandel

Es gibt Schreibungswandlungen, deren Bedeutung nur auf graphischer Ebene liegt, z.B. der altengl. Wandel von <th> zu <þ> (dem Runenzeichen), der Wandel von ae. <u> zur frz. Digraphie <ou> im Mittenglischen, der Wandel von ahd. -hh- im Inlaut zu -ch- u.dgl. In diesen Fällen ist eine rein "figuralistische" Analyse der Forscher ausreichend.

Figural läßt sich eine Typologie von Schreibungswandlungen (Zeichenwandlungen) aufstellen, z.B. rein graphisch Modifizierung von Schriftzeichen, Schwund oder Hinzufügung von diakritischen Zeichen auf oder über (unter) der Zeile, Zeichenersatz, oder "graphophonemisch", d.h. mit Berücksichtigung

des Phonemwertes Zeichenzusammenfall, Zeichenspaltung, Zeichenschwund usw.

Es hat sich gezeigt, daß gewisse (figurale) Typen von Graphemwandel und von Phonemwandel eine enge Bindung aufweisen. Phonemverschiebung zeigt sich z.B. durch Graphemverschiebung (Graphemersatz) im Ahd., wenn das frikative <th> oder <dh> (bei dem <h> den Reibelautcharakter bezeichnet) zu <d> wird (*ther*; *der*). Graphemzusammenfall zeigt Phonemzusammenfall, z.B. späthd. <i> und <e> in Nebensilben: *gotis/gotes* 'Gottes'. Phonemspaltung mit Zusammenfall führt zu verschiedener Zeichenverteilung: <e> ersetzt <a> in ahd. *ferit* 'fährt' zu *faran* 'fahren'. Phonotaktische Wandlungen wie Metathesen, Assimilationen, Dissimilationen, Schwund in einzelnen Morphemen werden graphisch stets ausgedrückt, da die Zeichen verfügbar sind: *stemma* neben *stemna* 'Stimme' (Tatian); *umberente* neben *unberenta* 'sterilis' (Tatian), frühnhd. *werlt* (ahd. *weralt*) zu *welt* usw.

(3) Synchronischer Figuralismus

Die gegensätzliche Beurteilung von Schreibungen in historischen Texten seitens der Forschung beruht meist auf schreibungspositivistischer gegenüber schreibungsstrukturalistischer Interpretation. Man könnte sagen, daß "Figuralisten" schreibungskonkret und schreibungspositivistisch und "Strukturalisten" mehr schreibungsrelativisch und schreibungsstrukturalistisch interpretieren. Oft wird im Figuralismus übersehen, daß die beabsichtigten Lautwerte der Zeichen auch "relativ" oder strukturbedingt aufzufassen sind: ein und dasselbe Zeichen <t> kann für den Schreiber nach seiner Aussprache einen dentalen oder alveolaren aspirierten Fortislaut, eine stimmlose Lenis, nichtaspirierte Fortis usw. bedeuten. Es war Otto Bremer (1895), der in seiner berühmten Kritik Georg Wenker und seine Mitarbeiter am Deutschen Sprachatlas daran erinnern mußte, daß die idiolektisch motivierten Schreibungen der Schulmeister nicht figuralistisch, d.h. einfach mit den Lautwerten der Marburger Herausgeber oder ihrer allgemeinen Norm gedeutet werden können.

E. Sievers' "Schallanalyse" (Penzl 1972, §7. 1c) von ahd. und ae. Schreibungsvariation ist tatsächlich ein extremer Fall von Figuralismus; er glaubte z.B. in Isidors Schreibungsvariation von *buohhum* neben *boohhum* direkte Wiedergabe von allophonischer Variation in verschiedenen Intonationsverhältnissen erkennen zu können.

Der Gegensatz zwischen figuralistischer und strukturalistischer Interpretation der Schreibungen zeigt sich bei ahd. Phonemwandlungen, z.B. bei der sogenannten "Medienverschiebung" von vorahd. *b *d *g, bei der ahd. Diphthongierung von *ē *ō, bei der Veränderung der Nebensilbenvokale. Nehmen wir zunächst die Medienverschiebung. Wenn im St. Galler Paternoster um 800 *sculdikem*, *kīp* steht und 200 Jahre später bei Notker von St. Gallen *sculdigen*, *kīb* (mit <k> nach seiner Anlautregel im Wechsel mit <g>), sehen Schreibungspositivisten darin *b>p>b und *g>k>g mit "späthd. Verschlußlautschwächung" (Braune-Mitzka 1963, §102b). Aber Braune (1911) rechnete

schreibungsstrukturalistisch auch mit gleichen Lautwerten der Schriftzeichen und <p>, <g> und <k> (§88, Anm. 2). Für die ahd. Diphthongierung von /e:/ und /o:/ finden wir z.B. in bairischen Namensformen aus Freising: *Freaso* (809), *Freso* (814), *Friaso* (823), *Frieso* (849) (Schatz 1907, §7). Figuralistische Deutung geht von der historischen Wiedergabe der Lautwerte in Diphthongen als Phonemgruppen aus und sieht in <e> /e:/, in <ea> /ea/, in <ie> /ie/ usw. Das gleiche gilt auch z.B. für Otfrids Diphthongschreibungen: *guat*, *gueteme*, *guoto* (Penzl 1971, §8.10), die strukturalistisch als /uə/ zu deuten sind. Auch in Nebensilben wird <o> in *offono* neben *offan* (Isidor), <i> in *sibini* neben *sibun* 'sieben' figural als /o:/, /i:/, strukturalistisch als Dittographie für /ə/ angesehen.

(4) Diachronischer Figuralismus

Diachronische Deutung beruht, wie wir oben (3) gezeigt haben, auf synchronischer Interpretation. Einige Typen von Phonemwandel werden konsequent durch Zeichenwandlungen (Schreibungswandlungen, vgl. (2) oben) wiedergegeben. Das trifft nicht immer auf Phonemverschiebung zu. Während mhd. *i* ü graphisch frühnhd. durch <ei> <au> wiedergegeben werden (mhd. *wîn*, nhd. *Wein*, *Haus*), bleibt im Englischen die me. Orthographie bestehen: *wine*, *house* (mit frz. Digraphie für den Monophthong). Die Diphthongierung wird figural nicht ausgedrückt, weil in der "Great English Vowel Shift" durch die Veränderung der historischen Diphthonge und der Kurzvokale kein Graphemwandel eine deutlichere Bezeichnung der Lautung der historischen hohen Langvokale gegeben hätte. Generativisten, die überraschender Weise in der historischen Phonologie Figuralisten sind, sehen in der "Tiefenstruktur" des Neuenglischen immer noch die alten Monophthonge, weil sich in romanischen Lehnwörtern Wechsel mit Kurzvokal findet (*crime*, *criminal* usw.)

Die Phonemspaltung führt in der Regel auch zu graphischer Wiedergabe, auch wenn kein Zusammenfall erfolgte, aber oft erst viel später nach dem historischen Ereignis. Einige Handbücher und Generativisten beschreiben immer noch den ahd. *i*-Umlaut als einen vor allem mhd. Vorgang, weil erst dann eine etwas häufigere Bezeichnung der Phonemspaltung durch Zeichenspaltung (diakritische Zeichen über *u o o: a* vor Konsonantengruppen) eintrat, obwohl die *i*-Laute in Folgesilben, die den *i*-Umlaut hervorriefen, z.T. frühnhd. (wie *j*), z.T. spätnhd. schwanden oder sich veränderten. Dieser strenge Figuralismus, der Phonemwandel nur bei Zeichenwandel anerkennt, müßte eigentlich dazu führen, auch für das Frühnhd. noch Umlautsmangel anzunehmen, da in manchen Texten bei *u o* systematische Bezeichnung des Umlauts fehlt. Bei Spaltung mit Phonemzusammenfall wie bei dem Umlaut von *a* vor *i* (*ferit* zu *faran*) oder dem Umlaut von *iu* und *u*: finden wir Umlautsbezeichnung, da Zeichen wie <e> (vorahd. *e) und <iu> dafür verfügbar werden. Notker schreibt *hiuser* als Plural von *hûs* mit <iu> als Zeichen für [y:]. Die Annahme wegen der Bezeichnung, daß nur [a] und [u:] Umlauts-

phoneme in ahd. Zeit entwickelten, scheint typologisch absurd und zeigt die Gefahr eines nicht durch strukturelle Erwägungen gemäßigten Figuralismus. Bei allen Phonemwandlungen außer dem Zusammenfall, wo aus zwei vorhandenen Zeichen eines werden kann, müssen wir mit einer Zeichenumwertung oder Schreibungsumwertung rechnen: ein Zeichen kann einen verschiedenen Wert annehmen, ein Zeichen kann einen neuen Wert dazubekommen, ein Zeichen mit Doppelwerten (z.B. <u> mit [u], [u:]) kann einen verlieren oder den zweiten annehmen (Längung des Kurzvokals: *Jugend*, *Kugel*). <ie>, mhd. noch ein Diphthong, z.B. in *hier*, wird frühnhd. in Wörtern geschrieben, wo historisch nie ein Diphthong bestand: *Biene*, *liegen*, *Sieg* usw. Die mitteldeutsche Monophthongierung gibt ein willkommenes Zeichen für [i:], so daß Langvokal und Kurzvokal [i] graphisch gut unterschieden werden können (Penzl 1975, §115).

(5) Figuralismus und Strukturalismus in der Schreibungsanalyse

Unsere kritischen Bemerkungen zum Figuralismus sollen nur eine übertriebene Anwendung des Prinzips der graphemischen Analyse in der historischen Phonologie kennzeichnen. Keine Einzelschreibung kann ohne Einbeziehung des graphischen Gesamtinventars des Textes, graphischer Tradition, graphischer Variation phonemisch oder gar phonetisch mit genauerer Lautangabe interpretiert werden. Es ist oft leicht zu entscheiden, ob eine Verschreibung im Text figuralistisch als Allegroform, Nebenform oder als rein mechanisch und linguistisch irrelevant angesehen werden soll (Sievers, Tatian, §61).

Figuralismus ist als strenger Schreibungspositivismus gegenüber dem "abstrakteren" Schreibungsstrukturalismus nicht an eine bestimmte Schule oder Forschungsrichtung oder an einzelne Forscherpersönlichkeiten gebunden. Es gibt auch wohl kaum einen Forscher, der nur Figuralist oder nur Schreibungsstrukturalist ist. Schon die Junggrammatiker haben figuralistisch und strukturalistisch gearbeitet. Man beachte z.B. die Interpretation des alem. <f> bei Notker: *fād* 'Pfad', *flēgen* 'pflegen'. Braune schrieb 1911: "Während man früher für *das* altalem. eine lautliche wandlung des *pf* > *f* annahm, hält man jetzt *das* alem. *f*, *ff* nur für eine orthographische eigenheit des altalem."... (§131, Anm. 4)

Eine vorbildliche Schreibungsanalyse verlangt figuralistische "Inventaraufnahme", aber dann strukturalistische Interpretation, wobei die graphischen Zeichen als Teile des Graphemsystems mit nur relativ feststehenden oder zu erfassenden Lautwerten anzusehen sind. Im Ahd., das wir für die meisten unserer Beispiele verwendet haben, ist im allgemeinen eine genaue grapho-phonetische statt einer allgemeineren "graphophonemischen" Deutung der Schreibungen nicht möglich.

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The Dialectal Position of Germanic within West-Indo-European

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In the chapter on the 'Indo-European Foundations' of Germanic in his history of the German language (1978), R. E. Keller states (p. 29):

'The fair number of parallel features which have been discovered to exist between Celtic and Italic (or some language of the Celtic group and some of the Italic group), between Slavic and Baltic, Baltic and Germanic, and Germanic and Italic are best seen in terms of isoglosses rather than as closer genetic links.'

This summarizes fairly well the stand currently taken by a growing number of scholars versus Meillet's assumption of 'intermediate units' like Balto-Slavic or Italo-Celtic between predialectal Indo-European and the individual Indo-European language groups. It makes it clear that language-to-language correspondences point to cultural contacts as well as to original closer kinship, and that it behooves us to scrutinize the former before jumping to conclusions about the latter. If we leave out the still unsolved controversy about assigning both Latin-Faliscan and Osco-Umbrian to a single common language or deriving them from two separate proto-languages, and therefore avoid discussing the Italo-Celtic hypothesis, and if we do not engage into the ongoing debate on the assumed original Balto-Slavic unity, it becomes possible to take a less biased look at the correspondences that link Germanic with Latin, Osco-Umbrian, Celtic, Baltic, Slavic and even Venetic.

Most of these correspondences are lexical isoglosses, though reference is made to some phonological and morphological parallels, such as the *-m-* endings of the dative (instrumental) plural in Germanic and Balto-Slavic, which Pisani tried to identify in Venetic *trumus* as well—a further comparison which cannot be maintained, as Ven. *trumus* presumably reflects **(k*)tru-* '4' + the suffix *-mo-* of the ordinals (expanded into the theonymic epithet *trumus.iiati-* in Lagole).¹ We could also mention the parallelism in the *-n-* stems for Germanic and Italic, though there are quite a few differences in the further elaboration of the inflectional paradigms. In the field of word-formation, one could point to the distributive numeral in *-no-*, e.g., Lat. *binī*: ON *tvennr* < **twisno-* 'twice', or to the locative adverb in *-nē* indicating 'from where', e.g. Lat. *superne* 'from above', Goth. *innana* 'from inside'. The occurrence of the

1) For a discussion of these Venetic forms, cf. Pisani, 1964: 267-70; Pellegrini-Prosdociini, 1967: II, 183-7; Lejeune, 1974: 85, 102, 145.

reduplicated form of the pronominal **selbho-* both in Germanic and Venetic may also be significant syntactically, though the context in which it appears only once in Venetic does not provide any clear clue as to its actual usage. In the field of phonology, may it suffice to mention the parallel treatment of *-tt-* in Germanic, Italic, and Celtic. In his recent paper on 'The position of Germanic within the Indo-European languages' (*Folia Linguistica Historica* 1, 1 [1980], 117-123), Witold Mańczak lists 18 such correspondences from earlier scholars (Krahe, *Sprache und Vorzeit* [1954] and Georgiev's *Introduction to the History of the IE Languages* [Russian ed., 1958], but, as always, the bulk of his material is lexical (using Wulfila's bible translation he identifies in parallel passages 643 Gothic-OCS correspondences versus 362 Gothic-Latin).

The method of Mańczak is characteristic of those who approach the problem on a purely statistical basis: elsewhere, the Polish scholar compared similar fragments in the Gothic Bible, the Old Church Slavonic Codex Marianus and Chyliński's XVIIth-c. Lithuanian Bible; the outcome was that 301 Lithuanian-Gothic lexical correspondences were identified versus 740 OCS-Gothic. On that basis, he concluded that 'Germanic is first of all related to Slavic, then to Italic and Baltic, and finally to other Indo-European languages.'

This kind of approach calls for a number of theoretical comments:

- (a) Is relationship between languages to be based on mass comparison of vocabulary, without discriminating between the grammatical categories to which the terms belong, between inherited common vocabulary and possible loans—counting each inflected form as a separate lexical item and assigning a statistical index to it? Are we, in other words, really measuring the degree of kinship between Gothic and Latin by stating that the 3rd sg. optative Goth. *sijai*: Lat. *sit* and the 3rd plur. optative *sijaina*: Lat. *sint* each occur 6 times in the fragment under consideration while the 1st sg. *sijau* (: Lat. *ero*), 2nd sg. *sijais* (: Lat. *esto*) and 2nd plur. *sijaiþ* (: Lat. *estote*) only appear once in it. And how much do comparisons like Goth. *waldufni*: OCS *vlasti* 'power' or Goth. *weitwodjan*: OCS *sŭvēdētelistvovati* 'bear witness' tell us about the actual closeness of the relationship between Slavic and Germanic when the whole derivation process involved is completely different? And why would Goth. *mizdo*: OCS *mizda* 'salary' make Germanic closer related to Slavic than to Greek (*misthos*) or Avestan (*niždəm*)? The large majority of Mańczak's comparisons consists of inflexional forms, root correspondences with fundamentally divergent word-formation patterns, terms shared by a considerable number of other IE languages or even approximate loan translations on the same model, such as Goth. *batizo ist*: OCS *uněe jesti* versus Lat. *expedit*. I fail to see how this proves that Slavic is closer to Germanic than any other languages.
- (b) If lexical material is the essential source to assess the position of Germanic within the Indo-European languages, can this be done on the basis of isoglosses, i.e. selected linguistic elements which point to

one-to-one relationships or areal connections? Or should one examine the whole vocabulary? Mańczak undoubtedly has a good point in rejecting isolated comparisons, but I don't think that a proper diachronic perspective will be gained by dismissing 'words of different origin' like Goth. *siþoneis* 'disciple' versus OCS *učēnikū* as well as words shared by OCS, Gothic and Latin, to concentrate on two types: those occurring in Gothic and OCS, but not in Latin (e.g. *sunus*: *synū*, but *filius*), and those occurring in Gothic and Latin, but not in OCS (e.g. *wiljan*: *velle*, but *chotēti*)—completely disregarding the appearance of cognates of the same in other IE languages. What we need is an etymological inventory of Germanic—not simply an approximate percentage figure of the material inherited from Indo-European, as A. Jóhannesson attempted to give several decades ago for Icelandic, but a detailed listing of the components of the vocabulary, similar to the inventory J. Vercouillie supplied for Dutch in his *Etymologisch Woordenboek der Nederlandsche Taal* (Ghent, 1925): his listings divide the vocabulary into (a) inherited material; (b) loans. The former contains, e.g., all the terms represented (1) all over the Indo-European territory; (2) only in the western part of the IE area; (3) only in Italic, Celtic and Germanic; (4) only in Northern Europe (i.e. Germanic and Baltic; Germanic and Slavic; Germanic and both Baltic and Slavic); (5) only in Northern Europe and in Celtic; (6) only in Northern Europe and in Italic; (7) only in Italic and Germanic, (8) only in Celtic and Germanic; etc. Similarly, loans from Celtic, Italic, Slavic, Greek, the Romance languages, etc. are organized in subgroups according to their origin.

Only such an approach will enable us to indicate how closely Germanic is related to other languages and to define the diachronic pattern of the cultural affinities between them. In this context language-to-language correspondences come, indeed, to stand in a different light: admitted that Indo-European was already a dialectally diversified language, some of the principles of dialect-geography have apparently been applied to the spread of its vocabulary with valid reasons, not only by the representatives of the Italian *neolinguistica*, but also by A. Meillet, J. Vendryes, W. Porzig and others. Thus, the lexical correspondences between Italo-Celtic and Indo-Iranian, especially in the field of religion, have long been recognized as typical archaisms of 'marginal' languages, these groups being situated as both ends of the Indo-European speech-community. Isoglosses of this type reflect preservation of common stock lost elsewhere, and it is possible to list in the same way characteristic correspondences between Germanic and Indo-Iranian, e.g., in the field of religion, Vedic *Dhīśānā* (a goddess personifying the motherly power of breast-feeding): ON *disir* (female deities supporting the clan by promoting the fertility of its women—also identified with the dead female members of the kin); Vedic *asuraḥ* 'powerful; lord': Germanic **ansuz* > ON *ǫss*, *ōss*, *āss* 'Ase' (designation of the sovereign

and warrior gods)—as 'endowed with vital energy' (cf. Hitt. *ḫaššu-* 'king': *ḫašš-* 'beget, bring forth'); O. Ind. *Manu(s)-* (ancestor of mankind): Germanic *Mannus* (founder of the Germanic people according to Tacitus). Particularly important are words belonging to the technical fields: two of the oldest crafts of the Indo-European are illustrated, for example, by Skt. *āra*: OHG *ala*, ON *alr* 'awl' and Skt. *aniḥ* for **arṇi-* <**elni-* ~ **{ni-* in Gmc. **luni-*> OHG *lun* (derived form, OS *lunisa*, MDu. *luns*) 'linch pin'—terms relating to the trade of the harness-maker and of the cartwright, just as Skt. *takrām*: Icel. *þél* < PGmc. **þenxla-* 'buttermilk' points to the importance of animal husbandry. On the other hand, the technical vocabulary may also give hints as to the time period in which the correspondences due to shared cultural development belong: thus, Chr. Stang has tried to show that the oldest Slavic-Germanic isoglosses definitely point to a period preceding the use of metals; on the contrary, those between Germanic and Italic, and especially Germanic and Celtic reflect a technologically more advanced culture. It is, however, difficult in many cases to determine what is genuine common heritage, either as regional IE archaism or as shared innovation or as localized survival of an older language stratum. It is obvious that the term for 'iron' designated something new in the earliest Hallstatt culture in the latter part of the 2nd millennium B.C., but where did it come from? The alleged Illyrian origin, advocated in the thirties by J. Pokorny, is no longer admissible in the present state of our knowledge about 'Illyrian' and the Lusatian civilization; the etymologies which make it the 'sacred metal' (+ Gk. *hieros*, Skt. *iṣirá-* <**iṣaros* 'powerful, holy' -or- + Italic/Etruscan **ais-* 'god' [Benveniste]) remain unconvincing. The Gmc. term **isarna-* corresponds to Celtic **isarno-*, but the often postulated borrowing of the Germanic word from Celtic is not documented by the facts—it is not established, indeed, that the Germanic people would have taken over the technique of iron metallurgy from the Celts.² Even for loans it is furthermore often difficult to determine the direction of the borrowing process: Lat. *brāca* 'breeches' is borrowed from Gaulish, but is the Celtic word borrowed from Germanic before **ā → ō*, but after the shift of **g* to **k* (as the assumed etymological link with Latin *suffragines* 'hock (of animal)' would imply) -or- did Germanic receive it from Celtic after the consonant shift but before **ā → ō* (since Gaulish has no *ō*). Archaeological data are inconclusive: the earliest finds of breeches in Germanic territory date back to the beginning of our era, but, then, such material is by nature very perishable. The *brāca* as typical Gaulish garment is reported only in Southern Gaul, where the *Narbonnensis* was nicknamed *Gallia brācata*. This makes direct contact of the Celts with the Scythian or Thraco-Cimmerian culture, as Birkhan suggests, less plausible, and DeVries' hypothesis that the *Volsce* and *Tectosages* brought it to Southern Gaul from Moravia, where they were in close contact with the Germanic people, is definitely more attractive. That

2) On the etymology of the name of 'iron', cf. especially Birkhan, 1970: 128–41. Cf. also deVries, 1971: 279; Devlamminck-Jucquois, 1977: 107.

the raising of *ā* to *ō* is rather late in Germanic is evidenced by Lat. *Dānuvius*: OHG *Tuonouwa* < Gmc. **Dōnauwa* (G. *Donau*) and (Caesar) *Bācenis* (forest near Fulda): OHG *Buochunna* < **Bōkonía* 'beech-forest'.³

Celto-Germanic isoglosses are, however, often recent correspondences due to borrowing with the relevant technique, e.g. Germanic **lauða*- 'lead': MlR. *luaide*. Birkhan has indeed shown most convincingly that Oswald Szemerényi's objection that 'the first century A.D. would be a late date for the Germanic people to become familiar with the metal' is irrelevant: they had actually known the metal since the Bronze Age as it appears on the handles of some swords of that period to 'equilibrate' them, but soldering came much later—with a new term for the metal. Sometimes complex phonological problems are involved in tracing back the borrowing: thus, in the same semantic field of metalwork, OHG *wiara* 'purified gold', ON *víravirki* 'filigrane', OE *wīr* 'metal wire' (as well as the ornament made of it) reflect derivations from the Indo-European theme **weyr*- 'curved' through various Celtic underlying forms: in insular Celtic **ei* was monophthongized to **ē* and **wēros* 'crooked' is reflected by OIr. *fiar*, W gwyr, Bret. *gwar*; on the mainland, two dialects treated **ei* differently: (A) **ei* → **ē*, hence *wēros* 'ornament' → Gmc. **wēra*- (OHG *wiara*); (B) **ei* → **i*, hence *wīros* 'ornament; curve' → Gmc. **wīra*- (OE *wīr*); in the Ø-grade, **wīrya* 'torques' → Latin *viriae* 'kind of bracelet'; [personal name] *Viriatu*s (the continental Celtic form and the late Latin borrowing have survived in the Romance languages). Some apparently old loans like Gaulish *sāpo* (Plinius), generally assumed to reflect Germanic *saipō* 'lye, soap', imply sound changes that are not properly documented for the period of the borrowing (Inguaeonic **ā* < Germanic **ai* cannot date back to the first century A.D.).

In other cases, much hinges on the etymology: thus, the Germanic word for 'leather' (OHG *leder*, ON *leðr*), whose only correspondents are Celtic (OIr. *lethar*, W *lledr*), could also be part of the LaTène culture loanwords, provided its Celtic prototype can be derived from the IE root **pel*- (: Lat. *pellis* 'hide, skin', Gk. *pella*) with the instrumental suffix *-tro*-. DeVries considers this etymology as very doubtful ('sehr fragwürdig'), but alternate solutions are not any better, e.g. Loewenthal's connection with Latin *lēvis* in the meaning 'smooth', referring to the treated hide. In this case we would have a Germano-Celtic 'altes Erbwort'—but the technique might be older, perhaps taken over from the pre-Indo-European inhabitants of Northwestern Europe with the term that designated the products?⁴

The investigation of the Celto-Germanic correspondences shows various types among them:

1. Some may reflect

- (1) Common preservation of archaic Indo-European material, e.g. IE **luh₂yom* 'juridical bond', with different semantic development in Celtic (OIr. *lu[i]ge*, W *llw*, Bret. *lê* 'oath') and Germanic (Goth.

³) DeVries, 1960: 71; Birkhan, 1970: 247-8.

⁴) DeVries, 1960: 71; 1971: 387 (with discussion of J. Loewenthal, *PBB* 53 1929, 462).

liuga 'marriage' (borrowing of the Gmc. term from Celtic is excluded by the fact that the concept of 'oath' is alien to the Germanic form of marriage);

- (2) Common preservation of pre-Indo-European material—an always disputable assumption, as appeared from the discussion of their term for 'leather';
2. Some may result from common cultural development: this applies in particular to semantic specializations like the use of the IE *nomen actionis* **oytos* 'going' (from the root **ey-* 'go') for 'oath' in Goth. *aiþ*, ON *eidr*, OE *aþ*, OHG *eid*, and OIr. *ēth* (cf. Swed. *edgång* = *Eidgång* 'going to where the oath is to be taken' → 'oath (taking)'). This also applies to the legal term Gothic *arbja*: OIr. *orb* 'heir', both derived with the suffix -*yo-* from the adjective **orbho-* 'deprived' (: Lat. *orbis*), pointing to the deprivation of an orphan minor (cf. the meaning 'orphan' of Arm *orb*, Gk. *orphanós*)—a basic meaning confirmed by Hitt. *harb-* 'sever'.⁵
3. Some are straightforward borrowings, such as OHG *ambaht*, OE *ombeht*, ON *ambati*, Goth. *andbahts* 'servant' from a Gaulish term *ambactis* described by Caesar as designating the members of a chieftain's retinue. However, in a number of cases, these borrowings may be quite recent, and some correspondences to which a high antiquity has been ascribed may be no older than the Viking Age when close socio-cultural contacts between the insular Celtic and the Scandinavian world developed. Thus, before reconstructing a typothetical IE **kwondnā* to account for ON *hugnn* 'angelica (archangelica)' and Ir. *cuinneog* 'angelica silvestris', one might wonder when that medicinal root was introduced in Scandinavia, and by whom and for what purpose!

If we apply the method of areal distribution to Germanic-Celtic correspondences, we find that they reflect the historical facts: the Celts were the last major Indo-European people with whom the Germanic people came into close contact for a prolonged period before the Roman conquest of north-western Europe. The vocabulary involved shows an advanced culture, characterized by an highly developed metallurgy and well-established social and political institutions, suggesting it represents a younger layer than the terms shared by Germanic and Celtic with Italic, for example. These terms reflect the shared features of the social, economic, religious, and political organization of the north-western Indo-European community including the ancestors of the Germanic and Celtic tribes as well as those of the tribes that invaded the Italic peninsula in successive waves in the second millennium B.C., i.e. the ancestors of the Osco-Umbrian and Latino-Faliscan peoples. Their agricultural technique, which included ploughing, is documented by the reflexes of **prka* 'furrow' in Latin *porca* 'ridge between two furrows', Gaulish **rica*, preserved in French

⁵) This etymology was first proposed by Polomé, 1954: 159–60, and suggested independently by Benveniste, 1962: 11–12 (cf. Szemerényi, *BSOAS* 27 1964, 158). See further, Tischler, 1977: 180.

raie, Provençal *rega* 'furrow, ridge between furrows', Germanic **furhō* (G. *Furche*, E. *furrow*); a common implement was the 'sieve': Lat. *cribrum*, OIr. *criathar*, OE *hrīdder*, OHG *rītera* (G. *Reiter* 'large sieve'). They used the same process to prepare 'butter' as the words OHG *ancho* 'butter' (still *Anke* in some German dialects), Ir. *imb*, Bret. *amann*, but a later change in technique introduced the loanword *butyrum* (from which E. *butter* ultimately derives) into Latin (from Gk. *boutūron* 'curds', actually itself a loan translation from the Scythian of the steppes above the Black Sea), and Lat. *unguen*, Umbr. *umen* came to mean 'fat, ointment'.

To judge from lexical data, it appears that, at that time, the ancestors of the Germanic tribes must have been separated from those of the Celts by those tribes from which the Indo-European peoples of ancient Italy are issued. In their northwestern European homeland, prior to their migration to the south, the latter seem to have been in close neighborly relations with what was to become the Germanic world since they share a set of cultural terms which the Celts do not appear to have. Again, here, the question arises: what is common innovation and what is common heritage, i.e. archaic vocabulary preserved in a marginal area? It is probable, for example, that Lat. *vadum*: ON *vað*, OHG *wat* 'ford' constitute an innovation versus OHG *furt*, OE *ford*: Lat. *portus*: 'harbor' < *'passage' (as in *angiportus*): Gaul. *-ritum* (in place-names), OW *rit* 'ford': Avest. *parātuš*, *pāšuš* 'passage, ford', < IE **pṛ-tu-* (: **pér-tu-* in ON *fiqrðr*). It is perhaps also the case with Lat. *sulcus*: OE *sulh* 'furrow', actually a θ -grade derivation from **selk-* 'drag', also found in Gk. *holkós* 'track', but here we deal with a localized specialization of the meaning as is often occurring, e.g. in Lat. *communis* 'common' < *'who shares the duties' (cf. *munia* 'official functions, duties of a magistrate'): OHG *gimeini* 'common', *gimeinida* 'community, commune' < **moyno-*, a noun derived from IE **mey-* 'exchange', also appearing in different meanings, without the sociolegal connotations of Gmc. and Lat., in OIr. *móin* 'precious object' and Avest. (Gāthas) *maēniš* 'punishment'. Different, however, is the case of Lat. *victima* 'sacrificial animal' to which ON *vē* 'temple, holy place' is compared: what is involved here, is the magico-sacral process by which the object of the cult or the ritual is *separated* from the profane (cf. Skt. *vinakti* 'separates, sieves')—a process indicated in Gmc. by the very Gothic *weihan*, OHG *wihen* 'consecrate' and in Umbrian by the imperative *eueietu*, ordering the ritual selection of the sacrificial victim. In such cases, we may deal with survival of the old ritual language, as is also shown by the correspondence Lat. *sacer* 'sacred': ON *sáttir* 'reconciled, at peace' from the IE root **sak-* belonging to the religious and judiciary vocabulary, with the connotations 'ratify, sanction, establish solemnly' (cf. Lat. *sancio*)—hence, also Hitt. *šaklais* 'law, rule'.⁶

Thus, sorting out the correspondences of Germanic with its neighbors in the west in the field of the lexicon tends to show a diachronic stratification in

⁶ The major part of the evidence presented here has been discussed in greater detail in my earlier papers. Cf. Polomé, 1954, 1972, 1975, 1980, etc.

the closer cultural contacts that the ancestors of the Germanic tribes have had with the adjacent IE peoples: the oldest connections are rather with Slavic and Baltic—with closer links to Baltic to Slavic—but in the second millennium B.C., there seems to have been cultural exchanges in which the ancestors of the Celtic, Germanic and Italic peoples have participated in the agricultural/pastoral community of northwestern Europe, as social institutions developed, new tools were introduced, but ancient tradition persisted especially in law and religion. This community may also have included other IE elements such as the ancestors of the Venetic people, if they were not part of the Italic group initially, and contacts with the eastern group, especially Baltic remained strong, but after the movements southward removed the Italic group from their northern homeland and the development of new metal techniques with the coming of iron provided new tools for territorial expansion and exploitation, the Celts and the Germanic people became close neighbors and cultural influences, in many cases predominantly Celtic, were reflected by new terms, though, located at the westernmost confines of the Indo-European area, Germanic and, to a degree, also Celtic remained very conservative in some parts of their vocabulary. The archaism of the Germanic lexicon, already illustrated by its correspondences with some of the older layers of Indo-Aryan, is also reflected by a number of Germano-Hittite isoglosses, such as the special semantic development of the root **sek**- 'follow' in Hitt. *šak(u)wa*- 'eyes': Goth. *saihwān*, ON *sjā*, OE *sēon*, OHG *sehan* 'see', or Hitt. *liš(š)ai-* 'gather': OHG *lesan* 'gather, pick, glean', ON *lesa*, Goth. *galisan*—also occurring with a divergent meaning in Lith. *lesù, lèsti* 'forage'. Particularly important in this regard is again the religious vocabulary, e.g. Hitt. *mald-* 'recite invocations, promise solemnly': Lith. *meldziù, melsti* 'pray': OHG *meldon* 'announce, reveal', OE *meldian* 'proclaim, announce, declare' (apparently, the term with strong religious connotations has been 'secularized' in Gmc.); Hitt. *talliia-* 'call solemnly upon a god (to do something)': ON *pulr* 'cultural orator' (G. *Kultredner*)—mediator between god and man, communicating with the deity in a special formulaic language.

That the vocabulary of Germanic would show specific archaic features is not surprising: several decades ago, M. Bartoli insisted on the marginal (=archaic) character of Germanic on the basis of his study of isoglosses, and recent developments in IE phonology and morphology, in particular as regards the consonant phonemes and the verb system add new arguments to this thesis. If, indeed, the views advocated by J. Hopper, and by T. Gamkrelidze and V. V. Ivanov, on the nature of the original PIE stops are vindicated, Germanic would indeed be closer to the original system than a number of other IE languages as one could account for the consonant shift by applying two simple rules to the assumed original stops of PIE (Hopper, 1982) in the order:

- (1) [− continuant] → [+ continuant / $\left[\begin{array}{c} \text{C} \\ - \text{low} \end{array} \right]$

$$(2) [+low] \rightarrow [-low] / \left[\begin{array}{c} C \\ +low \end{array} \right]$$

(Rule 1 change /t/ and /d/ [traditionally *dh] respectively to /θ/ and /ð/; Rule 2 changes /t'/ to /t/ [traditionally *d]; the feature [+low] indicates 'glottalization').

As for the verbal system referring to the studies of Erich Neu, Wolfgang Meid and others, it is probable that Germanic represents a stage of PIE prior to the development of the complex mood and tense system reflected by the Greek and Old Indic conjugation systems—presumably a pattern close of the Proto-Anatolian contrasting an 'active' and a 'perfect', to which a middle was added. From the later elaborate system Germanic has indeed only the *optative*. It has apparently *never* had an *aoist*-system; there are *no* traces of the *imperfect*, nor of the *future* tenses, nor any evidence of the *subjunctive*. Under those circumstances, it stands to reason that it is more plausible to assume that the IE group from which Germanic ultimately emerged left the original speech community at an early stage in the diachronic development of the verb system, possibly soon after the Proto-Anatolians. This would also account for the numerous other archaisms that are being identified at all levels of Germanic grammar, as Jean Haudry pointed out in his recent paper in *BSL* 76 (1981), 200, fn. 13.

This also fairly well excludes the ascription of such assumed major innovations of Germanic as the first consonant shift and the simplification of the verb system to a creolization process allegedly after the submission of the pre-Indo-European population of the Germanic territory by its Indo-European invaders. Culturally, the Germanic people seem to have preserved much of the Indo-European heritage, especially in the religious field. If the conquered peasantry of Northern Europe could not influence them deeply culturally, one wonders why they would have had such an impact on their language? The Indo-Europeans seem to have been very keen on preserving and imposing their revered language to the subdued populations—*not* on letting it be adulterated by alien influence, though they were not always successful in preventing it, as the impact of the non-Aryans on Indo-Aryan in India clearly indicates—but, again, this is definitely *not* creolization!

To conclude: Germanic is perhaps the most archaic western IE language.

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Diachronic Syntax and the Revival of Hebrew

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A confrontation of the earliest attested and the most recent stages of Hebrew reveals an interesting diachronic process on the level of syntactic patterning. This process, however, entails a change not of an element or a feature on the *plan de l'expression*, but rather of a function on the *plan du contenu*, in the sense that the syntactic disposition of the sentence parts into thematic and non-thematic or into rhematic and non-rhematic components as well as the distinction of synthetic from analytic and other types of predication are considered as elements of content.

In the description of these processes, we shall desire to underline that they are by no means the result of so-called 'foreign influences' operative in course of the revival of Hebrew in the present century; rather are they internal and natural developments of the sort that might be expected in any typologically related language. Once more, the observation of events that took place in the Hebrew renaissance will be made use of to assess what processes are possible in the history of a language and in diachronic typological syntax, at that,

A large number of Biblical Hebrew sentences contain a pronoun of reference⁽¹⁾ of the form

- (1) *hi*·? f. sg., *hu*·? unmarked gender sg., *hēnā* f. pl., *hēm* unmarked gender pl.;

these morphological gender-number distinctions do not recur with any other morpholexical entity in the language, nor do the cognates of these forms that are found in other ancient Semitic languages function syntactically in exactly the same way. This pronoun commonly goes under the term of "third person", but we doubt the appropriateness of this term for a number of reasons, at least for the "independent", i.e. "nominative" forms here quoted: its membership in the category of person as term indicating the third person may be doubted in the light of Biblical

- (2) *ʔani hu*·?. "It is me." (rare, but cf. Isaiah 41:4, 52:6, Deuteronomy 32:39),

which would involve an infringement of otherwise compulsory person concord; and one might even question the nature of *hu*·? as a pronoun considering

- (3) *Whin-ē-hiw*·?⁽²⁾ *Lēʔā*. "And behold it is (was) Leah." *Kaī lōdōn ēn Leāā*. (LXX) (Genesis 29:25),

where the substitution of the only possible referend for *hiw*[?], namely I.eah, would create a tautological pseudo-sentence, which explains its rendering by *it* or its non-translation in the Septuagint version.

It would appear that *hu*[?], while functioning in other contexts and in certain patterns in the grammatical realm with the force of a noun phrase, fulfils here a function on the enunciative level, that is characterizes the noun forming with it a sentence nucleus as the *rhème* of the latter, or more specifically, as its identificatory or at least synthetic predicate. This is a not altogether uncommon feature: similar statements may be made, e.g., with reference to French *le*, which while frequently acting as object pronoun, functions as pro-predicate in sentences like

- (4) *Je le suis*,

whereas Middle High German *ez* in the often discussed syntactic type

- (5) *Ih bin ez Ioseph*⁽³⁾

is apposited to the predicative nominal to characterize it as such, not unlike what is observed in Biblical Hebrew. The most perfect parallel to Biblical Hebrew *hu*[?] is Coptic enclitic *pe* (m.sg.), *te* (f.sg.), *ne* (pl.) as in

- (6) *Henrōme^I ne^{II} nrefji^{III} maein^{IV} 'Ανδρες^I τερατο^{IV} σκοπο^{III} εισίν*
ʔanše^I mo·pēt^{III+IV} hēmā^{II} (Zechariah 3:8)

which, while etymologically completely unrelated to *hu*[?], may be connected with it on the basis of areal typology.

The function of *hu*[?], which is very much similar to that of Coptic *pe*,⁽⁴⁾ becomes particularly important whenever more than one nominal precedes it:

- (7) *Šeba^I p·ârōl^{II} haṭ.ōbōl^{III} šeba^I šāni·m^{IV} hénā = ΑΙ ἐπὶ τὰ βόες^{II} αἱ^{III} καὶ αἱ*
ἐπὶ τὰ ἔτγ^{IV} ἐστίν + pronoun marking^V the immediately preceding nominal as *rhème*, i.e., as the essential element of the interpretation of Pharaoh's dream. (Genesis 41:26),

or if it is posited between two determinated nominals:

- (8) *Yō·sēp^I hu^{?II} haš·al·i·t^{III}, . . . hu^{?IV} ham·ašb·i·r^V.*
= Ιωσηφ^I ὃν ἀρχων^{III} + element^{II} putting "Joseph" in the foreground +
οὗτος^{IV} ἐπώλει^V. (Genesis 42:6),

while with no nominal preceding *hu*[?] can retain only an anaphoric pronominal function:

- (9) . . . *w^I hu^{?II} ʿāyēp^{III} "as^I he^{II} [was] tired"^{III}* (Genesis 25:29),

where the (grammatical) subject—predicate order is concomitant with conjunctive subordination.

Now it is obvious from a confrontation of

- (10) (way·ēd'u·k·i·) 'ē·rum·im hēm "(and they knew that) they were nude"
(Genesis 3:7)

with

- (11) Šad·i·q¹ hu·² YHWH^{II}. The Lord^{II} is righteous^I. (Lamentation 1:18)

as well as of (2) with (8) that the grammatical subject, finally posited in (11) and initially posited in (8), is not an essential part of a sentence of the type exemplified, but must be regarded as an expliciting apposition precisely as in the syntactically matching expressions

- (12) *Taurus mugit.* = *Mugit taurus.* = *Il mugit, le taureau.*⁽⁵⁾

We are thus establishing that Biblical Hebrew sentences are essentially bipartite, and that tripartiteness as well as a copula status of *hu·²* are apparent only, an impression most likely induced by considering translational equivalents in languages involving verbal copulas; we are furthermore in a position to state that the constitutive components of a considerable portion at least of verbless sentences are *rhème* and *thème*⁽⁶⁾, and that it is these rather than grammatical subject and predicate that in numerous cases determine the sentence order, the *rhème*, remarkably enough, normally preceding.

Before we go on to discuss the diachronic implications arising out of these facts, we have to dwell on the importance of the dependence created by gender-number concord which is observable in all of the examples so far exhibited, whether expanded as (7), (8), (11) or unexpanded as (2), (3), (6). This concord is the formal expression of an equational relation, i.e. identificatory or classificatory. A minimal sentence, such as one created by combining two nouns, which is devoid of concord, is not equational, but descriptive or qualifying⁽⁷⁾, the components being arranged in a topic-comment order; e.g.:

- (13) YHWH 'ēlōhi·m^I 'ēmet^{II} (f.sg.noun), hu·^{2III}—'ēlōhi·m^{IV} ḥay·im^V.
"The Lord God^I [is] [of] truth^{II} (i.e., is verily God), he^{III} [is] a living^V God^{IV}," (Jeremiah 10:10).

The quasi-copula *hu·²* may not be added to sentences of this type without transforming them into an equation-type pattern such as comparable Coptic

- (14) Pek^I·šaje^{II} pe^{IIIIV}·me^V, 'Ο λόγος^{II} ὁ^I σὸς^I (copula^{III}) (definite article^{IV})
ἀληθεία ἐστιν. Thy word is truth. (John 17:17)

I believe the decisive event of syntactic restructuration that occurred at some moment of the history of Hebrew is the loss of pertinence of syntagmatic features which had been distinctive on the level of enunciative functions, i.e., as markers of either *rhème* or *thème*. While in the recent generation, new means came into use to uphold that important distinction, characteristically—from a typological point of view—including clefting and shifting of the rhematic component towards the final segment of the sentence, a reinterpretation

tion necessarily took place of whatever formal oppositions subsisted in current usage. May I be permitted to recall at this point that the fundamental diachronic process in the revival of Hebrew consists in the functional or distributional differentiation of inherited features that have become competing either because of their origin in chronologically distinct layers or because of merger and diachronic neutralization.

The latter being the case concerning the features discussed here, the syntactic types involving two nominals and distinguished by the presence and, respectively, absence of what is now a copula, *hu*?, have undergone reinterpretation. In one case, that is if both nominals are nouns, the construction without *hu*? is interpreted—due to the loss of the rhematizing function of the latter—in the same way as the construction involving a copula: the sentence pattern exemplified by (13) is considered equational even though no concord is present. Thus

(15) *Dvaréxa ?emet.* and

(16) *Dvaréxa hem ?emet.*

are considered as synonymous, irrespective of which component is rhematic, and translatable as "Your words are [the] truth." Curiously enough, certain Biblical expressions, very much *en vogue*, will be currently misunderstood; e.g., the descriptive

(17) *‘ē-nayik yo-ni-m. Oculi tui columbarum.* "Thy eyes are of doves." i.e., "dove-eyes". Thou hast doves' eyes. (RV). (Song of Songs 1:15).

is notoriously taken as classificatory ("Your eyes are doves").

However, a much more interesting process arises for the sentence pattern consisting of a grammatical-subject noun and a predicative adjective. Here, the loss of one function (the enunciative one) is exploited to make way for the creation of a new distinction: a descriptive construction

(18) *Ha-p?ula pšuta.* "The procedure is simple."

(19) *Yosef xaxam.* "Joseph is clever."⁽¹⁸⁾

is in contrast with a classificatory type

(20) *Ha-p?ula hi? pšuta.* "The procedure is a simple one."

(21) *Yosef hu? xaxam.* "Joseph is a clever one." "... a smart guy."

There is also a possibility of distinguishing a general, atemporal from a *hic et nunc* predication:

(22) *Midot ha-xom be-Yapan gvohot.* "Temperatures in Japan are (normally, essentially, characteristically) high."

(23) *Midot ha-xom be-Yapan hen gvohot.* "Temperatures in Japan are high (at present, this year)."

It is in particular the latter, marked term (23) of the opposition which gave

rise to the conception of the copula as a present tense "to be", so that it came to be used also with adverbial predicates:

(24) *Yosef hu? be-Tel'aviv.* "Joseph is in Tel-Aviv."

The pattern subject-noun—copula—predicative adjective or adverb, which is practically non-existent in Biblical Hebrew, would seem to be an analogical expansion out of preexisting relations of quasi-inserted copulas vs. copulaless sentences involving predicative nouns; this innovation might be, after all, a process on the level of expression which consolidated differentiations of types of qualitative predication, which are conceptual distinctions rarely found, if at all, in languages other than Israeli Hebrew.

Notes

- 1) Benveniste, "La nature des pronoms", in *Problèmes de linguistique générale* 1. 215ff.
- 2) The consonantal text of the Pentateuch (the earliest layer of Biblical Hebrew) almost without exception presents one and the same form (*hw?*) for what the vocalization much later introduced distinguishes for gender, but most likely precisely this reflects a feature of later language, while the pronominal character of *hw?* (in this use) was still less pronounced in early language than in the later layers of Biblical Hebrew.
- 3) Cf. Adelberg, *Die Sätze des Typus "Ich bin ez Ioseph" im Mittelhochdeutschen* (= *Dt. Akad. d. Wiss. zu Berlin, Veröff. d. Sprachwiss. Komm.* 4, 1960). The MHG parallel is adduced by Polotsky, *Orientalia* 27 (1962) 426 = *Collected Papers* 431. What would maybe appear to be an obvious parallel, the Japanese theme-marker *wa*, aptly treated by K. Tsujita in an unpublished paper submitted at the Hebrew University, cf. *Watashi wa Yosehu desu* in the Biblical verse Gen. 45:3 just quoted, does not enter into consideration in this context, since we are here dealing with *thème*- or, respectively *rhème*-markers of a pronominal nature.
- 4) Polotsky, *l.c.* The Egyptian predecessor of Coptic *pe* was also uninflected for gender and number (cf. note 2).
- 5) Schwyzer, *Zur Apposition* (= *Abh. d. Dt. Akad. d. Wiss. zu Berlin, Phil.-Hist. Kl.* 1945/6, Nr. 3) 14.
- 6) This pair of terms is preferable to "topic" and "comment", since what the "comment" is a comment of is not always present in the sentence.
- 7) Cf. my study "Quelques phénomènes d'absence et de présence de l'accord dans la structure de la phrase en hébreu", *GLECS* 10 (1965) 78ff.
- 8) Cf. my *Contemporary Hebrew* 221 and Rubinstein, *Ha-mišpat ha-semānīy* ["The Nominal Sentence"] 82.

The Codification of Prescriptive Grammar

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Normative grammarians tend to prescribe in negative terms, that is to say they focus on the 'bad' forms rather than on the 'good' usage itself. This fault-finding approach predominates in 18th-century England, though exceptions occur. In the preface to his *The structure of the English language* (1798), John Sedger complains that 'most of our grammarians (have) written rather with a view to point out the common errors of speech, than minutely to investigate the genius of the tongue, . . .' However, for all his excellent principles, Sedger is on the list of sources utilized by *A Dictionary of English Normative Grammar 1700-1800* (DENG).^{*} His contribution is modest though: some thirty forms labelled as false. Topping the DENG list is John Knowles (1796) with c. 870 instances of 'improper' or 'inelegant' English.

It is the aim of the Bergen project to collect and process the forms which were frowned upon by the early English grammarians. To cater for the needs of the growing number of scholars for whom DENG should hold an interest, an overall scheme of presentation at once inclusive and easy of access had to be devised. In the first stages of our work, five different models were considered. Four of them were discarded and will merely be touched upon here, the fifth model being the main topic.

The *first scheme* is consistent with the method, employed by the grammarians themselves, of recording the errors under the parts of speech and syntactical categories where they seem to belong. To be sure, some grammarians also present separate lists of solecisms. The *second scheme* starts from the arguments with which the grammarians support their views on correctness: their appeal to etymology, analogy, logic, universal grammar, and other considerations, all of them discussed by S. A. Leonard in his classic study, *The doctrine of correctness in English usage 1700-1800* (Madison, 1929). Both schemes were rejected: they leak badly, there is a good deal of overlapping, and potential users of the dictionary would not know where to look for an answer to their particular problems. *Thirdly*, attempts were made to classify the data objectively by reference to the transformations called for in order to arrive at the approved form. Thus *come here* → *come hither* was entered under 'replacement', *the two first terms* → *the first two terms* under 'permutation', etc. The snag is that the 'correct' form is not always made explicit in the source texts,

^{*} The source texts mentioned in this paper are listed in A. K. Bjørge, *The sources of DENG* (University of Bergen, 1981).

and when it is, the interrelationship is sometimes too complex to be dealt with transformationally. A *fourth model* is ready to hand in the rhetorical grammars, where three 'grammatical figures' are distinguished, viz. ellipsis, redundancy and enallage. Cf. e.g. Kirkby 1746:135-141. While logically unassailable ('a word missing', 'a word too many', 'the wrong word'), this error typology is too crude for our purposes owing to the fact that enallage covers a wide diversity of phenomena currently described in terms of, e.g., concord, word-order, homonymy, synonymy, euphony, etc. But ellipsis and redundancy are among the seven error-based categories of the *fifth model*, which we trust will prove the definitive one, subject to minor revision. Near-objective, comprehensive yet economical, this typology has so far enabled us to deal with any deviations from 'correct' grammar in terms of AMBIVALENCE, DISSONANCE, ELLIPSIS, IDIOM, INCOHERENCE, INCONGRUENCE, REDUNDANCY. A conventional dictionary based on such a system will help students specially interested in, say, ellipsis to find the relevant data (types, select tokens and totals) in a single alphabetical list, and will thus relieve them of searching Adjectives, Prepositions, Verbs, etc. In addition, a complete data file is being trimmed for the computer, which will make it easy to retrieve, say, all the constructions in which a given word-class or functional unit is involved, dates of first appearance, information on the attitudes of individual authors, and so on. The register labels quoted or deducible from the source texts add up to well over one hundred, and so we have broken down the original list into a simple code (*dial, obs, vlg, inel*, etc.).

The seven error categories cut across traditional linguistic ones but have more or less negative connotations. For example, redundancy is constitutional for all human languages, but in our dictionary it is a name for tautologies, unnecessary repetition or mere verbosity. Similarly, 'idiom' is almost a pejorative term in the context of DENG: the grammarians were uneasy about the unitary meaning and syntactical constraints of multi-word lexemes, hence our IDIOM file.

To demonstrate further the problems of codification and the way we have tried to resolve them, let's have a look at INCOHERENCE and INCONGRUENCE, which account for about 60% of our data. INCOHERENCE is geared to word-order and is subdivided into Reversal, Split and Postponement. The subcategories of INCONGRUENCE are Concord, Government, Co-occurrence, and Hopping. Samples will now be given of Reversal, Split, Concord and Hopping, in that order. In the computer record, as many as seven fields can be queried, viz. error category, subcategory, main entry, subentry, citation, location (source), and register. The order followed in the samples corresponds to the hierarchical arrangement of the printed dictionary.

Reversal

In so far as the transposition of two or more clause elements is condemned by normative grammarians we call it Reversal. This term is distinguished from,

though it overlaps with, what is generally known as inversion. Our first sample illustrates the positioning of adverbials, which gave logical grammarians a lot of worry. To economize space we treat citations in terms of 'front' position and different subtypes of 'mid' and 'end' position:

//INCH/REV/Adv \subset PredP/M1 \rightarrow E3/*We always find them ready when we want them*/Mu1795:118/*ungt*//

The sample translates as follows: 'Adverbial included in Predicate Phrase' is the main entry, which comes under Reversal, one of the three subcategories of INCOHERENCE. The placement of an adverbial is at issue, and PredP represents the syntagmatic span of the order change. '*We always find them ready...*' illustrates mid-position 1 and is labelled as ungrammatical ('erroneous') by Lindley Murray, who prescribes '*We find them always ready...*', defined as end-position 3. The subentry specifies the order change.

So far we have noted about thirty different types of adverbial reversal, or nearly half the potential number. But for the code, it would have been necessary to specify the change by including the correct version in each citation.

Split

This error subcategory takes care of utterances in which a segment of variable size or structure comes between two elements seen as going together. It corresponds to *tnesis*, the rhetorical figure which 'divides a compound word, and interposes something' (Fogg 1796:249).

//INCH/SPL/Ant Pron_{rel}/if the walks *were a little taken care of* that lie between them. 'if a little care were bestowed on the walks that'
/B11783:472/*impre*//

The main entry gives the two elements that must not be separated, in this case a relative pronoun and its antecedent; the intervening segment is not marked in the entry but is underlined (italicised) in the citation. No subentry is needed. Incidentally, Blair's comment on the sentence and the way he amends it call for two more input slips: one placed in the IDIOM file ('take care of') and another in the Postponement file (the construction with non-initial preposition).

Concord

As will be remembered, Concord is the first subcategory of INCONGRUENCE; it numbers appr. 1870 examples, and about 75% involve the syntactic relationship of subject and finite verb. S V has 79 subentries, which are 'readings' of the main entry. They are ordered according to the nature of the subject, and for each subject according to its corresponding verb:

//INCG/CNC/S V/N_{sg.n}~or V ϕ /as soon as discretion, consideration or age have brought the man to himself/Cr1772:46/*ungr*//

//INCG/CNC/S V/N_{sg2..n} ~ or V_{aw}/The smart or the sneering manner of telling a story are inconsistent with the character of a historian/Us1785:108/unger//

In each sample, the first part of the subentry (as far as and including 'or') interprets S(object) as 'two or more than two singular nouns coordinated with or'; the tilde indicates that the ordering of the symbols is notationally expedient but contrary to actual usage. In the first citation, the 3rd pers. sg. -s is lacking (V_s). In the second sample, where *to be* has gone wrong, the relevant form ('are' in this case) might have been used in the subentry, but in the interest of generality we have settled on V_{aw}, the notation for *are/were*.

Hopping

While Concord, Government and Co-occurrence typically deal with the relationship between exponents of different parts of speech, Hopping is focused on one class or function at a time. Its domain is the complex sentence, and what is being criticized is the way a given unit is expressed when repeated. For example, the use of different relative pronouns with a shared antecedent is regarded as 'faulty' by Ussher (1785), and inconsistencies in regard to tense and voice are frequently criticized; the sample illustrates mood change:

// INCG/HOP/V_{ldc} ~ V_{abl}/if a Man *have* a hundred Sheep and one of them *is* gone astray/ Kn1796':62/impro//

The tilde used in the main entry separates correlating items and indicates that they do not necessarily occur in the order given in the grammatical description.

The last mentioned subcategory is marked by inconsistencies—and so is DENG. Or rather, the code is not, and could not be expected to be, uniform or evenly exploited. Four examples:

i. As a general rule, the grammatical description is based on surface structure arguments, but this principle has to be given up in dealing with ELLIPSIS and occasionally elsewhere.

ii. The entries are for the most part grammatical strings, but the level of abstraction varies and lexemes are regularly listed as separate entries in IDIOM and frequently in REDUNDANCY and Co-occurrence.

iii. Some grammatical strings are syntagms, others are nonsyntagmatic pairs of correlating items, but grammatical symbols are also used singly.

iv. The tilde is consistently used in Hopping, the colon has found its way into AMBIVALENCE, and the arrow, as we have seen, into Reversal.

Such differences argue that the error categories have a basis in fact although they were first set up intuitively. Once readers have made themselves familiar with each individual category, they should have little difficulty in using the dictionary for their particular purposes.

Rule Recession and Rule Loss

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The traditional Generative view of phonological change, as set forth in Robert King's (1969) *Historical Linguistics and Generative Grammar*, posits four main types of change: rule addition, rule loss, rule reordering, and simplification. The term 'rule loss' is used in at least two differing senses in the literature. In the writings of King and Paul Kiparsky, the term is applied to situations in which a rule of the type $A \rightarrow B$ is lost, causing all alternants which were formerly governed by the rule to revert to the underlying form at once. Obviously, if this is indeed a type of linguistic change, it provides strong support for the abstract nature of lexical representations.

King and Kiparsky compare rule loss in their theory with the 'analogy' explanation. In this view, a rule is lost gradually by means of lexical change, equivalent to what many linguists refer to as *paradigmatic leveling*. In the Generative approach the alternations are eliminated because the rule has been lost, while in the 'analogy' approach the rule is lost because the alternations have all been leveled. I will refer to the former as *rule loss* and to the latter as *rule recession*.

There are at least three main consequences which stem from viewing rule loss as a type of grammar change. One of these is that rule loss is a one-step process, not gradual as is implied in rule recession. Also, in all cases where a rule P of the form $A \rightarrow B$ is lost, all alternating allomorphs will revert to the underlying form A , never to B . Rule recession allows for the leveling to favor either A or B , or both. In addition, residue from rule loss can only be accounted for by assuming that restructuring had taken place in the lexicon before the rule was lost. With the recession explanation, residue is nothing more than those alternations which have, often because of high token frequency, been able to withstand the pressure to undergo leveling.

In Tiersma (1980) I present two phonological rules in Frisian which from a synchronic point of view might be considered to have undergone rule loss. A closer examination of the way in which these rules were lost, observing the changes which took place between generations, indicated that these are in fact instances of rule recession. The loss of alternations was gradual and there are residual forms in the language which cannot reasonably be attributed to restructuring.

Since these cases of possible rule loss turned out, on closer examination, to be rule recession instead, does rule loss in the sense of King and Kiparsky exist at all? One of the major problems in this regard is that it is often im-

possible to trace the history of purported cases; in most instances adequate records of the in-between stages of the change do not exist. Yet there is one relatively recent phonological change which has been attributed to rule loss, and which in addition is viewed as one of the stereotypical occurrences of the process: the loss of final devoicing in various German dialects.

The Swiss German dialect of Visperterminen, described by Wipf (1910), is a good example of where the rule of final devoicing may be considered to have been lost. Words like *tag* 'day', *woeg* 'way', *xo:rb* 'basket', and *xalb* 'calf', all with voiced (better: lenis) final obstruents, indicate that the rule of final devoicing, which was once operative in this dialect, has been lost and that the alternants have reverted to the underlying representation. Adverbs like *ap* 'from' and *woek* 'away' do not show the effects of rule loss since they are assumed to have undergone restructuring prior to the loss of the rule.

Yet other dialects, such as those of the Heimattal of Deutsch Wallis, show a more complicated situation (Bohnenberger, 1913):

(1) WALLIS	STANDARD GERMAN	GLOSS
b:rg	Berg [k]	mountain
tag	Tag [k]	day
xind	Kind [t]	child
(2) stöp	Staub [p]	dust
sork	Sorge	sorrow
di:p	Dieb [p]	thief
truok	Trog [k]	trough
(3) liep-liebe		clear
rat-reder		wheel(s)
xalp-xalber		calf(s)
jut-jude		Jew(s)

Note that the forms in (1) appear to have reverted to the underlying representation. Those in (2), on the other hand, have retained the voiceless stops, and those in (3) still undergo alternation between voiced and voiceless stops. According to Bohnenberger, the language of the area shows various stages of restitution of voiced obstruents in final position. In view of this, the dialect of Visperterminen, which initially appeared to illustrate a stereotypical case of rule loss, may in fact be better viewed as the end product of a long process of leveling, a process which in Deutsch Wallis has not yet reached completion. In other words, the dialect of Deutsch Wallis, in which the reversion of alternations to what is considered the underlying representation (the voiced or lenis obstruents) has not yet been completed, is the "missing link" in the question of whether this potential instance of rule loss is an instantaneous, across-the-board change or a process which gradually spreads through the lexicon. The evidence in this case strongly suggests that it is rule recession rather than loss.

Another example of this type of grammar change presented by King is the loss of the rule of Verner's Law in Gothic. This law, which produced alter-

nation referred to as *grammatischer Wechsel*, states that voiceless spirants become voiced in voiced surroundings when the accent does not fall on the immediately preceding syllable. The effects of the process in the Germanic strong verb system are illustrated below:

(4)	INFINITIVE	PAST SG	PAST PLURAL	PAST PART	GLOSS
OEng	snīpan	snāp	snidon	sniden	cut
OSaxon	tiohan	tōh	tugun	gitogan	pull
Gothic	sneipan	snaip	snipum	snipans	cut
	tiuhan	tau	tauhum	tauans	pull

Most traditional grammars assert that in Gothic the effects of Verner's Law were leveled by analogy. King (1969), however, attributes this change to rule loss, stating that this cannot be analogy because *all* of the alternations reverted to the underlying form. It is important to mention in this context that some words, like *aih* 'I possess' and *aigum* 'we possess' still exhibit this alternation. Furthermore, the observation in Paul (1920) that the verbs *hwairban*, *swairban*, and *skaidan* generalized the voiced spirant is an important counterexample to King's claim. Since rule loss implies that all alternants must revert to the underlying form, this argues persuasively that this is in fact rule recession.

Based on this evidence, I find it improbable that rule loss in the sense of King and Kiparsky actually occurs. Of course, it is impossible to disprove every purported case of rule loss—most claimed instances occurred hundreds of years ago. Yet if rule loss really exists, it should be possible to find a language where the older generation has a fully productive rule which has been lost (with reversion to the underlying form) by the next generation. The burden of proof lies with those who believe that rule loss in this sense exists.

Does this necessarily mean that a rule remains in the language until all alternation has been leveled, as suggested by the recession explanation, and is then lost? I believe that both rule loss as grammar change and rule recession occur. A productive rule can be lost for various reasons, as King and Kiparsky suggest, but the alternations do not all revert to the underlying representation in one fell swoop. Rather, they undergo paradigmatic leveling on a word-by-word basis and need not necessarily reinstitute the historically underlying form. Leveling (recession) takes place precisely *because* the rule has been lost and the alternations must therefore be memorized. As alternations are leveled, lexical entries are simplified, providing a natural explanation for why paradigmatic leveling occurs—it occurs because the rule governing the alternation has been lost from the grammar. In this sense, both rule loss and rule recession are actual forms of phonological change, the one leading to the other.

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On Models of Linguistic Performance

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Of the 315 items included in a recent bibliography on speech errors (Cutler, 1982) 258 have been published since 1950, and 223 only since 1970. Thus, although there has been scientific interest in such data since the beginning of the 19th century, the investigation of linguistic errors has really 'come of age' in the last 15 years. The increased interest in slips of the tongue results from the recognition of the fact that "The analysis of 'spontaneously incorrect' sentences belongs within the realm of psycholinguistics inasmuch as the errors they contain can give some clues to the particular mechanisms of language production, in which the abnormal case... can lead to conclusions about the factors involved in normal functioning," as suggested by Bierwisch (1981). He further suggests that "the phenomena... can (also) be of interest in sorting out questions of the linguistic system proper... It goes without saying that linguistic and psycholinguistic analyses of spontaneous error, if they are to be meaningful, can only be made against the background of significant hypotheses concerning the structure of the language in question." An examination of the papers included in the Cutler bibliography shows this to be the case.

The relationship between the mental grammar and real-time production processes is also relevant to a question raised by Cutler (1981). She refers to "a common confusion in the speech error literature (which) arises from a failure to distinguish between the CAUSE of an error's occurrence and the MECHANISM by which it occurs" pointing out that "The two are logically distinct." This paper will attempt to clarify this distinction. In general, the difference between the cause and the mechanism of an error may be exemplified by the following:

(T = target utterance; E = actual utterance with error)

- I. 1. T: He made headlines > E: He made HAIRLINES
2. T: The conquest of Peru > E: The conquest of PURDUE
3. T: I'm going to die young but I'll die less young >
E: ... Yes Lung

In (1) the speaker was referring to a barber. The cause of the error therefore appears to have been non-linguistic—an intruding thought which produced a "competing or alternative plan" (cf. Butterworth, 1981; Baars, 1980). The fact that HAIRLINE and HEADLINE have identical initial sounds, two syllables, and end with the same morpheme may constitute linguistic causal factors which increase the probability that such an error will occur. But such

causes are distinct, as Cutler has shown, from the mechanism involved in producing the error, which in this case is the incorrect selection of a substituted word for the targeted one. In (2) the speaker was a professor at Purdue. The similarity of phonological structure of the targeted and substituted words may have triggered the cognitive association which in turn may have increased the probability that the wrong word would be selected. The fact that the substituted word is phonologically similar to the targeted word raises the possibility that phonologically similar words are listed together in the mental dictionary or have connecting pathways. The third error further illustrates how an intrusive thought may be a causal factor in the production of a slip of the tongue, but a different mechanism is involved in this case. The speaker who produced the error cited in (3) had recently given up smoking. This might have triggered the mechanism by which the two sounds—/l/ and /j/—were reversed. Noone has yet hypothesized how the intrusive thought leads to such a transposition of sounds.

The possible causal effect of phonological similarity is discussed in an analysis of 65 word blends from Fromkin's (1973) corpus of errors analyzed by Butterworth (1981). He points out that 51 involve pairs of presumed words which are phonologically similar i.e. more than half of their segments are identical (II. 1) or have the same syllable pattern (II. 2), or the same initial segment (II. 3), or the same stress pattern as well as other similarities (II. 4). Only 14 pairs as exemplified in II. 5 do not show these similarities.

- II. 1. trying / striving > strying
2. terrible / horrible > herrible
3. grizzly / ghastly > grastly
4. mainly / mostly > maistly
5. instantaneous / momentary > momentaneous

These blends further support Cutler's distinction between causes and mechanisms. The causes again appear to be competing plans, internally induced, rather than due to external context, presumably because the words are semantically similar and, in some cases, almost synonymous. It is as if the speakers were unable to make up their minds as to which lexical items better expressed their intended messages. Two words were therefore selected, and because of the indecision were combined.

Phrase and sentence blends also occur (Fay, 1981). The blending of idioms or 'fixed' phrases also show that "competing plans" constitute a plausible cause.

- III. 1. In one ear and out the other / here today and gone tomorrow >
IN ONE EAR AND GONE TOMORROW
2. give him an inch and he'll take a yard / give him a rope and he'll
hang himself > GIVE HIM AN INCH AND HE'LL HANG HIM-
SELF

Thus, both linguistic and non-linguistic factors may be causes of speech

error production: intrusion of internally or externally conditioned thoughts as in the examples in I; phonological similarity as illustrated by the blends in II, as well as in segmental errors such as phonological spoonerisms (Shattuck-Hufnagel and Klatt, 1980). Phonological similarity may also be the sole cause of word substitution errors as exemplified in IV.

- IV. 1. T: the frequency of occurrence > E: the FLUENCY ...
 2. T: prohibition against incest > E: ... against INSECTS
 3. T: Trubetzkoy is less procedural > E: less PROCESSIONAL

As we have already seen from the examples in I, both form and meaning may be contributing causes. In those examples, the meaning did not relate to the semantic similarity of the two words involved in the substitution but rather to the referent of the intrusive thought. Other word substitution errors, however, appear to be caused simply by similar semantic class membership, as in V.

- V. 1. T: he has a wedding band on his finger > E: ... on his WRIST
 2. T: he paid her alimony > E: he paid her RENT
 3. T: a horse of another color > E: ... of another RACE
 4. T: straight jacket > E: STUFFED SHIRT
 5. T: I didn't want to be a sexist > E: ... a RACIST

Such examples once again provide clues as to the mental organization and representation of the grammar, in these cases, the lexicon. If words are stored together according to phonological structure or semantic categories (or are connected by a thesaurus like structure) random selection errors could account for these utterances. In all cases, then, the linguistic structure and system are contributing causes.

The linguistic structure of the intended utterance can contribute to the error in yet another way. The word substitution and blend errors presented above reveal paradigmatic influences; syntagmatic causes are also evident as shown in VI.

- VI. 1. T: She wrote a luke warm letter > E: ... a luke warm WATER
 2. T: She gave birth at midnight > ... at MIDWIFE
 3. T: it's fun to collect speech errors > E: ... to CORRECT ...

While it is not yet clear HOW phonological and semantic similarities, or semantic structure, or rhythmic structure (Cutler 1980), or competing plans, trigger the mechanisms which result in slips of the tongue, they are good candidates for causes of these errors. The mechanisms which they trigger appear to be similar across grammatical components: features, segments, syllables, morphemes, words, phrases can be anticipated, persevered, transposed, or blended. Another mechanism responsible for error production is incorrect morphological or syntactic rule application (Fromkin, 1980).

It is evident that both the causes and the mechanisms reveal the underlying grammatical system, showing that speech errors provide important data for

the construction and validation of theories of grammar and psycholinguistic models of production.

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English Quantifiers and the Perception of Risk

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In this paper I will report on a study of some numerical interpretations of the class of English words referred to as *quantifiers*. These are the words (or phrases) which may specify an exact numerical value of a fixed finite population, and which most English speakers would arrange in order of magnitude as follows.

(1) *Some quantifiers in order of magnitude*

none (of)

barely a few

only a few

some

several

many

most

nearly all

all but a few

all

There might be some disagreement about the ordering of some of these quantifiers. For example, *some* might be interpreted as more than *several*; but in any case both *many* and *most* are more than *some*. This list of course does not exhaust the range of English quantifiers, but it will serve to illustrate the class of words that I will be concerned with.

I became interested in the problem that I want to discuss while explaining the difference between restrictive and non-restrictive relative clauses in English.

Otto Jespersen in *Essentials of English Grammar* (1933) illustrates the difference by contrasting sentence (2), which is restrictive

(2)R *He had four sons that became lawyers (and two that became clergymen)*

(this may be continued with a phrase such as *and some others*)
with sentence (3), which is non-restrictive

(3)NR *He had four sons, # who became lawyers*

(This may not be continued grammatically as in the restrictive case.)

In another example, Jespersen contrasts sentence (4), which is restrictive with sentence (5) which is not.

(4)R *There were very few passengers that escaped without serious injury*
Here the number of passengers escaping is presumed to be small, but we don't know how many were involved altogether. From the non-restrictive clause sen-

tence (5)

(5)NR *There were very few passengers, # who escaped without serious injury.*

the total number, although unspecified, is presumed to be small.

There are many logical and syntactic problems concerning quantifiers which I will not be able to consider here. For example some quantifiers such as *no* may not be used with non-restrictive relative clauses. Thus it is alright to say

(6)R *There were no passengers who escaped without serious injury.*
but not

(7)NR **There were no passengers, # who escaped without serious injury.*
These and related problems have been investigated by Carlota Smith (1964).

In the consideration of Jespersen's restrictive relative clause sentences such as (4), we ask, for estimating the number of surviving passengers, *does it make a difference how many the total number was to begin with?*

The answer is it apparently does. Rephrasing the question in terms of a disaster with *only a few* survivors, one might expect to find an answer in terms of the Webster's III (1976) dictionary definition of *a few* as *some at least: not many but some—used with a preceding a to designate some rather than none* ... This does not offer much help. Another answer might be about five per cent or some other small fraction, but as it turns out English speakers seem to evaluate *only a few* in a quite different way.

In a pilot study the question (Q1) was asked of students and faculty at CSUF. (Q1) Imagine a series of disasters. Some people survive—but *only a few*. I want you to write down what number *only a few* means to you in the following situations. Put down the first number that comes to mind. There are no right or wrong answers.

(8) number of individuals n	—only a few (medial estimates)
10	3
100	10
1,000	25
1,000,000	1,000

Thus, rather than a fixed number or a percentage, it seems that

(9) (Only a few) $n=f(n) \cong n^r = n^{0.5}$, where we call r the "risk" associated with the quantifier *only a few*.

This suggests that most people have a consistent perception of risk in an actuarial sense. The range of values for *only a few* of n apparently reflects the subjective probability that expresses an individuals expectation of being labeled, or being selected—or in this case surviving.

It might be argued that these responses merely reflect the numerical notation, the non-random numbers of the stimuli, and also the situation—in this case a disaster. These objections may be overcome in part by using "neutral" data with randomized stimuli.

One such neutral question is

(QII) *Most, but not all of the inhabitants of Scotland have surnames be-*

ginning with the prefix *Mac* or *Mc*. In a sample of five villages near Glasgow with populations as indicated, how many individuals had family names beginning with *Mac* or *Mc*. Put down the first number that comes to mind. There are no right or wrong answers.

Another neutral question is

(QIII) The California brown bear population ranges in color from light brown to almost black—a property determined by genetic factors. In a random sample observed in five bear ranges, a few were lighter than medium brown, many had pelts ranging from medium to dark brown, and only a few were almost black. What number do you feel *many* indicates in the following samples.

Mid-range estimates of *only a few* survivors (QI), *most* Scotsmen (QII) and *many* bears (QIII) are shown in Figure 1. All of the situations are hypothetical; and for (QII) and (QIII) the stimuli are randomized. Gross fits are of the form $f(n)=n^r$, where $0.9 < r < 1.0$.

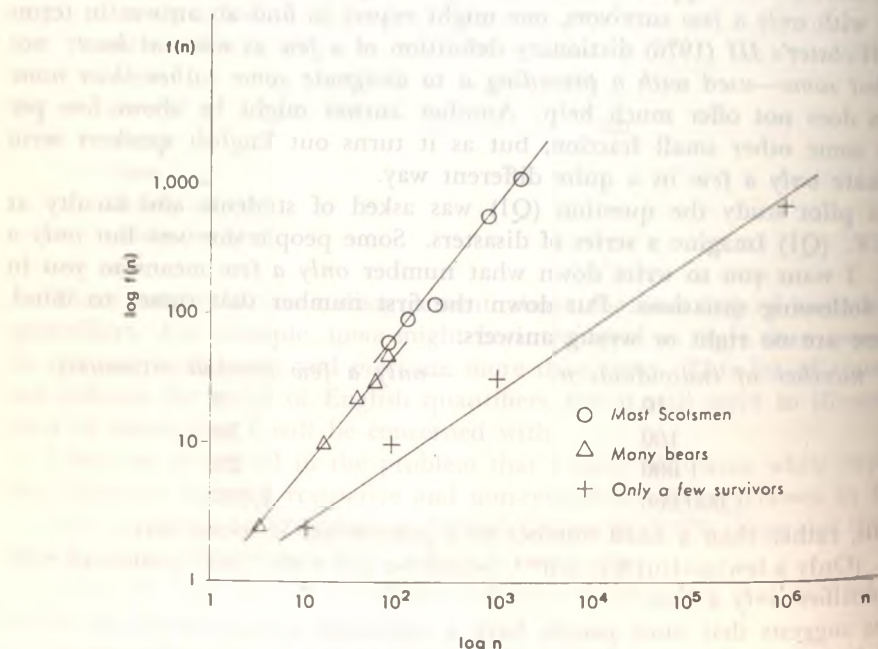


Figure 1. Mid-range estimates of *only a few*, *many*, and *most*.

The Webster III dictionary definitions of *many* as *consisting or amounting to a large but indefinite number: not few...* and *most* as *the greatest number: the majority of...* give little hint as to how specific values are selected. This sort of estimation may be a kind of learned verbal skill. My subjects were not representative of English speakers in general and cross cultural and social studies remain to be done.

These data suggest, however, that English speakers using quantifiers give responses certainly more complex than approximations of simple percentages. In a recent study on the application of "fuzzy sets" to psychology, where decisions must be made with incomplete information, Manfred Kochen (1975) found that evaluations of the distance terms *very far*, *far*... were non-linear. That people give non-linear responses to physical stimuli was established by S.S. Stevens (1961). In studies of psychophysical scaling estimates of loudness, heaviness, and brightness are described by "power functions" with various exponents depending on the type of stimulus. It turns out that what is perceived to be twice as loud, or twice as heavy, or twice as bright does not mean, in terms of the physical stimuli, 100% brighter, or heavier, or louder—but rather increases in orders of magnitude. Thus loudness is measured in decibels—i.e. powers of ten.

Then, since humans are not necessarily linear estimators of physical stimuli, that estimates of risk follow a non-linear pattern is not surprising—and may explain the utility of the terms which we call quantifiers.

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Quantification of Linguistic Expectancy by Means of Frequency Estimation Tests

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The experiment to be described here is based on the assumption that knowing a language means more than merely having acquired the meanings of words and their correct grammatical usage; it also involves having learned the probabilities of occurrence of linguistic units.

These data have been internalized without conscious effort on the part of the language user: simply by being exposed to the incoming stream of everyday language material. The storage of language frequency data and the implicit knowledge about frequencies of occurrence of linguistic units are thus induced by the total of an individual's past verbal experience in his language. Investigations concerning the underlying knowledge about the frequency structure of the language can contribute towards understanding and quantifying the concept of linguistic expectancy. We refer to linguistic expectancy as being related to the working knowledge of the language user: the register of linguistic data, unconsciously internalized by the total of an individual's past verbal experience.

The present experiment was, therefore, designed to examine whether and to what extent an aspect of linguistic expectancy can be made explicit as an psychological reality. For this purpose a forced choice test was constructed in order to compare the objective frequency data to the subjective ordering of language units on a continuum established by the judgments of individuals. The question now arises whether regular patterns in a language also become part of the knowledge acquired by a second language learner. If so, we hypothesize that by quantification of linguistic expectancy the concept can be used as a means for testing second language proficiency.

The following questions thus guided the present inquiry:

1. Can linguistic expectancy be made explicit?
2. Is the forced choice situation an appropriate means to make linguistic expectancy explicit and does it thus provide us with the possibility of quantifying the concept?
3. Is the knowledge of frequencies of occurrence of linguistic units also acquired in a second language?
4. Can it be demonstrated that linguistic expectancy increases with increasing overall knowledge of a second language?

Frequency judgment experiments form only a small part in the field of

frequency-effect studies, which, in addition, have mainly concentrated on word-frequency effects with visually presented stimuli, (e.g. Howes and Solomon 1951; King-Ellison and Jenkins 1954; Goldiamond and Hawkins 1958; Broadbent and Broadbent 1975; Pearson and Studt 1975; Hasher and Chromiak (1977). Begg and Rowe (1972) report on an experiment in which subjects gave frequency of familiarity judgments of meaningful words presented in a continuous sequence; they found the judged familiarity to be logarithmically related to exposure frequency.

We believe our present experiment to differ from a number of related studies in the following aspects:

- a) presentation of the test material was not preceded by training sessions in which subjects were familiarized with the stimuli to be used in the experiment. This study, therefore, clearly does not belong to the tradition of the so-called close-recognition experiments.
- b) subjective estimates were acquired in a forced choice situation instead of using either an open-ended or a fixed-ended rating scale.
- c) in an attempt to eliminate semantic association effects as much as possible meaningless linguistic units were used as stimulus material.

Test Material

The stimuli consisted of Dutch and English word-initial consonant clusters. Due to confusions between graphemic and phonemic representation of the clusters used in a pilot experiment, only clusters with a one-to-one letter-phoneme correspondence were used in the present test. The English objective data were extracted from Robert's (1965) phoneme count and the Dutch material was based on Uit den Boogaart's (1975) corpus.

Results of another pilot test suggested that the concept of linguistic expectancy corresponds with the lexical frequency structure of the language rather than with the total frequency data, and we therefore decided to base the stimulus material of the present study on lexical frequency data. This decision is also supported by the findings of Goldstein (1977) based on a perceptual identification test of CV and CVC clusters, in which results correlate significantly with lexical frequency of occurrence but do not correlate well with total frequency of occurrence.

Table 1 shows the Dutch and English clusters used as test stimuli in decreasing order of frequency of occurrence as found in the frequency counts mentioned above.

These English and Dutch consonant clusters were rank ordered and the maximal number of different pairs was formed for the English and Dutch list separately. In order to eliminate any possible bias in favour of the first appearing cluster of every pair an alternative version with interchanged position of the clusters within one pair was constructed for each list. Both versions were distributed in equal numbers among the subjects.

Subjects

In total 152 subjects took part in the experiment, 92 were undergraduate students at the University of California in Los Angeles, native speakers of American English and 60 were pupils of a Dutch secondary school, learning English as a second language. Of the latter group 27 were 1st grade pupils and 33 5th and 6th grade pupils, all of them native speakers of Dutch.

The subjects were asked to indicate that letter combination of a pair, which they thought to occur more frequently at the beginning of Dutch and English words respectively. English speaking subjects were given the English list only, whereas the Dutch population took part in the Dutch and, after a fortnight's time interval, in the English test.

Results

Test results were processed and analyzed by means of Edwards' (1957) attitude scaling technique.

Subjective scale values were rank ordered and correlated with the objective data yielding the following rank order correlation co-efficients.

	<i>English pop.</i>	<i>Dutch pop.</i>
English test	$r_s = .85 \text{ } p < .01$	$r_s = .78 \text{ } p < .01$
Dutch test		$r_s = .74 \text{ } p < .01$

In order to examine the hypothesized improvement in judging cluster frequencies of a second language with extended training in that language the Dutch population was subdivided into two groups and rank order correlations were computed for 1st graders and upper graders (5th and 6th form) separately. The results are as follows:

	<i>Dutch 1st grade</i>	<i>Dutch 5th and 6th grade</i>
English test	$r_s = .60 \text{ } p < .05$	$r_s = .81 \text{ } p < .01$

Application of the Spearman rank correlation coefficient test reveals that rank order correlation coefficients between subjective scale values and objective frequency data are significant at the .01 level in the case of the American, Dutch overall and Dutch upper grade group in the English test as well as in the Dutch overall group in the Dutch test and significant at the .05 level in the case of the Dutch 1st grade in the English test.

It may be worthwhile to tell you that subdivision of the Dutch speaking population as to their performance in the Dutch test shows that there is no significant difference between mother tongue test results of 1st formers on the one hand and 5th and 6th formers on the other ($t = .002$, ins.).

In making scatterplots of the relationships between subjective scores and objective data for every population and subpopulation, it was generally noticed that the relative change in intervals of subjective scores decreases as the objective values increase. This and the fact that estimates of frequencies have gen-

erally been shown to have higher correlations with logarithmically transformed values of objective frequencies (see e.g. Carroll 1971, Begg and Rowe 1972, Carroll and Lamendella 1974 and Rubin 1974) justify the application of the exponential curve fit.

Scattergrams showing the relation between objective and subjective data were made with the logarithmically transformed values of the objective frequency data plotted along the y-axis. The solid line indicates the "straightened out" exponential curve fit; Tuckey 1977).

Conclusions

1. The technique of a forced choice test seems to be an appropriate operational device for making explicit an aspect of linguistic expectancy. Subjects seem to be able to make statements with considerable accuracy about the frequency of occurrence of language units, in our particular case word-initial consonant clusters.

2. The coefficient of determination or "goodness of fit" computed by means of the exponential curve fit procedure is for all groups considerably higher than the one resulting from the linear regression fit, which suggests that there is a logarithmic relationship rather than a linear one between subjective and objective frequency data.

3. A comparison between the rank order correlation coefficients of the Dutch overall group in the Dutch test and the English group in the English test shows that the latter is somewhat higher. This could be accounted for by the greater number of word-initial consonant clusters in Dutch (30 in Dutch and 22 in English with a one-to-one letter phoneme relation and 34 in Dutch and 25 in English in total), which makes the mathematical probability of occurrence of an arbitrary cluster in Dutch smaller and, accordingly, the choice of a particular cluster more difficult. The degree of difficulty in judging frequencies of language units seems to change according to the frequency structure of the language in question, which makes it desirable to take native speakers' performance as calibration point when scoring a second language estimation test rather than subject's performance in his mother tongue.

4. In order to make our experiment fully symmetrical it would be necessary to carry out the Dutch test with English speaking subjects learning Dutch at the beginner's stage and at an advanced level. We fear, however, that recruitment of a population meeting such requirements would be a lengthy procedure if not a matter of near impossibility. The available data, however do suggest a positive answer to the question of a possible improvement of frequency judgments of subjects with greater overall knowledge in a second language: rank order correlation coefficient between subjective scores and objective data is higher in the case of the upper forms than of the 1st form. These data suggest, then, that awareness of cluster frequencies in a second language does increase with increasing overall knowledge of that language. The insignificant difference between mother tongue test results of 1st formers

on the one hand and 5th and 6th formers on the other means in our case that the improvement in English test results cannot be ascribed to a general improvement concerning linguistic expectancy as a function of the level of education or age.

5. Due to the smallness of the available sample native language interference could not be proved with a powerful statistical test. Achieved results do suggest, however, that the frequency structure of subject's mother tongue influences their judgment of frequencies of language units in a second language.

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Bilingual Memory for Prose

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An area which is a particularly fruitful and intriguing domain of research for establishing the principles underlying the psychological processes associated with language use is that of bilingualism. A knowledge of the processes and operations associated with the use of two languages may have important implications for both linguistic theory and psychological theories relating to cognition and learning. The study of memory in a bilingual context may be especially revealing. Since human cognition and learning is so highly dependent on access through memory to previously acquired information and experiences, any information which may further understanding of this phenomenon may have far reaching effects.

Investigating the effects on memory of the acquisition of information through two languages affords a particularly interesting avenue of research since the bilingual paradigm allows one to study the encoding and decoding of information into one cognitive system by means of two linguistically distinct channels. The researcher thus benefits from an unparalleled vantage point for examining the interaction between linguistic form and meaning. An especially interesting issue in this regard is whether the distinctiveness characterizing two languages at each level of analysis (phonological, lexical, and syntactic) leads to significant differences with respect to retention of the semantic information conveyed through the two linguistic systems. The study of memory for semantic material presented in two languages can lead to a clarification of the contribution of aspects of linguistic form and meaning to retention.

Therefore, the issue to be investigated is whether a unilingual vs bilingual transmission of information results in differential effects with regard to memory for the conveyed information. A number of possibilities can be considered. First, that language acts as a transparent code, with few or no effects on memory. Second, that the encoding of information in two languages significantly affects memory for that information in which case the significant effect may be that of a) interference, or poorer memory for the information, or b) a facilitation or greater access to information.

Much of the research in the domain of bilingual memory has been conducted by means of testing of the lexicon in isolation from any grammatical context. Some word list studies report equal recall from unilingual and mixed language lists (Kollers, 1965, Nott and Lambert, 1968) while others report poorer recall from mixed language lists (Tulving and Colotla, 1970). Other studies have examined bilingual lexical memory by manipulating the interference in-

duced by preceding or subsequent lists of words. An experiment by Goggin and Wickens in 1971 revealed that when a shift in language occurred from one list to another, there was significant recovery from interference, suggesting that categories in the two languages may therefore be psychologically distinct.

Such results would suggest that language is not merely a transparent code, but that the number and variety of distinctive attributes which differentiate languages at each level of analysis might account for the recall increase.

Support for the hypothesis that languages are psychologically distinct is provided by the notion of encoding variability and its effect on memory reported in the psychological literature. Encoding variability refers to the notion that an item may be encoded in a number of ways, i.e., a number of attributes may be associated with encoding of that piece of information in memory. And, the literature suggests that information encoded with a greater number and variety of features or attributes is more easily accessed than information encoded with a more limited number and range of features.

Thus it can be suggested that the presentation of information in two languages may result in better consolidation of information and greater recall than in a unilingual situation due to the greater number and variety of distinctive attributes characterizing the bilingual presentation. These attributes occur on a number of levels, including both form and meaning.

A number of experiments were formulated to test the preceding hypothesis. Contextualized materials, i.e., prose passages, were used in the experiments. Much of the experimentation done in the domain of bilingual memory deals exclusively with words or sentences divorced from any communicative context. While not vitiating the value of experiments which manipulate memory for words in establishing certain principles of short-term memory processes, one cannot assume that such results are equally representative of processing of text material where recall of the message is given priority, as in most situations outside the experimental laboratory where the individual reads for meaning rather than form.

Another factor which was taken into consideration concerns the subject population chosen for the study. Important differences in response patterns may be masked by a failure to adequately distinguish among the different types and degrees of bilingualism. Therefore, attempts were made to select bilinguals having language backgrounds as similar as possible within each experiment.

The first experiment required subjects to read a passage twice and answer questions on it following an interfering passage. Subjects were native French speakers who had taken over two years of university level course work in an English language university. Subjects were directed into one of three groups: English unilingual (the texts read twice in English), French unilingual (the texts read twice in French), or bilingual (half receiving the French text first, then the English and half the English first, then the French text). A one-way analysis of variance revealed a significant effect when the bilingual group was compared to the unilingual groups ($p < .03$).

The second study used a slightly different procedure where subjects were required to recall information from an initial passage following learning of three similar, interfering passages. The statistical analysis here showed a significant effect when unilingual groups were compared to the bilingual groups ($p < .02$). Both studies therefore showed significantly less interference in the bilingual conditions as predicted by the hypothesis.

A third experiment used a different subject population, English native speaking students in a French immersion program in a Montreal high school. Similar to the procedure in the first study, in this experiment students were required to recall information from an initial passage following learning of an interfering passage, then a second reading of the first passage, although different materials were used. The results here revealed no significant differences between the bilingual and unilingual groups ($p < .29$). Thus the third study did not support the hypothesis. It is possible that the failure to find a significant difference in the final study may be attributed to the fact that the students were not fluently bilingual. It is interesting to note that the bilingual groups did not perform significantly worse than the unilingual groups as one might expect when subjects have not attained full fluency in the second language. The results of this third study suggest that a threshold level of second language fluency is necessary in order for the bilingual presentation to have an optimal effect.

To conclude, the results from the studies indicate that there is significantly less interference in a bilingual presentation of information than in a unilingual presentation, providing subjects have attained a threshold level of fluency in the second language.

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A Repetition Experiment on Children's Comprehension of Complex Sentences in Japanese

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1. *Introduction* This paper reports a repetition experiment with kindergarten children which uses complex Japanese sentences and discusses some of the results in terms of children's mechanism of comprehension.

The purpose of the experiment is two-fold: Firstly, the processing of complex sentences (i.e., sentences with an embedded clause in surface structure) in Japanese children has not been studied extensively. Secondly, the major technique that has been adopted in previous studies of children's processing of complex as well as simple sentences has been act-out. However, since the content of complex sentences often cannot be represented by actions or toy-movement, the act-out method is not utilizable for the study of comprehension of complex sentences in general. Thus, we adopt here the technique of repetition.

2. *Subject* The subjects were 60 children, 30 five-year-olds (half male, half female) and 30 six-year-olds (half male, half female), attending the Kinjo Gakuin Kindergarten. Since the stimuli all consist of complex sentences, this age range is assumed to be appropriate to this experiment.

3. *Procedure* Children were tested individually in a room at the Kindergarten. They were first instructed to exactly repeat the sentences the experimenter presented orally and were then introduced into the test session in which they were given two blocks of twelve sentences each. A few sentences were given for practice before the test session. In the test session, stimulus sentences were given one by one by the experimenter and the children's task is to try to repeat a given sentence as precisely as possible. All children completed the task in a single session that lasted between 15 and 25 minutes. Each session was all recorded and later transcribed for analysis.

4. *Sentences* Twelve complex sentences were first constructed, comprising three each of the following constructions: a relative clause construction (R), an appositive clause construction (A), a complement construction (C), and an adverbial clause construction (Ad). The main and subordinate clauses of each sentence are all reversible except for the main clause of A. Each clause consists of a subject (NP *ga*), a direct object (NP *o*), and a predicate (a transitive verb) except for the main clause of C, which consists of a subject, an indirect object (NP *ni*), and a predicate. Each noun or verb in the sentences is used at most twice. Moreover, nouns are all animate except for the direct object of A. Then, to each of the twelve sentences was applied the rule of scrambling and the constituents dominating an embedded sentence were moved to the front, thus

yielding twelve scrambled versions. They are identified by attaching "s" to the label for each construction type: sR, sA, sC, and sAd. Construction types are as follows:

R:	NP ga	[NP ga Pred] N o	Pred.
	NOMINATIVE MARKER	ACCUSATIVE MARKER	
A:	NP ga [NP ga NP o Pred] tteiu	NP o Pred.	
		COMPLEMENTIZER	
C:	NP ga [NP ga NP o Pred] tte	NP ni	Pred.
		COMPLEMENTIZER	DATIVE MARKER
Ad:	NP ga [NP ga NP o Pred] to	NP o Pred.	
		SUBORDINATOR	
sR:	[NP ga Pred] N o NP ga Pred.		
sA:	[NP ga NP o Pred] tteiu NP o NP ga Pred.		
sC:	[NP ga NP o Pred] tte NP ga NP ni Pred.		
sAd:	[NP ga NP o Pred] to NP ga NP o Pred.		

The twenty-four complex sentences thus made were presented in a quasirandom order.

5. *Results and Discussion* For the purpose of analysis, noun phrases, particles, predicates, *-tteiu*, *-tte*, and *to* which may appear in stimulus sentences are numbered from left to right. E.g.,

A: NP ga [NP ga NP o Pred] tteiu NP o Pred.
 1 2 3 4 5 6 7 8 9 10 11

The responses of all subjects are scored (correct or incorrect) in terms of the numbered positions of constituents. Figure 1 shows the average numbers of correct responses to each numbered item for non-scrambled versions, while Figure 2 shows those for scrambled versions.

Six major trends emerged and they may be interpreted as in the following:

- 1) Subordinate clauses of the scrambled version tend to elicit more correct responses than those of the non-scrambled version ($p < 0.01$ significant).
- 2) The first (subordinate) clauses of the scrambled version tend to elicit more correct responses than the second (matrix) clauses $p < 0.01$. (1) and (2) may be taken to suggest that children process in a left-to-right manner, since the first clauses given to them produce more correct responses.
- 3) In the non-scrambled version, matrix clauses give rise to more correct responses than subordinate clauses ($p < 0.01$). (3) may suggest that when the left-to-right processing cannot lead children to a proper analysis of a given sentence, they tend to change the strategy, turning to a top-to-down analysis in accordance with the phrase structure they have constructed.
- 4) In the non-scrambled version, R and A elicit more correct responses than C and Ad ($p < 0.01$). Note that the former two constructions have a subordinate clause with the head whereas the latter two have a subordinate clause without the head. This may be accounted for by assuming that children form

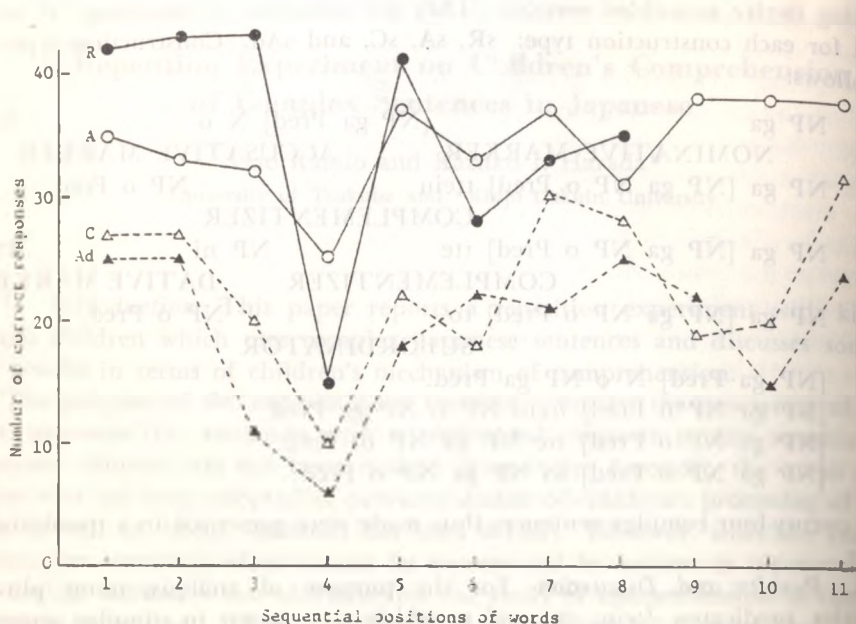


Figure 1. Sequential Positions of Words and the Number of Correct Responses: Non-Scrambled Versions

a proposition putting together a predicate and its argument(s). A head provides an argument, thus making the formation of a proposition easier.

5) In the serial position 4 of the non-scrambled version, a sharp drop of correct responses is observed. ($p < 0.01$ or 0.05). (5) suggests that children, in processing sentences from left-to-right, use a strategy in which NP-ga NP-o Pred is a canonical sentential form. This strategy was applied with the result that was observed.

6) In the serial positions of 7 and 8 of sC sentences, relatively small number of correct responses are shown ($p < 0.05$). Notice that in C sentences the subordinate clause functions as an argument for the matrix predicate but does not have the head. This makes it difficult to integrate the content of the subordinate clause into the main proposition, thus requiring a large amount of processing capacity, which may have led to the low level of performance in the matrix clause containing the positions of 7 and 8.

In conclusion, children of the ages tested here seem to have a well established processing mechanism in which the holistic structure of complex sentences is computed on the hand and the intraclausal structure of each clause is analyzed on the other. The former process seems to be represented by our results (1), (2), (3) and, in part, by (6) while the latter seems to be manifest in (4), (5) and (6). Moreover, our analysis suggests that they have a highly devel-

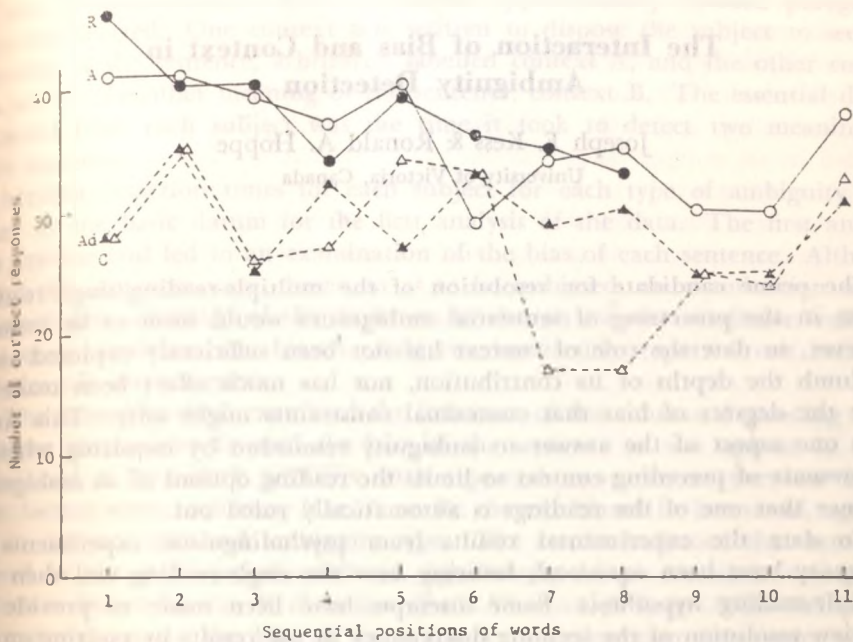


Figure 2. Sequential Positions of Words and the Number of Correct Responses: Scrambled Versions

oped knowledge of the syntax of Japanese on the basis of which their processing mechanism can function.

The Interaction of Bias and Context in Ambiguity Detection

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The prime candidate for resolution of the multiple-reading/single-reading debate in the processing of sentential ambiguities would seem to be context. However, to date the role of context has not been sufficiently explored so as to plumb the depths of its contribution, nor has much effort been made to grade the degrees of bias that contextual constraints might offer. This study offers one aspect of the answer to ambiguity resolution by inquiring whether the presence of preceding context so limits the reading options of an ambiguous sentence that one of the readings is automatically ruled out.

To date the experimental results from psycholinguistic experiments in ambiguity have been equivocal, favoring now the single-reading and then the multiple-reading hypothesis. Some attempts have been made to provide an overview resolution of the seeming discrepancy in the results by positing an in-sentence strategy that is dependent upon phrasal closure, but these have been directed at explaining ambiguity results in single, and thus isolated, sentences. The present study is directed at the role of context in resolving ambiguity with an eye to seeing whether multiple readings are still processed in the presence of context. One might speculate that an answer to the ambiguity controversy might be provided by the definitive role of contextual constraints in removing the possibility of a second or multiple readings for ambiguous structures. Thus, one might argue that many of the results supportive of the multiple reading interpretation are merely the result of dealing with isolated sentences in which the context was not sufficiently spelled out by more than a single preceding word. This methodological approach of employing single sentences is not the most effective in dealing with natural language inferences which demand a larger discourse reflective of actual language settings. It is obvious that this is an area which must be more fully investigated for if ambiguity does prove to be highly sensitive to certain contexts, then previous experimental results may be interpreted in light of the type and extent of context provided.

This experiment involved the detection of two meanings of an ambiguous sentences under three different conditions. In two of the conditions the sentences were preceded by a context, and in the third condition the sentences were presented alone without any prior context. Twenty-one sentences, ranging in length from 7 to 9 words were chosen from a collection of ambiguous sentences which had been used previously in studies of ambiguity and which were supposedly not strongly biased so that one meaning was much more likely to

be seen than the other. For the context approximately 70-word paragraphs were constructed. One context was written to dispose the subject to see one meaning of the sentence, arbitrarily labelled context A, and the other context was set for the other meaning of the sentence, context B. The essential datum collected from each subject was the time it took to detect two meanings of each sentence.

Median detection times for each subject for each type of ambiguity were taken as the basic datum for the first analysis of the data. The first analysis was unclear and led to an examination of the bias of each sentence. Although all of the sentences were chosen so as to be unbiased, an examination of the reported first meanings of the sentences in the no context condition indicated that every sentence was biased. Subjects consistently reported one meaning first for all the sentences.

The second analysis examined the influence of the context, depending upon whether the context favored the bias of the sentence or was against it. This necessitated a $2 \times 3 \times 2 \times 3$ ANOVA with repeated measures on the last factor. The factors were: sentence bias, A or B; context biased in favour of, against, or no context; sex; and ambiguity type. The results indicated that sentences biased in the A direction produced significantly faster detection times than sentences biased in the B direction ($F=12.58$, $df=1, 109$, $p<.001$). When the context was against the bias of the sentences, the detection times were significantly faster than when there was no context or the context favored the bias of the sentence ($F=6.38$, $df=2, 108$, $p<.01$). The differences in the type of ambiguity exhibited the fastest detection times when the ambiguity was in the underlying structure; the next fastest were for lexical ambiguity and the slowest for surface structure ambiguity ($F=11.91$, $df=2, 216$, $p<.001$). The interaction between the type of ambiguity and bias of the sentence, A or B, was significant ($F=6.81$, $df=2, 216$, $p<.01$). The above ease of detection order was true for the A sentences, not so for the B sentences; surface ambiguity was as easy to detect as lexical ambiguity. The interaction between the type of ambiguity and whether the context was in favor of or against the bias, as opposed to whether context was present, was also significant ($F=11.8$, $df=4, 216$, $p<.001$). Detection times were faster when the context was against the bias of the sentence as opposed to absence of context or context favoring the bias of the sentence for lexical and underlying ambiguity but not for surface ambiguity.

The results suggest that sentences have an inherent bias. A majority of subjects saw one meaning as opposed to the other meaning of the sentence in the absence of context. Furthermore, as a consequence of this, when the context is in line with the more common meaning of the sentence, then it takes longer to elicit the second reading of the ambiguous sentence. On the contrary, when the context is in line with the less common reading of the sentence, recognition of the other reading exhibits shorter detection times. It is as if the less common reading of the sentence was provided by the context while the more common reading is automatically provided by its a priori biased status. If the two

readings of ambiguous sentences were roughly equal in terms of their bias, then one would not expect to see significant differentiation in the detection of one reading as opposed to the other reading in the presence of context.

These results are consistent with a canonical access modification of the multiple-reading approach to ambiguous sentence processing. Sentences are decoded in an ordered access fashion with the inherent biased reading of the sentence playing a role as well as the context. While one can say these results can be taken as consistent with the multiple reading hypothesis, the processing of ambiguous sentences is not as simple as just entertaining two readings for an ambiguity. A realistic explanation of what goes on will call for attention to the interaction between context and the inherent bias of the sentence.

The Relative Accessibility of Cognitive Strategies

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This paper explores the application of cognitive strategies to language comprehension. Three particular language-independent strategies (CLOSURE, NORMAL FORM, and GIVEN-NEW) are formulated and applied to the relative clause structures of English and Japanese in an attempt to examine the predictions about relative processing difficulty that the strategies yield.

It is suggested that in utilizing CLOSURE, the hearer strives for recognition of familiar structures by using such cues as word order or order of semantic roles and attempts to obtain closure as early as possible (Bever, 1970). When using the NORMAL FORM strategy (Slobin, 1973), the hearer expects and anticipates that the unit he is processing will maintain a prototypic structural form with typical word order. The GIVEN-NEW strategy (Clark & Haviland, 1974) suggests that in the typical case, Given information precedes New, although when New precedes Given, special grammatical devices are used to signal the non-typical New-Given order.

These strategies produce different predictions about processing difficulty in English and Japanese. For sentences containing a subject (S), verb (V), and object (O), with a relative clause attached to either S or O, the following types of English and Japanese structures obtain:

Type	English	Japanese
SS	S(RP V O) V O	(O V)S O V
SO	S(RP S V) V O	(S V)S O V
OS	S V O(RP V O)	S (O V)O V
OO	S V O(RP S V)	S (S V)O V

In the type coding, the first letter represents the NP to which the relative clause is attached while the second indicates the grammatical role of the relativized NP (the RP in English) within the relative clause. CLOSURE predicts that non-interrupted clauses will be easier to process than interrupted ones, thus suggesting that English types OS and OO should be easier to process than SS and SO, while in Japanese SS and SO should be easier to process than OS and OO, for the same reason. The NORMAL FORM strategy predicts that sentences with relative clauses conforming to main clause word order should be easier to process than those with relative clauses violating main clause word order. Consequently, English types SS and OS should be easier to process than types SO and OO. In Japanese, however, the NORMAL FORM strategy does not predict a difference since the relativized NP is deleted in relative clauses,

and accordingly neither relative clause structure conforms to the main clause form. For the GIVEN-NEW strategy, no predictions of relative difficulty are made for English, since the relative pronoun is always at the head of the relative clause, in the Given position. In Japanese, however, the subject NP is far more frequently Given than an object NP, and the strategy predicts that SS and OS should be easier to process than SO and OO (Takahara, 1978).

These predictions are evaluated in light of the results of two psycholinguistic experiments. In each experiment, native speakers of the language (either English or Japanese) were instructed to make judgements about the relative naturalness of several sentences representing the four types SS, SO, OS, and OO. In the English experiments, 18 subjects each evaluated a set of 28 sentences, with seven replicates of each of the four types. In the Japanese experiment, 24 native speakers of Japanese evaluated a set of 12 sentences, with three replicates of each type. The results of the two experiments were analyzed by a two-way analysis of variance with the two factors of subjects and types (four levels). Subjects were not found to be significant in either study.

It was found that for the English experiment, types OO and OS were significantly easier to comprehend than SO and SS, thus supporting the CLOSURE strategy. Moreover, there was no significant difference between types OS and OO nor between SO and SS, and consequently the predictions of the NORMAL FORM strategy are not supported.

The results of the Japanese experiment, however, indicated that SS and OS were judged significantly more natural and easier to comprehend than SO and OO, thereby supporting the GIVEN-NEW strategy. While there was no significant difference between SO and OO, it was found that type SS was significantly easier than OS, thus supporting CLOSURE, but only when the GIVEN-NEW strategy was satisfied.

The results of the two experiments suggests that only one strategy, CLOSURE, was operative for English, while both GIVEN-NEW and CLOSURE seem to be at work in Japanese. The dilemma dissolves, however, if the GIVEN-NEW and CLOSURE strategies are placed into a hierarchy, with GIVEN-NEW higher than CLOSURE. In other words, GIVEN-NEW is first tried, and if it is satisfied, then CLOSURE is checked. The structures also satisfying CLOSURE are judged the most natural of all. In our study, all four of the English types satisfy GIVEN-NEW, and all are therefore checked for CLOSURE, while for Japanese only two types satisfy GIVEN-NEW, and only those two are checked for CLOSURE. The results of the two experiments suggest that the same hierarchy is operative in both languages.

It is also observed that the NORMAL FORM strategy seems to be rejected for English and Japanese. In fact, despite a language-specific constraint of verb position in the two languages, the relative order of S before O is common to the two languages and to some 98% of the languages of the world (Tomlin, 1979). It is at least conceivable that the relative positions of S before O in so many languages may result from a general application of the GIVEN-NEW

strategy.

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On the Notion of the Meaning of the Sentence

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Many Linguists and Logicians agree that two sentences often differ in their Linguistic meaning, though they share their intensions (truth conditions), i.e. they correspond to a single proposition (more exactly, to a single set of propositions with different reference assignments, see below). However, the research concerning the notion of Linguistic meaning is still scattered, and most authors attempt to contribute to this question without having an appropriate knowledge of what has been achieved by others, from European structural Linguistics to Californian and New-Zealandian intensional Logic. Our approach is based on the existence of an operational criterion for synonymy, which has been presented elsewhere¹ and may be summarized as follows:

Two expressions (Lexical or grammatical morphs, or syntactic constructions) *a* and *b* are synonymous (i.e. share one of their meanings) if and only if in every sentence containing *a* the substitution of *b* for *a* (if grammatically possible) yields a sentence having the same intension as the original sentence.

It can be checked that this criterion characterizes e.g. the morphophonemic differences between *Lit* and *Lighted* or *formulae* and *formulas* as synonymous. The same holds for such surface syntactic differences as those illustrated by (1) and (2):

- (1) (a) He permitted to smoke there.
(b) He permitted smoking there.
- (2) (a) He expects that Mary takes that train.
(b) He expects Mary to take that train.

On the other hand, our criterion gives a negative result for such examples as *John sold a car to Tom* vs. *Tom bought a car from John*. The expressions involved in these sentences are not synonymous, and thus the sentences as wholes also differ in their meaning, though they share their truth conditions. As a matter of fact, there are two semantically relevant differences involved here:

- (i) the verbs *sell* and *buy* do not fully correspond to each other: from *He is selling refrigerators to the inhabitants of northern Greenland* it does not follow that the inhabitants of northern Greenland are buying refrigerators from him;

1) Sgall, Hajičová and Procházka (1977); see also Hajičová and Sgall (1978).

(ii) also the topic-focus articulation is semantically relevant, and *Few painters sell paintings to many businessmen* is a different assertion than that of *Many businessmen buy paintings from few painters*.

The topic-focus articulation is one of the phenomena which have a pragmatic basis, though they are semantically relevant. The common treatment of this articulation is a typical example of how scattered the research in semantics still is. Even Kuno's paper presented at this Congress contains such misunderstandings as those connected with his conviction that it is "a normal state of affairs" that "the focus of the question is also the focus of the answer" (§7); others, who studied these questions earlier, noticed that this is not so, since such a question as *Do you know FRENCH?* is normally answered by *Yes, I DO*, or—without deletion—by *I KNOW French*, and hardly by *It is FRENCH I know*. Similarly, the answer to such a question as *Where did you put this KEY?* would be *I put it in my POCKET* rather than *I put this KEY in my pocket*; the specification of the focus of *wh*-questions is discussed by Hajičová (1976). A *yes/no* question such as *Did you stay in a hotel in LONDON?* cannot be characterized as being "a question about where (the hearer) stayed in London"; this characterization is suitable at most for the question *Did you stay in a HOTEL in London?*, which differs from the first in the position of the focus.²

One of the main pragmatic ingredients of the meaning of the sentence, or, more exactly, of the sense of an utterance (of a sentence occurrence), is the assignment of reference to the individual referring expressions included in the sentence. Lewis (1972) duly states that the reference assignment is based on the degrees of prominence of the items referred to. The degrees of prominence (salience, activation, foregrounding) can be characterized in a systematic way as a partial ordering of the items contained in the stock of knowledge shared by the speaker and the hearer(s), which changes during the discourse; the elements referred to by the focus of the just preceding utterance get a maximal degree of salience, which then can be maintained by the given objects being mentioned again (probably in the topic) in the next utterance, or else the salience fades away step by step.³ Thus e.g. a specific table can be referred to by *it* if it was mentioned just before, by *the table* if it has a high, though not maximal degree of salience (e.g. if not exactly the table, but say a dining room

2) A *yes/no* question may of course be answered by a minimal expression conveying the assertive modality of the verb; in English one of such minimal expressions consists in an auxiliary accompanied by its (pronominal) subject, but it should not be found surprising that in some languages it is the main verb that serves as such an expression; thus in Czech the equivalent of (was) *born* is used as a shortened answer to *Were you born in TOKYO?* Also the surface form of Russian answers to such questions clearly illustrates the fact that the verb is the focus of the answer, especially, if the verb has a narrower meaning: *Vy učilis' v LENINGRADE?* *Da, tam ja UČILAS'* (*Did you study in LENINGRAD?—Yes, I STUDIED there*) seems to be possible, if not typical. The object referred to by the focus of the question is now activated enough to be referred to by the topic of the answer, cf. below.

3) Now see Hajičová and Vrbová (1982), where the change of the hierarchy of salience during a discourse is examined.

was mentioned), by *the yellow table* if there is another table with a higher degree of salience, etc.

The four basic referential indices (*I, you, here, now*), an explicit description of which may make use of Montague's treatment, are assigned their reference immediately by the occurrence of the sentence: the hearer can identify the speaker, the time-point, etc.

Pragmatic elements have to be accounted for as included in linguistic meaning (e.g. the meaning of *today* includes *now* as well as *day*), and thus we do not consider pragmatics and semantics to constitute two separate levels of the language systems (Sgall, 1977). From this it does not follow (as Schank et al. assume in their paper presented at this Congress) that there is no "dictionary", only an "encyclopedia", or that there is no substantial difference between linguistic knowledge and common-sense knowledge. Linguistic systems, including linguistic meaning, should be distinguished from cognitive content, or truth conditions (intension, see above). However, it also appears as crucial to distinguish meaning (of a sentence) and sense (of its occurrences):

A sentence may have more than one meaning; each meaning of a sentence together with a specific reference assignment yields a sense (of an utterance). It is only a specific sense that can be assigned specific truth-conditions, i.e. a Carnapian proposition corresponds to a sense of an utterance, but the differentiation determined by 'sense' is more subtle than that based on 'proposition', as our examples above illustrate; thus the relationships between sentences and propositions can be described by means of the framework sketched in the Appendix.

This more subtle differentiation can be useful in describing the semantics of the so-called hyper-intensional contexts (e.g. belief sentences). The identity of intensions is a necessary condition for two expressions to be interchangeable 'salva veritate' in intensional (e.g. modal) contexts; thus e.g. *four* and *the square of two* are exchangeable in such sentences as the following:

- (3) The square of three is necessarily greater than four.
- (4) It is not necessary that the number of the planets is greater than four.

On the other hand, in belief sentences the identity of intensions is not sufficient; identity of sense is necessary here,⁴ cf. the following sentence, some occurrences of which certainly are true:

- (5) I believe that the number of chairs in this room is greater than ten, but I doubt whether it is greater than the square root of the product of the squares of two and of five.

⁴ It should be recalled that in metalinguistic contexts (which are not easily distinguishable from others) even the identity of sense is not sufficient for free exchangeability; in the general case no two different expressions are exchangeable 'salva veritate' in such contexts as *The expression x is longer than the expression y*, and also in such as *The intension of x is identical with that of y*.

Thus it can be stated that beliefs are neither *about sentences*, nor about *propositions*. Each of them is about a sense of an utterance. It is true that this approach makes it necessary to include 'meaning' and 'reference assignment' in some not yet specified way into the frameworks of intensional logic. But only in such a way it will be possible to give an explicit account of natural language with its paradoxes (sentences the meaning of which is such that there is no reference assignment for which the utterance of such a sentence were true for any possible world), synonymies, ambiguities and metalinguistic expressions.

Appendix

It seems possible to specify what may be called the semantic system of natural language as a 10-tuple of the form (Expr, Sent, Mean, Ref, Sense, Prop, U, W, T), where—if for every $f(x)$ we denote by $f(X)$ the union of all $f(x)$ for every $x \in X$:

Expr is a set of elementary expressions;

Sent is a set the elements of which are composed in a complex way (described by the grammar) from the elements of Expr; Sent is interpreted as the set of the (outer forms of) sentences of the language described;

for every $s \in \text{Sent}$, $\text{Mean}(s)$ is a set of labelled trees, interpreted as the set of the meanings of s , so that $\text{Mean}(\text{Sent})$ is the set of all meanings of the sentences of the language;⁵

U is a system of sets containing as its elements all entities that can be referred to (also linguistic expressions, cf. above), i.e. U is much more than the 'universe of discourse' known from many approaches to semantics; however, there are sets $U_i \subseteq U$, for $1 \leq i \leq n$, $n \in \mathbb{N}$; the sets U_i are interpreted as the sets of objects to which a referring expression can refer (the set of all dogs, of all English irregular verbs, etc.);

for every $m \in \text{Mean}(\text{Sent})$, $\text{Ref}(m) \subseteq U_1 \times U_2 \times \dots \times U_{k(m)}$, where $k(m)$ is the number of the referring elements in m , and an element of Ref is interpreted as an assignment of reference;

$\text{Sense}(m) = \{m\} \times \text{Ref}(m)$;

W is the set of possible worlds;

T is the set {true, false} (truth values);

for every $h \in \text{Sense}(\text{Mean}(\text{Sent}))$, $\text{Prop}(h)$ is a partial function from W into T.

The partial function $\text{Prop}(h)$ allows an assertion not to assign a possible world a truth value, if the presuppositions of the assertion are not satisfied in that world.

5) A generative description with a "semantic base" specifies first the set $\text{Mean}(\text{Sent})$ and then the inverse function of Mean as well as the composition of sentences and their meanings from elementary units.

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Neurolinguistic Feature Detection in Wernicke's Area

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This paper presents a neural model of a human cerebral cortical system likely to be arranged for the specific detection of phonological information from the environment, with implications for the theory of phonological distinctive features. In particular, we argue for a distinction between acoustic and articulatory features, with a priority for acoustic features.

As a first step toward a neural model of linguistic feature detection, we argue that the auditory association cortex known as Wernicke's area functions in a way analogous to the visual cortex. Since Hubel and Wiesel's demonstration in the cat and the primate¹⁾, we know that primary visual cortex contains neurons which are specialized in their detection of specific features in the visual world: specific neural arrangements responding to specific external stimuli, such as contours and moving lines. Some visual cells excite only if a certain line appears in a precise part of the visual field, other cells being indifferent to location. These detectors are part of the innate neural arrangement and physiological function of the visual cortex, and presumably represent a specialization across mammalian phylogeny as well.

Excellent work has been carried out, also in other sensory modalities such as in the somatic organization of somatosensory cortex, and auditory cortex, delineating a clear basis to view the initial stages of environmental information to be encoded into the nervous system through stimulus feature extractors.

For many decades a considerable extent of clinical data has established that Wernicke's area and Broca's area play some kind of principal role in language acquisition and use. Wernicke's area when destroyed has been shown to lead to a specific inability to comprehend phonological features. By any account of the basic neuroscientific data on the organization and function of sensory neocortex, it would be reasonable to assume a priority of importance in Wernicke's area, stressing that this system may be innately receptive in the extraction and analysis of elements of language occurring in the environment. Stated simply, in the normal hearing individual it would appear basic to assume that in response to environmental speech, feature detecting mechanisms are very likely to be involved in the encoding of human language, intrinsically

* T. M. Walsh was deceased on December 5, 1982.

1) Hubel, D. H. and Wiesel, T. N. (1959). Receptive fields of single neurons in the cat's striate cortex. *J. of Physiology* 148, 574-591. Hubel, D. H., and Wiesel, T. N. (1969). An anatomical demonstration of columns in the monkey striate cortex" *Nature* 221, 747-750.

specialized in the earliest organization of language development.

What then of the biological evidence on the organization of cerebral cortical tissue, subserving feature extraction? The history of this question is fundamentally the progression of the technical advances for exploring cortical organization and function. Before the end of the last century we knew that the six layers of cortex were more a dense zonal plexis of neuron cell bodies, dendritic surfaces, and complex arborizations of axons projecting to local and distant regions of surface and subcortical structures. In this century it has been the long axon cells (usually described as the pyramid cell) which have been mainly researched, providing important information toward an understanding of connectional neuroanatomical arrangements. However, in recent years the development of new methods for 3-dimensional cell reconstruction and detailed electromicroscopic analysis together with refinements in intracellular research, has allowed us to ask, to what extent should a cortical area be considered continuous, or conversely, how far can it be subdivided into separable functional units?

Work by Mountcastle²⁾, Hubel and Wiesel¹⁾, and numerous others over the past several years has substantiated the columnar arrangements of receptive field projections in sensory cortices. Szentágothai,³⁾ for example, has come to label the various shapes of neuron networks as "integrative units" of the neural tissue.

What is important, is that increasingly we are coming to see that each unit is sensitive to particular features in the sensory fields, each unit arranged in the circuitry to extract specific ranges of information from the environment. Classical concepts of neuron function and development are being revised as a result of a widening range of studies including recent intracellular recordings of postsynaptic patterns within these "units" traced to short axon cells, or better, local circuit neurons.⁴⁾ Local circuit neurons seem to have a continuous role in the establishment of new connections, and appear, as suggested by Jakobson⁵⁾ to bear the brunt of environmental integration pertinent to learning.

Our neural model suggests that phonological distinctive features are primarily auditory. This is in accord with Jakobson's version of distinctive feature theory.⁶⁾ Others, such as Ladefoged go to the opposite pole, arguing that phonological features are best defined in terms of articulation. The features as presented in Ladefoged's textbook⁷⁾ are all articulatory features except for one, the feature Sibilant. Jakobson's and Ladefoged's features are not isomorphic,

2) Mountcastle, V. B. (1957). Modality and topographic properties of single neurons of the cat's somatic sensory cortex. *J. of Neurophysiology* 20, 408-434.

3) Szentágothai, J. and Arbib, M. A. (1974). *Conceptual Models of Neural Organization*. Cambridge, Mass.: MIT Press.

4) Rakic, P. (1975). Local Circuit Neurons. *Neurosci. Res. Prog. Bull.* 13:3.

5) Jakobson, R. (1975). Development and evolution of type II neurons: conjectures a century after Golgi. In: *Golgi Centennial Symposium*. Santini, M., ed. New York: Raven.

6) Jakobson, R., and Halle, M. (1955). *Fundamentals of Language*. The Hague: Mouton.

Jakobson's acoustic features being strictly binary, and Ladefoged's articulatory features allowing in certain cases 3, 4, or 5 values.

There is actually neurobiological precedent for suggesting that both of these approaches may be on the right track and that the features utilized by the system may change according to the modality used. In their report on "Conceptual Models of Neural Organization,"³⁾ Szentágothai and Arbib state that "A point of importance here is that not only effector subsystems (that generate movements) but also preprocessing subsystems (arrays of feature detectors) are seen as being controllable by the perceptual system. Thus, we hypothesize that the "features" being utilized by the system change according to the perceptual task that is being carried out" (p. 335).

Four lines of evidence argue for the relative independence of auditory and articulatory features, and for the primary importance of the auditory features.⁸⁾ We present evidence from acquisition and infant perception, evidence from evolution and the abilities of other species, evidence from acoustic studies in laboratories, and evidence from Broca's and Wernicke's aphasia.

Acquisition and Infant Perception. It has become an axiom of child language acquisition that comprehension precedes production at all stages of acquisition. But if comprehension precedes production, how does this comprehension get started in the first place? How do infants learn to perceive and recognize utterances in human language? It has become clear in the last ten years that not only comprehension but perception of language sounds precedes production. A great deal of work done since 1971, most notably by Peter Eimas and his colleagues,⁹⁾ and ably reviewed by Juszyk,¹⁰⁾ has demonstrated that infants as young as the first month of life are able to perceive the basic contrasts of human language, and do so in most cases in categorical rather than continuous fashion. The prototype experiment by Eimas *et al.*⁹⁾ used a variation on the high-amplitude sucking technique in which rate of sucking on an electronic nipple was used to show that infants discriminate voice onset time in a categorical manner. Other experiments have demonstrated that infants perceive most of the other salient phonological distinctions of English.¹⁰⁾

Summarizing the work on infant perception, Juszyk states that "We now know that infants are able to perceive a wide variety of phonetic contrasts long before they actually produce these contrasts in their own babbling... Judging from existing data, it appears that infants are innately endowed with mechanisms necessary for making phonetic distinctions in any natural language, at least to a first approximation."¹⁰⁾

7) Ladefoged, P. (1982). *A Course in Phonetics*. 2nd ed. New York: Harcourt Brace Jovanovich.

8) Diller, K. C. (1982). Neurolinguistic considerations for phonological distinctive features. Paper for the conference on Phonological Distinctive Features, Stony Brook.

9) Eimas, P., *et al.* (1971). Speech perception in infants. *Science* 171, 303-306.

10) Juszyk, P. W. (1981). Infant speech perception: a critical appraisal. In: *Perspectives on the Study of Speech*, Eimas, P., and Miller, J. L., eds. Hillsdale: Erlbaum.

Evidence from Evolution. Once we begin talking about innate mechanisms for perception, it is worth looking for biological evidence from the performance of other species. It is clear that other species can understand certain aspects of human speech. It is said that well-trained dogs can distinguish up to 400 verbal commands. Chimpanzees, unable to articulate the sounds of human language, can understand spoken English words and simple utterances and respond appropriately in sign language. It is not clear how similar the chimpanzee perception of speech sounds is to human perception; the chimpanzees may rely heavily on secondary cues to distinguish English words. Yet we do know that even the chinchilla has categorical perception for voicing in alveolar consonants.¹¹ Chimpanzees do not have a functional Broca's area, but they have apparently evolved a reasonably functional Wernicke's area and have much in common with humans in auditory perception. Human perceptual abilities are species-specific, i.e., genetically specified, but build on the evolutionary base of the mammalian line.

Laboratory evidence. In contrast to the not very encouraging early attempts to identify distinctive features in sound spectrographs, Blumstein and Stevens¹² have shown in recent work that if one looks at *integrated* acoustic properties instead of individual components of the acoustic signal, by sampling the spectrum shape at particular times, one can indeed find invariant properties corresponding to distinctive features. "Thus for example," they say, "although individual components of the acoustic signal, such as the burst, onset frequencies of particular formants, or directions of formant transitions, do not provide invariant cues to place of articulation in stop consonants . . . , the shape of the spectrum sampled over a particular time interval at the release of the consonant does seem to provide an invariant pattern."¹³

Stevens and Blumstein do not deny the importance of contextual secondary cues for accurate speech perception, and they acknowledge the built-in redundancy of the system which involves what they call "acoustic fine structure" (p. 34). But they do provide evidence for invariant acoustic correlates for at least some of the distinctive features which could be perceived directly as primary properties. On the basis of this evidence they postulate innate property-detecting mechanisms as part of the infant's sound-reception system, mechanisms of the kind "that are needed to get the system started" (p. 33).

Evidence from Aphasia. Since the time of Broca and Wernicke, more than a century ago, the evidence from aphasia has been clear about the separability of perception and articulation in language. Let us review briefly the syndromes of Broca's and Wernicke's aphasia.

11) Kuhl, P. K. and Miller, J. D. (1975). Speech perception by the chinchilla: voiced-voiceless distinction in alveolar-plosive consonants. *Science* 190, 69-72.

12) Blumstein, S. E. and Stevens, K. N. (1981). Phonetic features and acoustic invariance in speech. *Cognition* 1, 25-32.

13) Stevens, K. N., and Blumstein, S. E. (1981). The search for invariant acoustic correlates of phonetic features. In Eimas and Miller (see note 10).

Broca's aphasia results from a large lesion in motor association cortex in the frontal lobe, including Broca's area and certain neighboring areas. Speech is effortful and slow, and characterized by an "agrammatism" in which the content words are more in control than the function words. Comprehension is good except where grammar is crucial.

Wernicke's aphasia, on the other hand, results from a posterior lesion, and is characterized by fluent seemingly grammatical speech that is repetitious and empty. There are many errors of word choice and transpositions of sounds. Comprehension is poor. The chief point to notice about Wernicke's aphasia is that well-articulated speech runs on at a fluent rate without feedback or control from the auditory comprehension area. In spite of the fact that the motor control areas are fully intact, there are fairly frequent "literal paraphasias" or phoneme substitutions, 40% of which involve only one distinctive feature.¹⁴⁾

Broca's aphasics have five times as many phoneme substitutions as Wernicke's aphasics, as we would expect from a lesion in the motor speech area. But comprehension for Broca's aphasics does not seem to be impaired by this gross phonological deficit (comprehension in Broca's aphasics seems to be impaired only by grammatical complexity).

Thus lesions in auditory processing areas will affect production of phonetic segments, but lesions in the motor speech areas apparently do not affect perception of phonemes, giving us more evidence of the semi-independence of articulatory and acoustic features and evidence for the priority of the acoustic features.

Conclusion. Our four lines of evidence, then, evidence from infant perception and acquisition of language, evidence from evolution and other species, evidence from acoustic studies in laboratories, and evidence from aphasia, all support a neural model of feature detection in Wernicke's area which allows language to get going and allows language to be perceived it is learned. This model and these lines of evidence point to independence of acoustic and articulatory phonological features, and suggest a certain priority for the acoustic features.

14) Blumstein, S. (1973). *A Phonological Investigation of Aphasic Speech*. The Hague: Mouton.

Russenorsk: The Russo-Norwegian pidgin. New findings.

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1. Introduction

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1.0 Russenorsk (RN) is the pidgin or trade jargon which was used in Northern Norway (Finnmark and Tromsø) by Russian merchants and Norwegian fishermen during the period of the Pomor trade, from the second half of the 18th century till the Russian Revolution in 1917. This paper reports on and summarizes what we consider the most important results of our study of this pidgin since 1979. (For a detailed account of the project, see Broch/Jahr 1980, 1981, 1982 a,b.)

1.1 In several ways RN is different from other pidgins which are based on European languages. Most important is the fact that there was no significant social difference between the users of the pidgin, and hence no social distinction between the two major languages on which RN was based, Norwegian and Russian. This feature of RN is in sharp contrast to, for example, English- or French-based pidgins, where the social difference between the users is reflected by the fact that the European base language dominates the pidgin language in a way which is not comparable with the relationship between Norwegian and Russian in RN.

1.2 The characteristic features of RN can be briefly summarized as follows: The phonology is based on the Norwegian and Russian sound systems, but simplified so that f. ex. Norw. [h] > RN [g] because Russ. lacks [h], and Russ. [tʃ] > RN [ç] because Norw. lacks [tʃ]. Russian consonant clusters are also simplified: RN *drasvi* < Russ. *zdraustvujte*. The most frequent pronouns are *moja* and *tvoja* (1st and 2nd personal and possessive pron.). The verbs lack markers of tense, aspect and person. The suffix *-om* is a general verbal marker, though it is not always used. Nouns have no declination and the suffix *-a* seems to have a noun marking function, like *-om* for verbs. There is no copula, but *ligge/liggom* (= to lie down) or *stannom* (= to stand) are used with existential meaning. The vocabulary is mostly of Norwegian or Russian origin, but some words derive from other languages (Low-German, Dutch, English, French).

2. Short survey of RN syntax

2.1 Earlier descriptions of RN seem to regard the language as being almost without syntactic rules (e.g. Broch 1927:231). This is obviously incorrect, although the syntactic possibilities are quite restricted. The largest variety seems to have developed in interrogative sentences, which is not unexpected.

considering that RN was used to ask questions about prices and barter for merchandise.

2.1 Sentences are generally combined paratactically by means of juxtaposition or by conjunctions:

1. moja på anner skip nåkka vin I drank some wine on another ship,
drikkom,
(I on other ship some wine drink,)
så moja nåkka lite pjan, så . . . etc. then I got a little drunk then . . .
(so I some little drunk, so . . .)
2. Principal grot pjan i på kâna The captain was very drunk and beat
kludi,
(skipper very drunk and on wife
beat,)
ja kanske på vater kasstom. and maybe he'll throw her in the
(and maybe on water throw) water.

Dan I. Slobin (1977:201) claims incorrectly that RN had no conjunctions and no embedding. Coordinating conjunctions are shown in sentence 2, and sentence 3 shows both a subordinating conjunction and embedding:

3. moja smotrom kak ju pisat I saw that you were writing
(I see that you write)

The subordinating conjunction *kak* and the coordinating conjunctions *ja* and *jes* are found in sentence 4:

4. kak ju vil skaffom ja drikke te, If you want to eat and drink tea, then
davaj
(if you will eat and drink tea,
please)
på sjib tvoja ligge ne jes på slipom come on board and lie down to sleep.
(on ship you(r) lie down and on
sleep)

Thus, Slobin's claim that RN had no conjunctions and no embedding is wrong. *Kak* is used on the sentence level to express subordination in the same way that the preposition *på* is used on the syntagma level to express all dependence relationships:

5. a) possessive: klokka på ju ('watch on you', your watch)
b) local: mala penga på lomma (little money in ('on') the pocket)
c) temporal: på morradag (tomorrow)
d) directional: på Arkangel reisom ('on A. go', go to Archangel)

2.2 RN word order is normally SVO as in Norwegian, but if the sentence contains an adverbial, the verb usually occurs in final position:

- | | |
|--|----------------------|
| 6. moja kopom fiska
(I buy fish) | I will buy some fish |
| 7. moja tri vekkel stannom
(I three weeks stay) | I stayed three weeks |

2.3 We find declarative, interrogative and imperative/hortative main clauses in RN. It is reasonable to assume that sentence intonation was often used to signal the syntactic status of the sentence.

2.3.1 Of the three types of main clauses, the interrogative sentences are the most interesting ones, as five different types can be distinguished:

a) With the question word *kak*:

- | | |
|---|---------------------|
| 8. kak pris på tvoja?
(what price on you?) | What is your price? |
|---|---------------------|

b) With rising intonation:

- | | |
|---|--------------------|
| 9. tvoja fisk kopom?
(you fish buy?) | Will you buy fish? |
|---|--------------------|

c) With a modal verb:

- | | |
|---|------------------------------------|
| 10. vil ju på moja stova på morradag
skaffom?
(will you on my house on tomorrow eat?) | Will you eat at my place tomorrow? |
|---|------------------------------------|

d) With the suffix *-li*, which can be suffixed to different word classes:

- | | |
|---|--------------------------------------|
| 11. mangoli år tvoja?
(many years you?) | How old are you? |
| 12. kak vara ju prodatti?
(what goods you sell?) | What kinds of goods are you selling? |

e) With the word *kanske* (maybe) in sentence initial position:

- | | |
|--|-------------------------------|
| 13. kanske lite tjai drikkom?
(maybe little tea drink?) | Do you want to have some tea? |
|--|-------------------------------|

3. The life span of RN

3.1 RN is first referred to between 1812–1814. The governor of Finnmark in this period wrote several memoranda about this northernmost county. He notes, after first having commented on Norwegian, Lappish and Finnish: "A fourth language is also spoken in Finnmark, put together from Norwegian, Russian, Dutch, German, Lappish and maybe Finnish. One could call it the Trade Language (*Handelssproget*), because it is used by traders in order for them to understand each other. However, it is only used when dealing with Russians" (Jarlsberg 1887:152).

This reference to RN is the earliest we have found. Broch (1927) had his

first example of RN from 1842. Lunden (1978) was able to move this even further back to 1827. In our material, however, RN words occur as early as 1785 and 1807.

3.2. A lawsuit in 1785 provides us with the RN word for a "Russian": *rusmann*, spelt "Rusmand". In the Legal Records, Russians are usually called "Rus(s)" or "Rys(s)", this is the only time the RN word for a Russian is used (Broch/Jahr 1981:64 f.).

3.3. In the Legal Records for Finnmark for 1807, we have found another early occurrence of a RN word. A Russian fisherman is quoted directly as shouting "Krallum". *Krallum* (< Russ. *krast'*, pret. *kral-*, = (to) steal) later occurs several times in the RN corpus, and we see the first occurrence of the verbal marker in RN, here spelt *-um* (Broch/Jahr 1980).

3.4. In view of these findings and of the governor's reference in 1812-1814, we must assume that RN developed during the second half of the 18th century, which is about half a century earlier than formerly thought.

3.5. In our examination of RN, we have found an affirmed life span of 141 years. A pidgin rarely lasts that long without being creolized. The reason why RN did not develop any further as a pidgin language and did not expand functionally but more or less exhibited a grammatical and lexical minimum is that it was used almost exclusively in connection with the seasonal trade during the summer months. This circumstance is another special feature of RN as compared to other pidgins. After the turn of the century the socio-economic conditions for RN gradually disappeared. Presumably, ability to speak RN became less extensive during the final decades of the Russian trade. The Russian Revolution in 1917 put a definitive end to this trade and hence also to RN.

4. Social evaluation of RN

4.1. Pidgin and creole languages usually have a low social status in the speech communities in which they are used. It is widely believed that these languages are 'corrupted', 'unstructured', 'babytalk' and so on. RN, however, seems to have enjoyed a high status in society up to the middle of the 19th century.

4.2. Around 1850, there seems to have been a change in the social evaluation of RN (Broch/Jahr 1982 a). Before 1850, RN was commonly used by Norwegians in dealing with the Russians. But after 1850, RN was used mostly by fishermen, while the merchants learned Russian by spending one or two years with business colleagues, especially in Archangel. It is striking that the available reports and evaluations of RN on the whole are more positive before 1850 than after. The reason for this is that as long as the merchants had to use RN the pidgin was socially accepted by the local upper class as an equivalent to other languages. But when the merchants started learning Russian and, as a result, RN was limited to the common fishermen, the use of RN was likewise devalued socially. This in turn also influenced its status as a language as such.

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Variation/Variability within a Linguistic Contact Domain

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Current research and consideration of bilingual setting, can be attached to two main sources: a) theories with a psychological background, and b) theories with a linguistic and sociological (i.e., sociolinguistic) background. As it is already known by scholars of the field a non fluent bilingual is "the second language user who possesses sufficient skill with a language for successful basic communication but who nevertheless is perceived by others and by himself as not possessing native-like control of the language". (Segalowitz and Gatbontom 1977:77) whereas a bilingual dialect is "the intermediate system that arose through bilingual contacts (this writer's bilingual dialects)" (Haugen 1973).

The competitive linguistic situation to which this paper is devoted can be partially linked to both concepts at a time. One can discuss whether our informants are non-fluent bilinguals or not, depending on one's view of the situation, i.e. which can be considered the first language and which the second.

Our informants are monolingual in DPU (Uruguayan Portuguese Dialects) and have a relative command of either Spanish or Portuguese.

DPU is a bilingual dialect which has emerged through close contact of Portuguese and Spanish; an intermediate system thus arose and as Haugen clearly states (1973:534) it can be "Characterized by a combination of additions [intrinsic specific properties not available neither in Portuguese nor in Spanish] and deletion [most of them also present on both source languages].

The actual situation, can be nowadays summed up as follows: people living in the border use a bilingual Portuguese-based dialects which evidences strong influence of Spanish grammar and lexicon. We have recorded 140 informants with little or null education (illiterate) who use the DPU as home language. As a result the techniques of elicitation used true vernacular arose. About 60% of our informants were children, a very important fact indeed, which evidences the vitality of DPU in the area; obviously children can not learn it but from their parents and peers.

We have proposed elsewhere (Elizaincín 1981 a y b) to call *variability* the phenomena which emerges as a consequence of contact, thus confining the already standard term *variation* to the field of true sociolinguistic analysis of standard languages.

Now, the main methodological and theoretical tools have been used for the study of our corpus. These are: *structural instability*, *variability* and *community vitality*. By *structural instability* we mean the *internal variation* of DPU grammars which can be easily observed by just looking at some items

in the corpus; thus the alternance between pronouns *Yo-Eu* "I" goes back to this distinction. This instability can be related, most times, to variation within substandard Portuguese, or Spanish, or both. Being the case that one and the same variable feature is observed both in Spanish and Portuguese, it is almost sure that it will also appear in the DPU grammar. For example, lack of concordance in plural noun phrases is one of the most prominent features of DPU grammars.

Variability itself can be viewed as a consequence of structural instability; therefore something like the classical dichotomy competence/performance is at use. For this case it can be conceived as instability/variability.

Variability is observed within the speech of a single informant but also matching the ages of informant of the 6 different geographic places where the recordings took place. For example, there is an evident variability in the general linguistic behaviour of Artigas and Rio Branco speakers as to the rendering of the thematic vowel of 1st Plural Present Person of verbs ending in -ar: the form *trabalhemo* "we work" (alternating with *trabalhamo*) reaches the following percentages: Artigas 60%~Rio Branco 0%.

Therefore, interpretation of these figures allow us to establish a great variability between the two places: on the contrary, observing only Rio Branco, there exists no variability at all; in other words, Rio Branco speakers of the DPU use only the form *trabalhamo* and never *trabalhemo*.

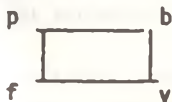
Lastly, *communicative vitality*. By this term we mean native-like fluency in DPU. When faced to institutional domains such as school (where only Spanish is allowed and DPU severely forbidden) children remain silent and inactive; in case the teacher compels them to use Spanish their performance is poor and hesitating.

Let us now provide some examples of DPU.

Rendering of Spanish or Portuguese phonemes vary accordingly to coincidence or not of phonemes in the phonemic systems of both languages. Spanish lacks a voiced labiovelar *v*, which Portuguese possesses. Spanish sets up a triangular system with two correlations engaged: continuant/non continuant~voiced/unvoiced on the labi(odent) al order:



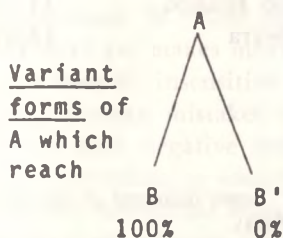
Portuguese sets up this system:



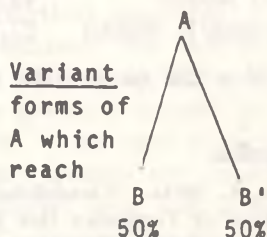
As a consequence, DPU also shows a voiced labiovelar which freely alternates with voiced bilabial *b*.

As a means of measuring variability we compute forms of a certain phenomenon and (if possible) present them as binary opposition. Thus, percentual differences between percentages reached by each of the opposing forms (i.e., the variable forms) state the length and nature of variability. As an example, situation (1) is a case of maximum percentual difference and minimal variability; supposing that B could be safer related to Portuguese, then in this particular case conclusions will be obvious; only Portuguese is the basis for this segment of DPU grammar. Case 2 is the opposite possibility.

(1) Unstable feature



(2) Unstable feature



Null percentual difference between variant forms automatically entails the maximum variability. In case B is attached to Spanish and B' to Portuguese variability is considered as a free verbal behavior without (at least, not up to the moment) any special constraint of linguistic context.

Let us now exemplify our methodological proposition: rendering of the preposition Spanish Portuguese *para* "to". Although "*para*" performs some different functions in Spanish and Portuguese (some of them, anyway, coincide), we can compare them in the following way. (Let us say, first, that substandard Spanish form of *para* is *pa* and Portuguese is *pra*).

In all the sample *pra* reaches 47,01, *pa* 35,86 and *para* 13,54. Three forms are observable in all places. Examples:

"Sempre me deşan algo pronto *para* mi"

"Se trae *pra* fora"

"Us puntero tan *pa* faşer gol"

The following Table 1 shows percentual distribution of both *pa* and *pra* (that is the most frequent within the total sample).

Applying the percentual difference technique, the places can be ordered (decreasing order of percentual difference; therefore increasing of variability: Table 2).

Rivera shows the greatest variability whereas Artigas the least.

This way of presenting data must be accomplished for every item considered. It helps us to view the irregular performance of DPU speakers in an objective way and, last but not least, to establish 1) which aspect of the DPU grammar is open to influences 2) which city (village, etc.) presents the most variable

behavior with regard to data collected.-

Table 1

Places	Pa	Pra
Artigas	22	78
Rivera	56,82	43,18
Tranqueras	41	59
Vichadero/M. de Corrales	64,10	35,89
Rio Branco	42,85	57,14
Aceguá/I. Noblía	27,77	72,22

N = 208 ($\alpha = 0, 01$)

Table 2

Artigas	56
Aceguá/I. Noblía	44,45
Vichadero/M. de Corrales	28,21
Tranqueras	18
Rio Branco	14,29
Rivera	13,64

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Cross-Cultural Sociolinguistic Profiles Misreadings and Misunderstandings

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INTRODUCTION The cultural/social knowledge in communication cannot be overvalued. If a language learner acquires excellent phonology, grammar and vocabulary yet makes mistakes in appropriate behavior, the "mistakes" may be taken as boorish, insensitive or so on.¹⁾ Learners who acquire the appropriate behaviors yet make mistakes in pronunciation or grammar usually get along well without such negative readings. The behaviors which need to be learned for total communicative competence are many; only samples are treated here, including such nonverbal aspects such as effect of dialect, style, and so on. The approach to these features is through television, in particular the American TV shows dubbed into Japanese.²⁾ The motivation for this approach is that the behaviors appropriate to actors for the camera (such as in close-ups vs. long shots, for example) do not always match the natural setting and the Japanese³⁾ or other learner of English may learn an erroneous set of behaviors. In addition the dubbed versions sometimes do not reflect the sociolinguistic dimension⁴⁾ of the original, as will be explained below. This paper, then, explores the potential misreadings and misunderstandings in cross-cultural sociolinguistic aspects of television.

SOCIOLINGUISTIC PROFILE For the purposes of this paper, a sociolinguistic profile⁵⁾ is the set of social/cultural features associated with a certain set of linguistic/sociolinguistic behaviors. A composite sociolinguistic profile can be made using testees from all walks of life, ages, and so on. It is especially interesting to note that the above reading also takes place across cultures even

* This research was funded by grants from Trinity University. Thanks to Dean C. Boyd.

1) Dr. Gordon Fairbanks of the University of Hawaii is credited with Fairbanks' law: a person with limited linguistic competence can err nonverbally and be ignored or forgiven, but a polished speaker who errs nonverbally is assumed to know better and is held responsible.

2) The first stage of this research was funded by the Social Science Research Council in 1973 in Tokyo in preparation for the 2nd U.S.-Japan Sociolinguistics Committee meeting. The research is reported in Hoffer (1974).

3) Few Japanese have enough experience in American social settings but do have the opportunity for 1000's of hours of American TV viewing during their learning.

4) The translators often work only with the written text. Vocal qualifiers, gestures, and so on which affect the communication are thus not factored into the translation. See Hoffer (1974).

5) Profiles take different forms for different types of research. Here only the general features are discussed.

when the testees do not understand a word of the original language and when only the audio tape is used.⁶⁾ One especially interesting area of research seeks to isolate the diagnostic feature(s) which triggers the profile. In the longer form of this paper, the average sociolinguistic profile for certain TV shows and actors is used to show how cultural misreadings and misunderstandings occur even though the word for word translations from English to Japanese are among the best in the world. The focus herein is on the vocal/verbal and the nonverbal/kinesic features. These social features of communication need much more study; translation should strive for total communicative competence and not stop at linguistic competence.

SOCIOLINGUISTIC STUDY OF TELEVISION Dubbed TV shows often miss the sociolinguistic meaning, including such items as the social level, the humor types, the power/solidarity information and so on carried by the vocal features and the nonverbal behavior. The problems are multiplied by the skewed nonverbal features used in such a drama setting. The research here investigated 27 American TV shows on Japanese television (60 shows appeared during the research period of January-February, 1982; these 27 were videotaped) for the verbal/NV sociolinguistic profiles which would be appropriate for the actors in those series. Note that "Police Story", "Dukes of Hazzard", "Trapper John" and others include DIALECTS, which have social features attached and that STYLES are used with good effect in "Quincy" and other shows. The NV behavior of the actors on all these shows is usually *much* different from the real world situation and provides skewed sociolinguistic profiles. Space limitations limit the items here to a brief list.

PROXEMICS The use of space on TV is dictated not by "real" proxemics but by the demands of the drama/camera eye. "Mission Impossible", "Little House on the Prairie", and others use at times close personal space for formal, public communication or up to 12' for intimate/casual speech. Often 8" between noses with 180 degrees head orientation accompanies public conversation. **BODY ORIENTATION** is skewed by the need for the camera to view all actors and often all interacters are shoulder to shoulder, as in some "Quincy" episodes. A few shows, such as "Trapper John" work at more "real" body orientation. **EYE BEHAVIOR** is perhaps the most skewed NV behavior on TV; it is almost always skewed to an open-eyed stare by both speaker and listener. Learners copying such behavior make native speakers uncomfortable. Only a few top actors can approach relatively natural eye behavior; at times Larry Hagman of "Dallas", Pernell Roberts of "Trapper John" and occasionally Jack Klugman of "Quincy" handle it well. **TOUCH BEHAVIOR** on TV is either more frequent or nonexistent in most shows and is a poor model for a Japanese observer. A Japanese who used the touching behavior seen on TV might be highly offensive to Americans. **LISTENING SOUNDS AND MOVE-**

6) Tetsuya Kunihiro of Tokyo University and this author have carried on such research since the former spent a semester at Trinity University in 1975.

MENTS which are appropriate to interactants rarely exist on TV; a learner who imitates such behavior might be read as inattentive, or perhaps poor in English, or even rather dumb. Many other NV behaviors are omitted for space limitations. The point here is that NV behavior is a basic part of communicative competence, yet absent or skewed on TV.

SOCIOLINGUISTIC PROFILES ACROSS CULTURES The relative lack of contact with Americans by Japanese prevents the development of a bikinesic system in most cases. The system which might be learned from the abundant TV shows or the similar plethora of American movies (102 during the test period) would produce an English speaker whose own sociolinguistic profile would be skewed and often misread and misunderstood by the American interactants in any real situation. The learned system and the observed profiles thus are both rather skewed and contribute little to cross-cultural communication and, in fact, perhaps impede such communication. It is precisely the *potential* of TV for modeling appropriate behavior and for helping define accurate sociolinguistic profiles which should be used in the future; current TV shows are a hindrance.

SUGGESTIONS FOR THE FUTURE Education of accurate cross-cultural sociolinguistic profiles through TV would counter the often negatively stereotyped profile already present. Japanese who now appear on American TV in ads usually stay in groups, have cameras and say "Ahhhhh!" a lot. No accurate profile can be learned from such exposure. A series of well-done shows of Japanese interacting with Americans would do more for cultural profiles than any number of books *about* Japanese and their culture. The second suggestion follows from the first. Businessmen, diplomats and so on would profit from workshops on appropriate behavior in the cross-cultural setting and TV could be a great help in such cases. Study of Japanese behavior with Japanese is interesting and important but the cross-cultural setting is different. Finally, the pervasive influence of TV these days requires much study. The study of cross-cultural TV sociolinguistics may provide a distancing, a controlling effect on intracultural studies and thereby provide an added objectivity. Studying misreadings and misunderstandings across cultures may make it easier to specify them within a culture in a manner which allows us to compare the total system of person to person communication.

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Two Functional Aspects of Politeness in Women's Language

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The universal hypothesis that women are more polite than men in their use of language has been explored by studies on English (Lakoff 1975, Zimin 1981), Japanese (Peng et al. 1981, Ogino 1982), Tzeltal (Brown 1980) and some other languages. There are various devices to make speech polite, e.g. the use of hedges, but the most notable device used by women is the use of high level/formal linguistic forms. In English we have seen evidence of women's use of high level phonological forms such as the use of a post-vocalic /r/ studied by Labov (1972), Trudgill (1972) and many others.

This paper focuses on high level linguistic forms found in women's speech and proposes that they should be looked at from two functional aspects. I wish to claim that women's use of high level forms not only functions to express the speaker's polite attitude toward the addressee but also emotively functions to express grace, a quality of femininity.

Among several pieces of evidence in Japanese I cite Ogino's work in which degree of politeness is computed from the levels of linguistic forms used by about 500 people in Tokyo. Diagram 1.) We find in the diagram that women's blocks (1b) are higher, which means that women are more polite. The most notable difference is in the height of the spouse blocks. To investigate the

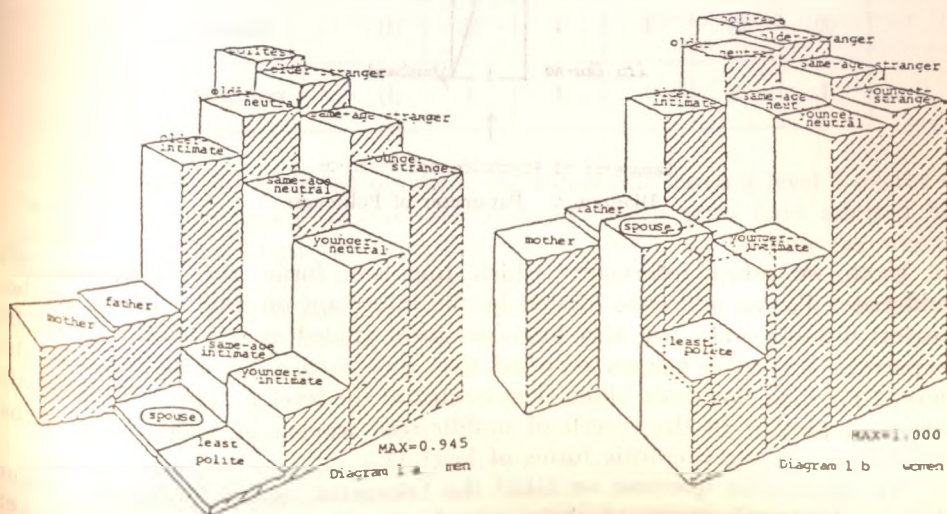


Diagram 1. Degree of Politeness Used by Men and Women

reason for this phenomenon we conducted the following four-step interview with informants. We first hypothesized that part of the reason for the difference is due to the emotive function of language to express grace.

Assuming that we have a hierarchy of variations of linguistic forms which can be arranged along the politeness axis, we asked informants to rate the variation forms according to five levels of politeness scales. Second, we posited a parameter of psychological distance which is graded from levels 5 to 1. This parameter is the scale of politeness with level 5 the most polite and level 1 the least polite. We asked informants to locate people they deal with in their daily lives on the scale of politeness. Then, we connected the results of the first and second questionnaires with the politeness parameter. (Diagram 2.)

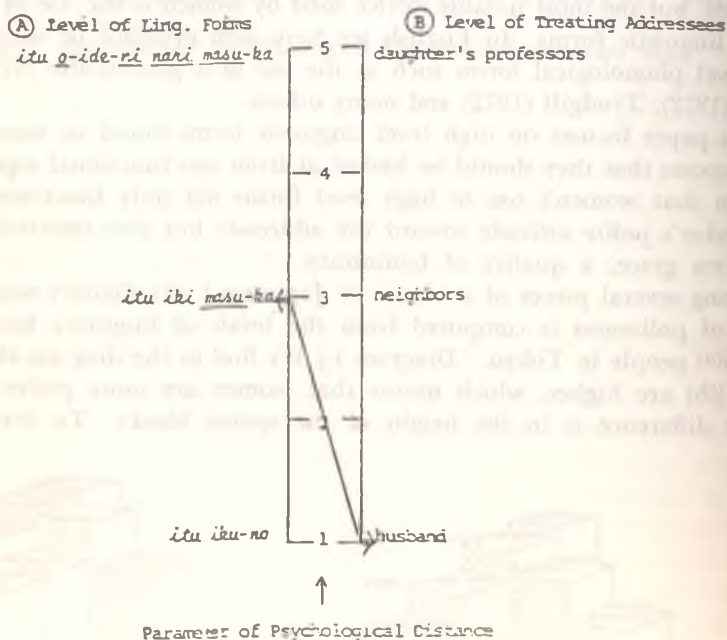


Diagram 2. Parameter of Politeness

Next, we asked informants which linguistic forms they used for each addressee. For an addressee located by the informant on level 1 of the psychological distance scale, use of a linguistic form graded as the level 1 would be expected. But some women reported that they used a linguistic form of the level 3 for their husbands whom they located at the level 1. This is a commonly observed pattern in the speech of middle-aged women of middle class. Why don't they use the linguistic forms of level 1?

To answer this question we asked the informant, as the fourth step, about five psychological aspects of their attitude they have toward each addressee:

(1) status/age difference, (2) acquaintance, (3) intimacy, (4) wanting to show grace and (5) consciousness of opposite sex. The former three determine the degree of psychological distance and the latter two affect the speaker's choice of higher linguistic forms. (There are a number of other factors which are at work in determining psychological distance and the level of politeness of linguistic forms, but only those relevant to this discussion are presented.) Informants were asked to judge intuitively the degree of each of these aspects of their attitude toward each person they deal with. The degrees were rated from 5 to 1 so that the higher number corresponded with the larger psychological distance and with the higher level of linguistic form. The result of questioning a typical housewife is shown in chart 1.

Chart 1 Degree of Psychological Factors

SPEAKER	PSYCHOLOGICAL FACTORS ADDRESSEES (level)	FACTORS FOR PSYCHO. DISTANCE			FACTORS FOR HIGH FORMS	
		STATUS/AGE DIFFERENCE	LACK OF ACQUAINTANCE	LACK OF INTIMACY	WANTING TO SHOW GRACE	CONSCIOUSNESS OF OPP. SEX
M (25)	daughter's professors (5)	4	4	4	5	1
	beautician (2)	1	4	4	3	3
	husband (1)	②	1	1	③	⑤
	daughters (1)	1	1	1	2	1

As we see in the encircled numbers, the informant shows a level 3 degree of "wanting to show grace" toward her husband, which must have affected her choice of a higher linguistic form than level 1. Next we see that the woman shows a level 5 degree of "consciousness of the opposite sex" toward her husband. This factor makes a woman behave as women are expected to behave according to the stereotypical image of an ideal woman; that is, to express femininity, which consists of grace and sweetness, among other things. The use of high level linguistic forms, which results from circumspection, adds grace and dignity to the tone of speech. Thus, the high degree of a woman's consciousness of femininity can be interpreted as another reason for her use of higher linguistic forms.

As further evidence we find similar discrepancies between the levels of

politeness of linguistic forms and the treatment of people when a woman speaks to a beautician at a beauty parlor or to a woman at PTA meetings, where the expression of femininity is encouraged.

Additional evidence is the special use of honorifics called "beautification honorifics", which are mainly used by middle class women. Honorifics, a morphologically well-defined system, are used to express the speaker's politeness toward the addressee.

- (1) *sensei no o-tomodati* (honorifics)
teacher poss friend
'teacher's friend'
- (2) *watasi no o-tomodati* (beautification honorifics)
I
'my friends'

In (1) the honorific prefix *o-* is attached to indicate that the speaker expresses politeness toward the teacher. But, in (2) the honorific prefix is associated with the speaker. In (2) the honorific prefix no longer functions to show politeness, but only functions as a beautification of the language through the use of an honorific, a high level form.

In this paper I have tried to show the necessity of looking at politeness in women's speech from two functional aspects. This is important in investigating the social significance of women's use of high level forms. Such use of high level forms has been explained as a reflection of women's subservient position in society, but it should be also taken as a reflection of a quality of femininity.

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Indian English—A Critique of Ethnographic Analysis

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0. This paper discusses a BBC TV programme entitled *Crosstalk* that dealt with the communicational dimension of interethnic contact between white Britons and minority group members such as Asian immigrants. It was produced in cooperation with the Government-sponsored National Centre for Industrial Language Training and John Gumperz and addressed itself in particular to the so-called social gatekeepers, i.e. personnel officers, job supervisors and career officers. Several of its claims reflect the pervasive influence of the ethnography of speaking. Firstly, despite the similarities between *Indian English* and *Western English* on the formal-structural levels, Indian English must be considered a distinct *ethnic* variety in Great Britain which differs from Western English mainly with regard to pragmatic, text-building and discourse features. The second is that interethnic miscommunication is often the result of non-shared social knowledge about the norms of interaction. Thirdly, these differences lead to miscommunication and an increase in prejudice in longer-term, goal-oriented encounters, e.g. negotiations or job interviews. From a methodological point of view *Crosstalk* is similar to most of Gumperz' studies in that it combines micro-analysis with participant questioning.

The intended wide public impact of this programme and the fact that it is typical of the British functional approach to cross-cultural communication problems call for a critical re-assessment. I will argue that the application of the ethnography of speaking to socio-economic problems is in fact biased against minority group members and may fail to diminish prejudice. I will also argue that there is an over-emphasis on the impact of concrete communicative encounters, as against non-communicative factors such as the political and economic climate for the creation and maintenance of ethnic prejudice. For brevity I will concentrate on one section of *Crosstalk*.

1. In their micro-analysis of a job interview between an Indian immigrant and a white selection committee at a college, where the Indian had applied for a post as librarian, the authors distinguish various layers of communication breakdowns which result in a complete lack of mutual understanding and conversational friction:

(1) A knowledge of the purpose, the overall structure and the implied role relationship between the interactants in a job interview is taken for granted by the committee but is in fact not shared by the applicant. This expectation led the interviewers to the use of indirect (key) questions and they expected the candidate's answers to be appropriately structured, i.e. to have immediate relevance to the position he has applied for.

(2) Both parties draw wrong conclusions from each others' verbal behaviour. Because the applicant had not understood the indirect questions, the interviewers did not see the relevance of his replies. Furthermore, they were constructed in a typical Indian English manner in that they were "not synchronized to the questions in terms of rhythm, key words and other features" and begin with the least relevant information.

(3) The interviewers changed their questioning strategy from indirect to more direct questions when the candidate seemingly failed to show any interest in the job. They did so, however, by means of prosodic features which are used quite differently in Indian English.

A closer examination of this encounter shows that the interviewers started out with a well-defined, conventional interviewing strategy but departed from it later on. The whole conversation then rotated around the sincerity of the Indian's application and his personality as such was cast into doubt.

2. In my re-analysis I followed closely the methods of the original one but discovered various deficiencies related to methodological and terminological unclarities, e.g. the use of terms like information structure, synchronization, and an unjustified restriction to those aspects of pragmatic behaviour that allegedly distinguish Indian from Western English. I will limit the discussion to those, see also Leitner (forthc.).

Due to this restriction the authors overlook the consequential, parallel development in the speech of both parties which clearly indicates the conversational strain on the participants, the deteriorating *role relationship*, but also the very explicit, negative evaluation of the applicant by the selection team. This linguistic process, however, affects different levels of speech behaviour in the two sides. The Indian's command of English is, in general, rather good. There are rather few typical Indian English features, but as the interview progresses the number of formal-structural errors increases. To give just a few examples: 'appli,cations, 'int7r'view, 'aca,demic, 'appreciate, ex'amination; if teacher want..., this job at present I am doing was the only into I got, I already sent 50 applications but this is my second interview.

The interviews, on the other hand, went from more indirect to more direct questions, cp a. with b. and c. below:

- a. HoD3: ...Erm I *just wondered*, you know, you've really changed jobs a lot of times, and er, *if you don't mind my saying*, I mean, I *just wondered* whether this was *sometimes*...
- b. VP3: You say that er you've put in for about 50 jobs recently and that you have no particular idea why want to come to M. Do you really expect us to consider you...
- c. CAO8: Why not, Mr. S?

The pragmatic function of this switch is ill-explained in *Crosstalk* which, in line with speakers' self-reports, assumes that it was to facilitate comprehension. A more satisfactory interpretation must refer to Goffman's work on social face

and personal preserves and to Brown/Levinson's study on politeness and face-threatening acts. It can easily be seen then that the direct questions violated the applicant's face needs entirely, in particular in such a crucial situation.

A second point concerns the claim that the applicant was unable to interpret indirect "key" questions. It is said, for instance, that he failed to recognize the intention underlying a question that was to elicit his social skills in job-related situations and that, by talking about past university activities in India, he gave an irrelevant reply. A closer look at the subsequent exchange, however, reveals that the interviewer operated on two different levels of communication, probing the applicant's social skills in job situations and his personality as such. Following Argyle (1972) one may safely assume that it was the latter level that really interested the team and that his job-related social skills were of secondary importance. If this is so, the candidate's answer may be seen to relate to the former level and need not demonstrate non-comprehension at all, cf also Leitner (forthc.).

In other words, the role of ethnicity has not been demonstrated convincingly in *Crosstalk* and the role relationship as it is established by the interviewers is a more decisive factor. It emphasizes social asymmetry and distance, and these factors are aggravated in the course of the encounter. While they are unavoidable in job selection settings, they need to be deemphasized so as to allow the interviewers to get as fair a picture as possible of *any* candidate. Additional skills are required to the team, cf Argyle (1972:201) since a candidate's performance in such an encounter cannot be taken as reflecting his normal behaviour. In the interview under consideration the selection committee fails in serious ways.

3. Some more general conclusions must now be drawn.

Firstly, while Gumperz has convincingly demonstrated the role of the dynamics of conversational encounters and of participants' communicative backgrounds as against macro-factors such as the ones studied by Labov for an understanding of language variation and communicative success, the assumption that the "cumulative effect" of miscommunication in *any one* encounter explains negative outcomes and may lead to prejudice is questionable. The ethnography of speaking assumes, for one, that on-the-spot interpretations in concrete encounters directly reflect one's communicative background and experience and, secondly, that they are guided by a fairly uniform cognitive framework in interethnic communication, i.e. an unreflected notion of ethnic difference. *Crosstalk* suggests that an awareness of this fact and of the limitations of this framework may lead to improved conversational practice. While this seems acceptable as a neo-Platonic version of learning through insight into one's actions, one must point out that the relationships are more complex and that some people perform better than others. In particular racial prejudice is not re-created in any one encounter but is a more general background factor that pre-determines to varying extents the outcome of conversational encounters.

Evaluative judgements in applied spheres are thus unavoidable.

The second conclusion concerns the conceptual framework itself, i.e. the postulation of Indian English as an ethnic variety and the role of ethnicity as such. Conceptualizing interethnic communication in institutional settings solely in terms of ethnicity can lead to serious mis-judgements of conversational encounters as I have shown and to a re-inforcement of prejudice instead of its diminishment.

For instance, Loveday (1982) argues that Japanese speakers of English must unlearn their own culturally-determined indirectness. They must learn to be more direct, explicit and able to (constantly) pass evaluative judgements, features that are considered as typical of English speakers. House's comparative study of German and English speakers (1979) found that Germans tended to use much stronger and explicit speech acts in criticizing others than the English. They would have to unlearn directness. *Crosstalk* suggests to English speakers that a greater explicitness and directness would make inter-ethnic communication more effective. In view of these conflictory findings and the present lack of accepted parameters along which ethnic differences can be measured it seems safest to play down the role of ethnicity in explaining communication breakdowns in industrial areas and to look for an explanation in the role relationships in hierarchically organized institutions and large-scale factors such as personnel policies, personnel training and the wider socio-economic climate.

This leads to the last point. Given critical statements such as "When expectations created by dialect stereotypes are further reinforced by misapplied social science findings, education suffers" (Gumperz), one is surprised that the poor performance of the interviewers is only alluded to in the conclusion. Could it have been that the prevalent understanding of media balance and neutrality led to an avoidance of judgements on present industrial practices and reinforced the conceptualization in terms of ethnicity?

To sum up. The ethnography of speaking takes too broad and imprecise a notion of communication. By limiting itself to micro-analyses it overlooks the relevance of large-scale socio-political and of macro-sociolinguistic factors and is led to rather naive suggestions on the possibility of overcoming prejudice.

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Linguistic Variations of Different Age Groups in the Atayalic Dialects

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1. Age plays an important role in language structure and change. Different age groups may have different sound systems in the same language. For instance, many interesting examples of sound change in progress can be observed in the variations of speech forms exhibited in the different age groups in many different dialects of Atayal, an Austronesian language in Taiwan. These include the variants (1) -p~-k and -m~-ŋ, (2) -g~-w and -g~-y, (3) -l~-n, (4) t~-c and d~-j, (5) ʔ~-x, (6) -b~-p, (7) c~-s, and (8) aw~-ɔ and ay~-ɛ. 1101

2. Implications of Linguistic Variations for Historical Linguistics

2.1 Linguistic Variations Indicate State of Change

In general, older speakers retain archaic forms, whereas younger speakers possess innovative forms. A linguistic change usually goes through these three stages: Stage 1, before a change takes place, as exhibited in the speech of the oldest speakers; Stage 2, the intermediate stage of variations, including free variants; and Stage 3, completion of the change, as manifested in the speech of the youngest speakers. The intermediate stage best shows the state of change. It may take up to 30 or 40 years to complete a change, as in the cases of Skikun and Inago. We have not found any case in which a change is completed in less than 20 or 30 years in precisely the same dialect or speech community.

2.2 The Atayalic Group Has the Same Direction of Change

The Atayalic group has the same tendency and direction of change. Each change in each dialect may be independent, but parallel.

That genetically-related languages or dialects have the same direction of change was best explained as "drift" by Edward Sapir in 1921:

"Language moves down time in a current of its own making. It has a drift." (p. 150)

"The momentum of the more fundamental, the pre-dialectic, drift is often such that languages long disconnected will pass through the same or strikingly similar phases. In many such cases it is perfectly clear that there could have been no dialectic inter-influencing." (p. 172)

Thus it is wrong to assume that if the same change has occurred in all dialects in the group, then the change must have taken place in the proto-language, i.e. prior to when dialects split apart from the proto-language. Dialects may have parallel changes long after splitting apart. We have ample evidence

for various sound changes occurring long after the split of the Atayalic group.

Judging from the same general tendency and direction of change in the Atayalic group, it is predictable that eventually /p, b, m, t, g, l/ will not occur in word-final position, there will be no more /c/'s in initial and medial position, and there will only be weakened or reduced vowels in unstressed syllables throughout the entire group.

2.3 The Overall Direction of Sound Change Is towards Simplification

The overall direction of sound change in the Atayalic group is towards simplification. All changes are from "marked" to "unmarked," i.e. from sounds with more complex features to sounds with less complex features, including (1) devoicing of the final voiced stops: -b > -p, -d > -t, and devoicing of the voiced fricative: γ > x (2) shift from final labials to velars: -p > -k, -m > -ŋ, (3) vocalization of the final voiced velar stop: -g > -w or -y, (4) palatalization of the dental stops: t > c, d > j, (5) nasalization of the final lateral: -l > -n, (6) shift from dental affricate to sibilant: c > s, (7) labio-dentalization of the bilabial fricative: β > v, and (8) monophthongization of the diphthongs: aw > ɔ, ay > ɛ.

2.4 Sound Change is Phonetically Abrupt, but Lexically Gradual

As first suggested by William Wang in 1969, sound change is phonetically abrupt, but lexically gradual. As in Skikun, a speaker produces either a labial consonant or a velar, but never a speech sound somewhere between the two points of articulation. Similarly, a speaker may pronounce either /-g/ or /-w/, or sometimes /-g/ and sometimes /-w/ (i.e. free variants), although there seems to be a transitional sound [w] between the two. Even in vowels that are inherently susceptible to gradual change, the changes also appear to be abrupt, i.e. from /aw/ to /ɔ/ and from /ay/ to /ɛ/ in two different age groups, as in Talawan.

That lexical application of a sound change is gradual is also supported by the evidence of variations among different age groups in the Atayalic dialects, particularly in the cases of Skikun and Inago. The Skikun speakers between ages 80 and 32 apply the rule -p > -k and -m > -ŋ to different lexical items and they differ in the number of items to which the rule applies, largely depending on the age and sex of each individual speaker. Except for a few free variant forms, each speaker is fairly consistent in producing a certain ending for a certain lexical item. I checked with four Skikun speakers in both December 1979 and August 1981, and three of them showed slight changes in the endings they used. Consequently, lexical diffusion is to be understood such that it is gradual as manifested in the speech of different age groups (i.e. "successive generations"), rather than in the speech of the same adult speaker as he gets older although it also occurs within the speech of individuals to a much smaller degree.

In short, the evidence of the linguistic variations between different age groups as reported in this paper seems to support Wang's theory rather than structuralist or neo-grammarians theories.

There is substantial evidence from the Atayalic group of dialects that

language changes much faster and is more readily observable than what the structuralists, such as Bloomfield and Hockett, have assumed. All the changes in progress can be observed through the variations of different age groups within the same dialect.

2.5 Language Changes Due to "Discontinuities in the Grammars of Successive Generations"

In contemporary linguistics, language change is explained as a process of language acquisition. In Noam Chomsky's view, child language acquisition is a process of constructing the "optimal" (i.e. simplest) grammar to account for the "primary linguistic data" presented to him. Morris Halle accepted this view and attributed most linguistic changes to language acquisition by a child.

Speakers of different age groups from the same speech community do show that they have somewhat different grammars. The younger generation, on the one hand, may simplify the grammatical structures. On the other hand, they may also introduce some new rules to the grammar of the adult. For instance, younger speakers of Skikun and Mnawyan dialects of Atayal add a new phonological rule by changing all labials to velars word-finally, and younger speakers of Matabalay, Mayrinax and Inago dialects of the Atayalic group add a new phonological rule by changing all or some word-final /-g/ to /-w/ or /-y/, depending on the preceding vowel. Such evidence indicates that children are capable of adding rules to the grammar and thus making some aspect of the grammar more complex than the one of the adults.

In short, the evidence we have obtained from the Atayalic dialects confirms Halle's theory that language changes due to "discontinuities in the grammars of successive generations" and, at the same time, it suggests a partial revision of some of his (Halle 1964:344) statements:

"The language of the adult—and hence also the grammar that he has internalized—need not, however, remain static: it can and does, in fact, change. I conjecture that changes in later life are restricted to the addition of a few rules in the grammar and that the elimination of rules and hence a wholesale restructuring of his grammar is beyond the capabilities of the average adult."

It is evident that addition of a few rules in the grammar is not restricted to the adult, but also actualized in the speech of the child.

It is not easy to tell what constitutes a new generation and the demarcation between generations is by no means clear. Thus the term "discontinuity" as used by Halle may be somewhat misleading. To further complicate the picture, in the whole process of a sound change, language goes through stages of free variants, and both age and sex factors come into play, as exemplified in Skikun and Inago.

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Personal Names in Hindi

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The purpose of this paper is to examine the personal names in Hindi in relation to the socio-cultural factors and thereby refute Lenneberg's assertion, "Only proper names are relatively immune from these extra-linguistic factors" (Lenneberg 1967:344).

Religion is the main stay of Hindu society. The vast majority of Hindi names are derived from Hindu gods and goddesses, saints and philosophers, sacred rituals and centres of pilgrimage. There is hardly a deity, male or female, of the Hindu pantheon that has not figured in personal names. According to a survey made by Vibhu the Hindu deities adopted in personal names have been placed in the following order of popularity—Shiva (10.5% of total names), Krishna (10.1%), Ram (6.4%), Vishnu (5.3%), Parvati (3.2%), Ishwar (2.63%), Surya (1.2%), Ganesh (4%) and Bramha (.62%) (Vibhu 1958:68).

This practice of naming one's children after deities implies an attitude towards the divine which is in no sense universal. In some areas of the world it would be unthinkable to name a child after a god. Christians, for instance, do not name their children after Jesus or God, although among the Muslims there goes a saying "If you have a hundred sons, call them all Mohammad." There is a strong belief among the pious Hindus that the mere utterance of the name of a god or goddess works as a spell upon the utterer which not only protects him from the evil spirit but also brings good luck, happiness and prosperity. It has been said that "Rama's name has more power than Rama himself" (Masani: 51). Thus whenever the elders would be calling or referring to their children, they would be uttering the names of gods/goddesses and thereby making them entitled to the *punya* (the divine reward of a righteous deed) that accompanies the mere utterance of a divine name.

Caste has also been a strong determining factor in the choice of a name. Certain personal names are inseparably linked with certain specific castes. *Vidya Nivas* (the abode of learning) and *Buddhi Sagar* (the ocean of wisdom) can only be the names of Brahmins; *Samar Bahadur* (one who is brave in warfare) and *Jalim Singh* (the tyrant) belong to Kshatriyas; *Karori Mal* (the multi-millioner) and *Daulat Ram* ('Daulat'-Wealth) can only be Vaishyas; *Chamaru* (sweeper) and *Panaru* (Gutter) are the names reserved, as it were, for Harijans and other low castes. Sometimes specific linguistic features characterize names of the people belonging to a particular caste and thereby mark them out as distinct from members of other castes. We note for instance that the name of a deity *Ram* occurs both at the beginning and end of the name of a Harijan.

a pattern which will not normally be discernible in the non-Harijan names. Some examples from a University record of Harijans are: Ram Jit Ram, Ram Bali Ram, Ram Bachan Ram and so on. This is a new trend in adopting names, particularly among the educated Harijans born in the post-independence period. "On the one hand these names are deliberately given a look different from the traditional names adopted by the erstwhile Harijans and suggestive of a backward and inferior status; on the other they are motivated by a desire to retain their separate identity to highlight their minority character and thereby apply pressure tactics when necessary" (Mehrotra 1979:208).

Family ties in India are closer and stronger than in most places in the West. The fact of belonging to the same parents is suggested by names that appear to be patterned on a kind of analogy and have a striking phonological resemblance. The following groupings of names of real brothers illustrate this point: *Jay-Ajay-Vijay*; *Sanjay-Sanjiv*; *Mahesh-Ramesh-Dinesh*; *Trijugi Narain-Satjugi Narain*. All the ten sons of *Satya-Bhama* in Indian mythology had their names formed with "Bhanu": *Bhanu*, *Subhanu*, *Swarbhanu*, *Sribhanu*, *Prabhanu*, *Chandrabhanu*, *Vrihatbhanu*, *Atibhanu*, *Pratibhanu* and *Bhanuman*. This similarity of designation clearly and adequately symbolizes relationship among children of the same parents.

Female names in Hindi provide yet another instance in which linguistic markers single out the names of a particular class. It is customary for Hindi female names to end either with *ā* or *ī* sound as has also been prescribed by the ancient law givers. *Rita*, *Vimala*, *Laxmi* and *Savitri* are illustrative examples of the common feminine names in Hindi. It is interesting to note that there are many masculine names in Hindi which are derived from some common feminine names. The examples are *Rama Kant*, *Durga Das*, *Bhagwati Prasad*, *Munni Lal* where *Kant*, *Das*, *Prasad* and *Lal* are some of the common masculine markers which distinguish between masculine and feminine names in Hindi. There are however some names of unisex type used for both males and females. It is not possible to identify the sex of the persons bearing such names unless otherwise indicated by suffixes or gender markers. Some of the names common in both males and females are *Santosh*, *Gopi*, *Madhu*, *Arun* and *Prem*. Such ambivalent names which are reminiscent of the naming practice of several primitive tribes are common today in other cultures as well.

During the several hundred years of foreign domination India has been particularly influenced by two foreign cultures—Muslim and British. This influence is noticeable not only in our life style, food habits and dress but also in the sphere of personal names. Some of the Hindu names revealing unmistakably the impact of the Muslim culture are: *Ekram Singh*, *Iqbal Narain*, *Khursheed Bahadur* and *Suleman Singh*. But for the surnames i.e. *Singh*, *Narain* and *Bahadur* any of these names may pass as belonging to the members of the Muslim community. Similarly, the influence of English language and culture is noticeable in Hindi names/nicknames such as *Inspector Singh*, *Judge Singh*, *Dipty* (deputy) *Singh* and *Jolly*, *Pinky*, *Bobby*, *Ruby* etc.

Names very often serve to distinguish people with a rural background from those who come from an urban setting. Names such as *Khet Singh*, *Kharpattu* and *Dal Singar* are normally given to children born and brought up in a village and cannot be given to the children born and brought up in educated urban families. Similarly, names such as *Parimal*, *Amitabh*, *Anuradha* and *Bandana* belong exclusively to the families in educated urban setting. The names of the two categories described above are not interchangeable.

The Hindu way of life is influenced considerably by superstitions relating to child birth, longevity and good luck. Many of the common names of the Hindus owe their origin to them. When a person finds his children dying in their infancy one after the other, the child born to him next is presumably sold out to someone. It is believed that by transferring the ownership of the child, it would be saved from death. Hence the children are given names such as *Bechu*, *Bikanu* which mean "sold out" or else *Mulai*, *Bisai* which mean "bought." In most Hindu families the birth of a female child is greeted with gloom and anxiety mainly because of poverty and dowery system. When after many female children another child born also happens to be female, she is given the name *Kshama* meaning 'pardon' or *Tripti* meaning contentment implying a plea to a god or goddess to excuse them and not to bless them with any more children.

Thus personal names in Hindi are more than mere labeling devices. They are the indispensable repository of our customs and traditions, our outlook and thoughtways, our sociocultural identity. The expression 'What's in a name?' has to be dismissed and replaced by "What's not in a name?"

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Componential Analyses of the Lexical Structure of Pidgin Languages and Socio-Linguistic Factors Affecting Their Acquisition

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Recently, more attention has been focused upon the studies of pidgin and creole languages since their importance as the means to delve into the mechanisms and principles governing the second-language acquisition is becoming more and more succinctly recognized. Promotion of these studies has also been enhanced by the recent development in syntactic, semantic, phonological, and socio-linguistic theories and practices. However, the lexicons of these so-called "make-shift" languages still basically consists of mere listing of vocabulary items with the designations of their parts-of-speech, the linguistic origins showing which languages they are derived from, and their meanings. The following study is an attempt to demonstrate that mere listing of the syntactic functions, the etymological origin, and the meaning of each vocabulary item is not sufficient to show the relative importance of the lexical items in the pidgin communication. (In this study, the term "pidgin" is to be defined as being; (1) the native language to none of its users, (2) the product of a language contact situation, and (3) extremely simplified in its syntactic and lexical structures.)

For this purpose, two different analyses of the lexicon of the pidgin English used by the Japanese immigrants in communicating with the speakers of the other languages on the sugar plantations of Hawaii will be attempted. (This type of language is designated as PEJH in this study.) The corpus for this study is the same as for my previous publication.¹⁾ It was recorded during the period of 1961-62. All the informants were born in Japan and have worked as plantation laborers after they migrated to Hawaii. They did not receive any formal instruction in English and were forced to rely on the pidgin English as the means of communication with the members of the other ethnic groups. They were interviewed by either English or Illocano speakers for the recording. The interviewers were instructed to restrict the topics to the informants' earlier experience on the Hawaiian plantations. Due to the long residence in Hawaii, their Japanese speech was considerably influenced by the features of English, mainly by those of their pidgin English speech. Therefore, it became necessary to establish a criterion for distinguishing the Japaneseness and the PEJH-ness of their utterances. It was done according to the principle that all utterances

1) Susumu Nagara, *Japanese Pidgin English in Hawaii, A Bilingual Description*, (Honolulu, the University Press of Hawaii, 1972).

whose predicate words are derived from Japanese are classified as the Japanese utterances. Otherwise, the utterances are classified as those of PEJH irrespective of borrowings from the other languages. This principle was adopted because, when the predicate words are derived from Japanese, the morphophonemic rules of Japanese were applied. Also, when a Japanese predicate word was used, the terminal juncture conformed to the norm of the Japanese language. From the recordings of 10 male and 4 female informants, a corpus of 5 hours and 45 minutes was selected for analysis.

A total of 1,203 vocabulary items were found out of this corpus of a little less than six hours. For a pidgin language which is ordinarily characterized for the paucity of the lexical items, this size of 1,203 lexical items is considerably numerous. According to a survey by the National Language Research Institute of Japan, a house-wife in Tokyo was found to have used 1,123 words between 6:00 a.m. and 11:00 p.m. of the same day.²⁾ Even considering the fact that my recordings are speech activities of several speakers while the National Language Research Institute data is based upon those of a single individual, the size of 1,203 words is not small. These 1,203 items are divided into the following nine groups in terms of their linguistic characteristics:

1. Forms derived from English	879 (73%)
2. Forms derived from Japanese	156 (13%)
3. Forms derived from Hawaiian	73 (6.0%)
4. Interjectional expressions	38 (3.2%)
5. Proper names (personal names, place names)	29 (2.4%)
6. Forms probably derived from the Filipino languages	12 (1.0%)
7. Loanblends	8 (0.6%)
8. Form derived from Portuguese	1 (0.08%)
9. Forms whose origins are unknown (limited to one interviewer)	8 (0.6%)

At the first glance, the above table seems to show that the forms derived from Japanese play much more important roles than those derived from Hawaiian. However, the comparison of these forms with the 200 basic word list of Lexicostatistics and the componential analyses of the first three groups in the above list reveal that those derived from English and Hawaiian play much more important roles than those derived from Japanese.

For example, 120 out of the 200 basic concepts of the Swadesh's word list are found among the 1,203 words. These 120 concepts can be subdivided into the following groups in terms of their linguistic origins:

1. Concepts expressed exclusively by the English words	89
2. Concepts expressed exclusively by the Hawaiian words	4
3. Concepts expressed by both the Portuguese and English words	1
4. Concepts expressed by both the Hawaiian and English words	22

2) Taro Takahashi, ed. *Nihonjin no Gengo Seikatsu, Shin Nihongo Kooza*, 5, (Tokyo, Choonbunsha, 1975) p. 2.

5. Concepts expressed by both the Japanese and English words 4

A total of 116 English words, 26 Hawaiian words, 4 Japanese words, and a Portuguese word are used to express these 120 concepts. This result shows that the Hawaiian lexical items perform much more significant function in expressing basic concepts. The four concepts which are exclusively expressed by the Hawaiian words are *kolohee* (bad), *makau* (fearful), *pui* (fat), and *lepo* (earth).

Componential analyses of the lexical structure of this type of pidgin English also reveals that even though many more English-derived lexical items are used than the Japanese-derived ones, the former can be more systematically analyzed in terms of fewer categorical and componential features than the latter. The Hawaiian-derived lexical items can also be more systematically analyzed in terms of fewer features than the Japanese-derived ones. For example, all 73 items from Hawaiian can be analyzed in terms of five features: (1) cultural borrowings, (2) human relationships, (3) topographic and geological features, (4) terms related to the plantation work, and (5) terms expressing conditions or qualities. Componential Analysis of the 77 Japanese nouns found in the corpus reveals that 58 of them can be classified in terms of six categories; (1) cultural borrowings, (2) terms expressing human qualifications, (3) human body parts, (4) abstract concepts, (5) institutional names and their activities, and (6) food-stuff and garden produce. However, this analysis leaves 19 Japanese nouns for which common denominators couldn't be found.

Superficial observation of the componential analysis of more than 550 English nouns seems to indicate that it requires much more categories and denominators, namely eight major categories and thirty-five subcategories. The eight major categories are: (1) quantitative expressions, (2) temporal expressions, (3) work-related terms, (4) terms with the human feature, (5) housing and institutions, (6) features of the new land (Hawaii), (7) terms used to describe the new life-style, and (8) abstract concepts. However, closer observation of the componential analysis reveals that these eight classifications account for much more than 354 English nouns listed in the handouts (Nagara's handouts pp. 8-11). In addition, the analysis of the English nouns leaves only 24 lexical items for which common denominators couldn't be found. This results in the unaccountable ratio of less than 4% (24 out of more than 550) while the unaccountable ratio for the Japanese nouns is 24.6% (19 out of 77). Therefore, we can conclude that the lexical structures of the Hawaiian forms and the English nouns are much more systematic than that of the Japanese nouns in this type of pidgin speech. This is probably because in Hawaii both English and Hawaiian performed more important roles as the means of communication among various ethnic groups. Consequently, the acquisition of the English and Hawaiian forms was governed more strictly by the situation which produced the plantation pidgin English. The Japanese items are more diversified because they were mainly used as the stop-gap measure.

The outcome of this study shows that the mere listing of lexical items cannot

show the importance of the roles performed by the various forms. Considerations of the socio-linguistic factors affecting their use provide the clues for the systematic analysis of the lexical structure of pidgin languages.

Three Issues on Languages in Contact

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I. Introduction

In the past five years a marked change of emphasis has been noticeable in linguistics. The illusion of a completely homogeneous, Chomskyan language community has given way to one which takes into account social, psychological and individual components. Purely formalistic, descriptive features were forced temporarily into the background in favour of a diachronic, socio-cultural, political science of language; in short, one which includes non-linguistic factors as well. Consequently, multi-dimensional strategies were to replace the (frequent) uni-dimensional systemic linguistics. Instead of the technical difficulties in describing the field of semantics, for example, new problems arose: namely those of variation and model-diversification. The inclusion of numerous related disciplines such as sociology and psychology, as well as the discussion of speech act theories, areal linguistics and the problems of language barriers pertaining to social-political issues, all led to a spectrum of methods whose variational possibilities were both their strengths and weaknesses.

Soon one of the central themes of variational linguistics included that of contact research or contact-linguistics whose historical tradition goes back to the fifties. This area depending on one's point of view, uses methods of sociolinguistics or the sociology of language. It originated in the U.S.A., just as did the beginnings of systemic linguistics, where Weinreich's, Fishman's and later Labov's work revived what had been frowned upon for a long time: field research of an empirical nature which, in particular, was to be found amongst the areal linguists of Europe.

At the same time, language-contact research is indebted to the socio-politically oriented language-barrier research in the sense of Bernstein's and Oevermann's numerous endeavours. Completely new areas of socially-dependent, semi-, bi-, and multilingualism were discovered in addition to previous analyses of bilingual contacts in conflict zones (for example, French and English in Quebec).

With the aid of the diglossia concept, developed and expanded by Fishman which replaced to a certain extent the idea of bilingualism as having paedagogical-historical significance, attention was now paid to the socio-politically motivated difficulties of dialect speakers, socially under-privileged city-dwellers and mono- or multi-linguals in language-conflict zones who were handicapped in their chances of professional advancement.

In this way, creole and pidgin languages in the Third World became the

centre of interest among linguists and full-fledged means of communication. In the United States, sub-stratum- and semi-languages of members of different classes were sociolinguistically described, (usually as a Black/White contrast), and numerous forms of double diglossia were discovered in Europe: thus, not only are all European countries with the exception of Iceland multilingual, but also predominantly dialectal.

2. Three Issues in Language-Contact Research

Completely different values, on the national as well as regional level, are attributed to each individual language—which makes any uni-dimensional, linear investigation more difficult—due to cultural, historical, social and politico-economic developments. In this way, the communicative variants in use comprise native and foreign monolingualism, diglossia and multilingualism, along with additional linguistic transitional zones and mixed forms with which contact linguists are trying to come to terms by mono- or inter-disciplinary means.

2.1 Problems of Language Census in Multilingual Areas

It is astonishing how the results of state-run censuses are taken for granted, even in the literature of meritorious language-contact researchers, and are used as a decisive argument for language-planning strategies. In language-contact zones which have no sense of conflict, the quotation of official results may appear meaningful—in those areas in which political or socio-economic factors are clearly to the disadvantage of the minority, such counts can only serve to show a tendency and not the exact affiliation of the linguistic group. On the one hand, a type of bi- and multilingualism, which has recently become (situationally and contextually), relevant for European minority areas, cannot be associated with any specific mother-tongue, since the diglossic linguistic behaviour usually shows, a *functional distribution*; this means that particular, every-day speech situations and conditions constantly require the same linguistic variants, so that, frequently, the use of foreign and native languages already appears institutionalised. Due to economic reasons, only a few linguistic areas permit the constant, free exchange within different variants. The results of a poll and the difference between foreign and native speakers lose their relevance for this reason.

On the other hand, any answer to a question concerning the every-day language use is subject, today more than ever, to a sociological framework of requirements which, above all in conflict zones, appears so complicated that even an inquiry by trained interviewers can lead to distorted results. In his reply, the informant will by no means be considering the problems of linguistic variety within his language use in the same way as the interviewer, rather, he will—consciously or unconsciously—identify himself with a group and give more importance to the group loyalty which he aspires to.

Neither linguistics nor sociology have models and methods at their disposal which come to terms with these extra-linguistic features. Such results concerning individual linguistic behaviour therefore reveal more information about social

consciousness than the real language use of the informant. Thus, the social pressure, which brands a particular linguistic variant as a prestige variant, must constantly be taken into consideration in conflict zones. A broad area of work in this direction is opened up to researchers of prejudices and stereotypes.

2.2 Polarisation

More than enough has been written for the individual and social groups in the relevant literature about the drastic consequences of political and social upheavals—in Europe: since the Industrial Revolution, in Africa: since the end of the colonial rule. Naturally, language contacts in multilingual areas cannot be considered separately from social change.

Multilingual communities which had grown up more or less harmoniously together for centuries, or had come up in the course of hostile disputes, had found ways and means of multilingual communication whereby clear parameters of valuation arose in the form of privileged languages or prestige variants.

Example I: Speakers of a village community in India, where twelve languages are spoken, communicate by switching from their mother-tongue into at least one, sometimes into two or three prestige variants of the dominant village languages in a state of constant yet, nevertheless, institutionalised change.

Example II: In one of the smallest but now, however, most violent areas of language conflict in Europe, the Voer (Fouron) area (lying between Belgium and the Netherlands close to the German border), a *stable diglossia*, Dutch dialect and French standard, had developed in most of the language domains in spite of foreign infiltration, so that, among those immanent domains such as school, the city council and in public, language use, with an extraordinarily high degree of consistency, accordingly followed an almost ritualistic pattern.

The trend of the modern industrial society towards a unity of language use, together with a dominance of standard variants, created unwelcome problems for those minority areas with a marked use of dialects. Territorial monolingualism replaced the individualised diglossic situation and forced the speaker to decide for or against a particular standard language.

This *polarisation* therefore had, to a certain extent, completely unexpected results at least in the eyes of the "logically" operating language planners: the *diglossic* or dialect-speaking population group rejected this pressure of polarisation by abandoning the respective standard variant and turning to a foreign "high" variety, (in India: English, in the Belgian Voer in Europe: French). As such a shift in domains cannot always be successful, it leads to *schizoglossic* conditions (Haugen) or to language shift.

2.3 Language Shift

Sociology has already developed applicable models which allow a sociological description. Amongst these are, for example, descriptions with the aid of

groups of reference and identity which, furnished with the same norms and role requirements, represent a guide-line and an instrument of control which respect the observance of these norms and sanctions of behaviour.

The current, world-wide minority problems usually arose from three factors: migration, socialisation and assimilation, whereby the latter is particularly important for the analysis of language shift and stabilisation in a multilingual area: under what kind of conditions and how quickly is the immigrant assimilated, with which group does he identify himself and in which cultural community does he prefer to live? However, sociology has not succeeded until now in designing a valid, theoretical picture of multilingual countries and their respective group-behaviour. The reason for this obviously lies in the variational range of language contact.

It appears to us to be important that language shift results not collectively, but individually, whereby these situations are to be categorised sociolinguistically.

Where does the extraordinary willingness come from, in times of world-wide recognition of the principle of existential equality of languages and cultures, to still accept (in many cases) another foreign language and culture? Together with the alienation mechanisms arising from the infrastructural description of the most important sectors in the labour market, another factor plays a part which makes *language shift* automatic. Without strong cultural connections, migrants from the countryside come not as a group, but as individuals with a strong desire to integrate as quickly as possible in order to be able to advance socially; after all, the gross national product in the city often lies a third or more above the average in the country. This over-riding willingness to assimilate leads to rapid integration. Since the urban linguistic group appears so much stronger, both professional and private contact is sought with this status group. The effort to integrate results in social advancement.

Mixed marriages are disproportionately high amongst this group. The marriage partner who speaks a foreign tongue will like-wise adapt from both a linguistic and cultural point of view for status reasons, regardless of sex which, in turn, will strongly influence the future language in the family so that a snow-ball effect arises in the second generation. For this reason, the language used in marriages requires just as much attention in any sociolinguistic analysis as the mother-tongue which, until now, was always the main factor.

The inquiry confirmed this observation. 74% of all children from mixed marriages are sent to French-speaking schools and thereby undergo a language shift at the beginning which will have been completed in the following generation. This is supported by the fact, since women, as earlier inquiries have shown, tend to change language and culture for reasons of prestige quicker than men do, the answer lies perhaps in the various fields of work: women are generally employed in the private sector which uses, to a large extent, the prestige variant.

3. Results of Contact-Linguistic Investigations

What consequences arise from such an investigation of language contacts and conflicts which exist in an urban population?

A few points, although differing in importance, may be summarised here:

- 1 The desire to progress to a status group which is regarded as being both financially and socially superior, and which therefore demands a condition of exclusiveness, must lead to language and culture shift or to language and culture conflict.
- 2 The linguistic and cultural alienation of the lower and middle classes, particularly of the worker, seems to be inevitable as long as a linguistic group of equal status is missing.
- 3 Sociolinguistic research into language and cultural conflicts should not confine itself to the individual, but rather should include groups, (families), and their linguistic behaviour, since only in this way can the trigger-effects of language shift processes be clarified.
- 4 Scientific research into the conditions concerning language and cultural shift can only be undertaken in an interdisciplinary manner.

On Intrasentential Code-Switching in Japanese/English

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There has been a growing interest in syntactic aspects of intrasentential code-switching, the use of two or more languages within a sentence, a phenomenon often documented in bilingual or multilingual communities (Timm 1975; Wentz 1977; Pfaff 1979; Poplack 1979; Sankoff and Poplack 1980). These works have shown that intrasentential code-switching is not a random phenomenon but is subject to syntactic constraints. However, Sankoff and Poplack attempted to formalize the constraints on switching for the first time. Based on the Spanish/English data, they posited two general constraints which they claimed to be universal or near-universal. One is the 'free morpheme constraint': 'a switch may not occur between a bound morpheme and a lexical form unless the latter has been phonologically integrated into the language of the bound morpheme'. The other is the 'equivalence constraint': a switch occurs only between two sentence elements or constituents whose relative order is shared by the two languages involved. This paper examines whether these two constraints hold in the Japanese/English code-switching. Since the concept of word order is the main factor in formulating the equivalence constraint, an investigation of switching involving two languages with radically different word order seems very promising. Spanish and English share basically the same word order; SVO, while Japanese is an SOV language.

Data

Sentences used for analysis were taken from sociolinguistic interviews and natural speech tape-recorded in a Japanese community in Toronto, Canada, a total of 3.5 hours of speech. The speakers are all Niseis (second generation) over 55 years of age who are competent in both languages. They learned Japanese from their parents who had come to Canada early in the century and received education in English except for one who spent several years in Japan in her teens. From my observation, they code-switch in interacting with other bilinguals.

Analysis

Sentences containing both English and Japanese sentence elements were extracted from the transcribed data and syntactic categories of these elements and switch sites were identified. Personal names, place names, interjections and fillers were excluded from this analysis. Everything else which is phonologically not integrated into the other language was considered to be code-

switching. No violation of the free morpheme constraint was found. For the purpose of this paper, those types of switching where the equivalence constraint does not hold will be presented.

A. Japanese has a great many postpositional particles. Topicalization and grammatical cases are all marked by these particles. Our informants use these postpositional particles with single English nouns as in 1) and with English noun phrases as in 2).

- 1) vocation GA NAKATTARA, ... (GN1A330)
 particle adjective
 subject marker conditional

'Unless (they) have a vocation, ...'

- 2) Young people WA GAKUMON SITAI KARA, ... (MN1A36)
 particle
 topicalization

'Since young people want to study, ...'

B. Japanese has a verb 'SURU' meaning 'to do'. It combines with a noun, usually, of Chinese origin, and forms a new verb meaning 'to do whatever the noun refers to'. Thus, 'BENKYO-SURU' is a combination of a noun 'BENKYO' and a verb 'SURU'. Our informants combine English verbs with 'SURU' as in 3).

- 3) kids WA easy NI pick up SURU KARA. (GN1B88)
 'Since kids pick up (languages) easily, ...'

C. Japanese has the copula 'DA' which occurs after nominals, nominal plus particle, and nominal adjective. Our bilingual informants use English nominals, adjectives and prepositional phrases with the Japanese copula as in 4), 5) and 6).

- 4) ANO TOKI NO medicine DA KARA, ... (SS7B475)
 that time's
 copula particle
 since

'Since (it) was medicine of that time, ...'

- 5) KORE fresh DATTARA, ... (MN1A356)
 pronoun
 copula
 this
 conditional

'If this was fresh, ...'

- 6) All over place DATTA NO YO. (MN1B225)
 copula
 past

'(They) were all over the place.'

Notice that they are all complete Japanese copulative sentences. In 4) and 6), the subject is deleted; the subject deletion is common in Japanese when the subject is recoverable from the context.

D. Japanese nominals and adjectives are inserted into the English copulative sentences as in 7) and 8).

- 7) The ones we've seen are BIMBOO NA KODOMO. (MN1A329)
 nominal phrase
 poor child
 'The ones we've seen are poor children.'
- 8) That's YASUI. (MN1A109)
 adjective
 cheap
 'That's cheap.'

E. It is known that English and Japanese have mirror image ordering of constituents in surface structure (Smith 1978). For instance, English has prepositions, while Japanese has postpositionals. English conjunctions initiate clauses, while Japanese equivalents terminate them. Such structural correspondences seem to be the cause of the following type of switches:

- 9) We bought about two pounds GURAI KATTEKITA NO. (SS7A318)
 particle verb
 about bought
 'We bought about two pounds.'
- 10) That's all for her KENKYU NO TAME. (MN2A80)
 noun particle noun
 study of sake
 'That's all for her study.'
- 11) Because DASU KARA. (VS8A90)
 verb particle
 vomit because
 'Because (the dog) vomitted.'

In above three cases, what is once expressed in English is repeated in Japanese. The mechanism is that since the final elements of a sentence, a prepositional phrase and a subordinate clause initiated in English, could be the initial elements of their Japanese equivalents (due to the mirror image correspondence), the English elements preceding the final elements are repeated in Japanese.

Conclusion

All the sentences presented above demonstrate that the equivalence constraint does not hold in the Japanese/English data. English single nouns and noun phrases are followed by the Japanese postpositional particles, even though postpositional particles do not exist in English. A Japanese verb 'SURU' combines with an English verb, although there is no English equivalent of 'SURU'. The Japanese copula follows English adjectives, nominals and prepositional phrases, and the English copula precedes Japanese nominals and adjectives.

This happens despite the fact that position of the copula relative to location of nominals and adjectives differs in both languages. According to Sankoff and Poplack, switch is done between syntactic equivalents if they are also positional equivalents. In our data, switching occurs between syntactic equivalents, even though positionally they are opposite. As a result, repetition occurs.

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Linguistic and Social Aspects of Pachuco Caló: A Bilingual Variety of the United States-Mexico Border

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Sociolinguistic Background

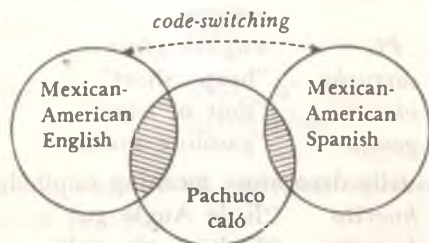
The Spanish caló of the U.S. Southwest is most commonly referred to as Pachuco caló, although other terms are also used, such as, *tirili*, *tirilongo*, *tarzán*, *bato*, *bato loco*, and many others. It appears undeniable (Webb, 1976 et al.) that the variety must have developed from the caló of Spanish Gypsies (Romanies), arriving from Andalusia, Spain, as early as the 16th century. A basic lexical stock has been demonstrated by various investigators, while Webb presents strong evidence that Pachuco caló (PC) shares much of its lexicon with calós of Brazil and Portugal (calão), Spanish America, as well as Rumania, Hungary, the Balkans and remotely the Apache of Paris.

Superimposed on the above base lexicon are a body of lexical creations, highly metaphoric and topical, completely intelligible only to those who enter into the street culture of the barrio slums. Sociolinguistically the speakers of PC are asocial or at best marginal, from the lower socio-economic status, illiterate or barely literate, ranging from merely defiant of middle-class norms to delinquent and criminal. They are composed of about 90 percent males, ranging from 14 to 30 years of age. As they near 30 they either integrate themselves into conventional society or succumb to narcotics or alcohol, often becoming hard-core criminals. Women share in the culture somewhat, and are treated "democratically" as long as they are able to rival men in the survival techniques and strategies of gang warfare.

PC is an urban variety with basically Spanish features. It exists in the entire Southwest, particularly in the cities and towns along the 2,000 mile U.S.-Mexico border. Calo provides an added register, especially for males below 50, used for a humorous and racy effect as well as to provide a touch of *machismo*. PC adds still a third possibility to the normal Spanish-English code-switching. Words from PC are even used by politicians or distinguished Latinos as a mark of solidarity. The Chicano movement has re-interpreted the role of the Pachucos, finding them more victim than aggressor, thus bringing more prestige to PC. The use of PC in Chicano literature is abundance, approaching a "cult" or perhaps "chic".

In short, it can be said that PC is fully a bilingual dialect, since it borrows heavily from both regional Spanish and English varieties in its lexicon, even though the grammatical norms of Popular Spanish are followed for the most

part. The diagram below illustrates the sociolinguistic situation of Pachuco caló in a somewhat simplified way.



Basic Lexical Stock

Although new lexemes are continually created in Pachuco caló, there is a basic lexical stock which is also drawn on as the examples below illustrate:

PC	English gloss	Source
<i>jando</i>	"money"	'jandorro' ("money")
<i>rache, rachi</i>	"night"	'rachi'* (Romany)
<i>bato</i>	"guy; fellow"	'bácsi' ("uncle") from Hungariam [Gypsy]

Linguistic and Rhetorical Devices for Lexical Innovation

PC uses a variety of different devices to continually innovate lexically.

1. Truncation (deletion of post-base form)

a. Apocope (syllable based) (deletion of final or penultimate final syllable)

[+syll] → Ø / (____) ____ #

PC	English gloss	Source
<i>compa</i>	"pal; chum"	'compadre' ("godfather")

b. Afaeresis (deletion of initial syllable) [+syll] → Ø / # ____

PC	English gloss	Source
<i>fileriar</i>	"to stab or knife"	'alfileriar' ("to knife")

c. Simplification (reduction by deletion of one or several constituents of a lexical compound)

PC	English gloss	Source
<i>Los</i>	"Los Angeles"	'Los Angeles'

2. Suffixation

a. Augmentative (semantic preference for connoting: hugeness, power, machismo, ugliness, contempt, craftiness, undesirability)

Suffix	PC	English gloss	Source
-on; -ona	<i>jambón</i>	"stupid person"	'jamón' ("ham")
	<i>huevoón</i>	"loafer; lazy bum"	'huevo' ("egg")
	<i>chavalón</i>	"lady killer, handsome devil"	'chaval' ("boy; kid")

*Spanish calo

-ote; -ota	<i>locote</i>	"high on drugs; crazy"	'loco' ("crazy")
	<i>buenota</i>	"physically attractive woman"	'bueno' ("good; good-looking")
<i>Suffix</i>	<i>PC</i>	<i>English gloss</i>	<i>Source</i>
-ucho; -ucha	<i>carrucha</i>	"heap; short"	'carro'
-aso	<i>vinazo</i>	"shot of wine"	'vino' ("wine")
-ofa	<i>gasofa</i>	"gasoline sniffer"	'gas' ("gas")

b. Diminutive (usually derogatory meaning implied)

-ito, ita	<i>huerito</i>	"little Anglo guy"	'huero' ("blond; Anglo")
	<i>huerita</i>	"little Anglo girl"	

c. Aspectual (adding the meaning of quick/abrupt action or intensification of an action or quality)

(i) Post-verbal

-ada	<i>pistiada</i>	"a big drunken brawl or spree"	'pistiar' ("to drink")
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(ii) Prepositional phrase

<i>a la chingada</i>	"screwed up; bad;"	'chingar' ("to screw")
<i>a todo dar</i>	"all the way; complete; super"	'all giving'
<i>al alba</i>	"wise-up; groovy"	'at the dawn'

(iii) Gender shift (change in category of gender to form a new meaning)

<i>grifo</i>	"male pot-head"	'grifa' ("pot")
<i>jefa</i>	"old lady; Mom"	'jefe' ("the old man; Pop")
<i>puto</i>	"male whore; pimp"	'puta' ("whore")

2. Metaphorical Process (primary strategy of PC speakers; in most cases correlative analogy is used)

<i>mango</i>	"sexy woman"	'mango' ("fruit")
<i>vaisa</i>	"hand"	'vise' (from English)
<i>antorcha</i>	"match"	'torch' (for English)
<i>arroyo</i>	"Rio Grande"	'arroyo' ("creek; wash")
<i>talonear</i>	"to hustle"	'talon' ("heel") (from Spanish)

3. Phonotactic Recombination (often aimed at either semantic concealment or humor)

a. Metathesis:

<i>pader</i>	"wall"	'pared' ("wall") from Spanish
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b. Blends:

<i>manil</i>	"money"	'money' + <i>il</i> (("nonsense"))
<i>binoles</i>	"beans"	'beans' + 'frijoles' (("beans"))
<i>aceite</i>	"thus; so"	'así' ("thus" +
PC	English gloss	Source 'aceite' ("vinegar"))

c. Semantic-phonetic analogy (A play on words between English and Popular Mexican Spanish)

<i>cortos</i>	"shorts"	'corto' ("short") associated with Eng- lish "shorts"
<i>peseta</i>	"hard; difficult"	'pesado' ("heavy") Associated with Sp. "peseta"

4. Verbal play

d. Verbal play:

- (i) Rhyming (a large series of forms for a given lexeme produced in competition with a fellow PC speaker)

nanay, naranjas, nariz,
narices, negros, nela, nels, "no"
nelson, nelsen, nenel, nicle,
nicolás, niguas, nil,

- (ii) Dummy insertions

tioso, tiososo "uncle" 'tío' ("uncle") from
 Spanish

5. Loans (borrowings from various languages and dialects)

a. Direct loans:

glasos "glasses" 'glasses' from English

b. Calques:

quebrada "to give someone a 'quebrar'
break" ("to break")

(*dame una quebrada*)

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A Socio-Cultural View of Language Contact: Building a Theory

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Prefatory Remarks

Despite the existence of numerous case studies dealing with language contact, we are far from a theory of language contact, yet monolingual models are insufficient to apply to multilingual and/or multidialectal situations. The purpose of this paper is to suggest some operational concepts for the building of a theory of language contact as well as some generalizations about language contact which might be tested throughout the world. It is argued in this paper that a socio-cultural view of contact which incorporates a broad view of language as well as language usage is necessary for the enquiry of such contact phenomena as: (1) language shift—an eventual change in the pattern of speaking of an entire group of speakers; (2) code choice—code-switching, diglossia, and style shifting by means of bilingual dialects in given interactional situations and domains; (3) linguistic borrowing—an obvious type of linguistic convergence which primarily affects the lexicon; (4) language standardization—planned convergence; (5) pidginization/creolization—the creation of new languages; (6) bilingualism—the result of degrees of social and linguistic convergence; and (7) language attitudes.

In order to build a theory of language contact, it would first be useful to discuss assumptions about the socio-cultural nature of language which are operationally crucial for the enquiry of language contact. The following assumptions are briefly discussed along with some of the implications for methodology and field work in the study of language contact phenomena.

Fundamental Assumptions about Language

1. Codes (languages/dialects) are acquired social symbols which are culturally defined and which speakers are aware of to some degree.

Literature detailing research on language contact has made reference to at least three symbolic values which are useful to explain code choice and other contact phenomena. These include (a) prestige value; (b) stigmatized value; and (c) neutral value. What can be noted briefly is that within contact situations a code may have very different symbolic values from one sub-group to another. For example, in Nigeria, British English has prestige value in academic and highly educated communities whereas it has a stigmatized value when used with uneducated members, such as, policemen or members of the army.

A second part of this assumption maintains that there are indeed different

categorical distinctions which communities of speakers make of verbal codes within a contact community and these distinctions are in turn associated with symbolic values. We would suggest here that the degree of awareness of the differences in verbal codes along with categorical distinctions have a great deal of bearing on *when* codes are used and *why*.

2. Language is both linguistic and socio-culturally contextual.

This assumption implies that at least two dimensions are necessary when examining language contact: (1) the linguistic context, i.e. the linguistic form or code embedded within other linguistic forms; and (2) the speech event or interactional context. Methodology for studying language contact must take into consideration both of these concepts in order to capture accurate contact generalizations.

3. Language has a variety of social functions which operate in addition to the referential function.

Function is here being used in the sense of inferred roles of language at the highest level of abstraction rather than domains of usage (Mathiot and Garvin 1975). It is suggested that different social functions can be inferred for each contact community at both the micro-level (face-to-face interaction) and the macro-level (society-at-large). The following functions are suggested:

- a. *Solidarity or in-group function*: A group-defining function in which verbal codes denote in-group/out-group relationships (Barker 1972).

- b. *Status marking function*: The use of a code to reflect both the social structure and the individual speaker's participation in that structure. In contact situations, verbal codes often carry different social meanings and a different status association which is culturally defined by the sub-group using it. The understanding of this function in contact communities can explain code choice and other contact phenomena.

- c. *Neutralizing function*: In opposition to status marking is the need to avoid relating the individual to the social structure, especially if power relationships are involved and there is need to defuse the lack of social equivalence. The neutralizing function may be used to avoid marking status, ethnicity, social class and other social categories defined by the social structure.

- d. *Theatricalization function*: The use of verbal codes or the alternation between them is often found to reflect an awareness of a group other than interlocutors, i.e. an audience along with the display of something valued in the society. An understanding of this function may be useful to explain what seem to be counter-rules of linguistic convergence. For example, in the United States Southwest, middle-class and mainstream Mexican-Americans readily borrow lexical items into Spanish from *caló*, a stigmatized code spoken by delinquents and norm-breaking Mexican-American teenagers.

4. Speakers participate both in the society-at-large and face-to-face interactions.

This assumption implies that individuals have both interpersonal roles, defined through small group interactions, and societal roles, defined by the

social structure. These roles may or may not be equivalent in the pressure which they convert on language use in contact situations. In short, this assumption highlights the notion that social group variables may not correlate with linguistic variables in a direct fashion so that it is simplistic to suggest that social networks can be directly traced to language use. The implication of this assumption is that our research must look beyond traditional social science classifications like age, sex, socio-economic status, caste, and discover the culturally defined variables that are significant in face-to-face interaction.

5. Standard languages operate differently than non-standardized languages so that the degree to which a variety is standardized affects its usage and receptivity to change.

Since a standardized language is codified, by definition, and has an institutionalized means of enforcing the code, there is logically more emphasis placed on the linguistic form or "the correct way to say something". Researchers have noted that standardized languages also are characterized by a greater degree of language loyalty, pride, and desire to participate in wider speech communities (Garvin, 1976). Regarding non-standardized languages, although they too have norms, there is more concern with *when* to use a particular code in terms of interactional contexts than *how* to use it correctly. For example, in contact situations we would suggest it is a much bigger mistake to choose the wrong or inappropriate code than to choose the correct or appropriate code and yet not speak it perfectly. This assumption implies that in contact situations social circumstances and variables may be more crucial than the linguistic form. If the above does hold true, as well we might expect a great degree of flexibility in the code and more openness to linguistic change, perhaps convergence.

Having suggested some crucial concepts of language for language contact, along with an extremely brief discussion, it would be useful to also propose some generalizations about language contact which could be tested in a variety of situations in the world. These generalizations are listed below with a brief discussion where space permits. (For much more detail and several examples, contact the author for the paper)

Some Generalizations to be Tested

1. The degree to which a language is standardized influences its linguistic convergence potential in multilingual communities.

This generalization relates to Assumption #5. It seems reasonable that if there is great flexibility regarding the linguistic form of a code as perhaps exists for non-standardized languages in contact communities, then non-native speakers of various degrees of competency will participate in using such codes perhaps bringing about a greater degree of linguistic convergence.

2. Language loyalty in practice can predict resistance to linguistic convergence.

Language planning literature suggests that cultural groups who have remained loyal in practice towards their language and who have had the leader-

ship to enforce or procure this loyalty have often been successful in resistance to borrowing or language shift. The case of Native American Indian languages in the United States may negate this generalization or better yet shed light on it (see Bates Hoffer).

3. Attitudes are only indirectly related to linguistic convergence. Rather it is the functions of language which are often predictive of linguistic convergence in contact situations.

Researchers in language contact have cited numerous cases where negative attitudes exist toward other languages or native speakers of these languages, yet linguistic borrowing occurs profusely or even language shift. On the other hand, there are other cases cited in the literature where positive attitudes exist toward another language or native speakers of this and yet linguistic convergence does not take place. Therefore, this generalization suggests that social function is more crucial to explain contact phenomena than language/dialect attitudes.

4. Language shift is always preceded by widespread bilingualism and a shift in functions for the languages involved by some groups in certain domains of usage.
5. Linguistic divisions and language usage patterns may not be congruent with non-linguistic or socio-cultural divisions.

This generalization suggests that social variables may only indirectly be related to linguistic differences. Therefore, a universal set of social variables for the study of language contact may be misleading. Rather researchers must first determine the symbolic associations which members of a community may have toward the codes involved, since it is these symbolic associations and values which are crucial in defining linguistic divisions.

6. Social functions of language determine the degree and direction of linguistic convergence or borrowing.

Haugen (1969) refers to "prestige differential" in which social pressure is placed on one language because another has more prestige and/or power so that the less prestigious language continues to borrow from the more prestigious. However, the "prestige differential" is not always sufficient to explain linguistic convergence. In at least some instances, stigmatized codes are borrowed from heavily and Generalization #6 suggests this is so because of the social function of a particular code in the contact community, such as illustrated previously by Pachuco caló in the U.S. Southwest.

7. Code-switching is a type of social marking which performs different language functions defined by each individual speech community.
8. The degree of bilingualism and the pattern of language acquisition of two structurally similar languages can predict the amount and type of borrowing.

This generalization hints at the possibility of relating two different levels of linguistic convergence to cultural convergence and linguistic competency. Since different generations of bilinguals often reflect different systems of bor-

rowing—as primarily determined by acquisition patterns and degrees of competency—there is as a result difference in frequency and type of borrowing. Determining what each of these consist of would, of course, not be an easy task.

9. The level of competency in a bilingual community where two structurally similar language systems are in contact may predict the type of lexical borrowing (convergence).

Using Haugen's classification of types of lexical borrowing combined with that of Weinreich, the following correlations are suggested. *Loan translations* or *calques* require the greatest degree of knowledge of the internal structure of the source language and thus might be considered to reflect the greatest amount of convergence between the source and recipient language. *Loan blends* or *hybrid* terms reflect the next greatest amount of convergence with a partial transfer of the internal linguistic structure from the source language into the recipient language. *Loanwords* reflect some degree of phonemic convergence—depending upon the degree of integration—but very little lexical convergence as well as little required knowledge of the internal lexical structure of the source language. *Loan renditions* and *loan creations* reflect only cultural convergence with hardly any linguistic convergence since they draw entirely on the linguistic resources of the recipient language.

If the above correlations do hold true, it might be possible to assess contact communities quantitatively according to linguistic convergence at the lexical level.

Conclusion

In this paper we have argued in favor of several basic assumptions about language useful to arriving at language contact generalizations or perhaps universals. Some tentative generalizations produced from various case studies of contact situations are suggested for future testing. It is hoped that through discussion of the above and agreement as well as disagreement a working theory of language contact might be arrived at as well as universals.

Accommodation in Interpersonal Encounters

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In an attempt to deal with the question why speech modifications occur, Giles, Taylor & Bourhis (1973) suggested a theory of interpersonal accommodation. It is stated that if a speaker, member of one ethnolinguistic group, chooses, as a means of facilitating social attraction, to speak to another in the language of his/her ethnolinguistic group, two results will follow: the second speaker will display positive attitudes towards the first and will also seek to accommodate the first speaker. On the other hand, if a speaker is looking for greater social distance, he/she will display a divergent speech style (Giles, 1973).

The present study was designed to determine: (a) to what extent student responses to the use of English (E) or French (F) in interpersonal encounters meet the ends sought by recent language legislation and, consequently, the changing linguistic behavior and expectations in the province of Québec; and (b) whether an accommodative speech act by an English Canadian speaker is differently perceived from an accommodative speech act by an English American speaker.

The recent language legislation referred to is Bill 101 (1978), which has given F the status of the official working language of the province. Québec is predominantly French speaking. However, E is the dominant language on the North American continent and the English speaking minority in Québec has long enjoyed numerous privileges because of its dominant economic position (Taylor, Meynard & Rheault, 1977). French speaking Quebecers were forced to learn E for reasons of vocation and professional survival. The new language legislation and the re-election of the Parti Québécois is causing an increase in the strength of and confidence in the French language and culture.

The question, then, is whether a change in the values of French Canadians has occurred. A scenario between an English-speaking customer and a French Canadian salesman in a Montreal bookstore with deliberately ambiguous conditions as to who should linguistically accommodate whom was presented to 160 French speaking secondary school students, who were then asked to give their reactions. In the first four conditions, a male speaking E identified himself as an English Montrealer and in the second four conditions, the same voice, with no alterations in accent, pitch or intonation, identified himself as an American. If, as has been suggested (Heider, 1958; Giles et al, 1973), accommodation is perceived in terms of ability, effort and external pressure, then the English Canadian and the American should be perceived the same way, since their performances were identical. In addition, new language legislation in

Québec would mandate the use of French as the language of communication for both speakers. This implies that the French Canadian salesman may speak in F, even if the English speaker indicates that he prefers E, and furthermore, that the customer should accommodate the salesman and use F. Conversely, social norms such as "the customer is always right", as well as previous language norms in Montréal, would suggest that the French speaking salesman should accommodate the English speaking customer in the language the customer chooses to speak. Or, in order to ease the speech act, the salesman should use his mother tongue.

Genesee & Bourhis' study (reference note 1) found that the use of E by the salesman and by the customer was accepted and viewed favorably, whereas the use of F by the salesman was perceived less favorably by both English and French speaking subjects. The question then is to what extent these findings are still true in a fast-changing society such as Québec, and moreover, to what degree the new language legislation is already reflected in the reactions of a French speaking population to language use.

The subjects listened to tape recordings of simulated dialogues between a customer and a salesman in a Montréal bookstore. The dialogue was comprised of three speech acts which were presented in the following order: customer—salesman—customer. An English speaking Canadian played the roles of the customers, a Canadian and an American, and a French speaking Canadian played the role of the salesman. Both the English speaking customer and the French speaking salesman had a competence in the reciprocal language. Eight different dialogues were recorded. The English speaking Canadian introduced himself as a Montrealer in the first four dialogues, and as an American visiting from the U.S. in the last four dialogues. Except for this information relating to the customer's background, both groups of dialogues were identical. The dialogues varied in terms of the language of response (E or F) of the salesman to the customer and the language of response (E or F) of the customer to the salesman. The customer always started off in E. The two groups of four dialogues each consisted of the following pattern:

Condition:		Language of:		
		Customer (Act 1)	Salesman (Act 2)	Customer (Act 3)
English Canadian	1.	E	F	F
"	2.	E	F	E
"	3.	E	E	F
"	4.	E	E	E
American	5.	E	F	F
"	6.	E	F	E
"	7.	E	E	F
"	8.	E	E	E

The customer could always easily be identified as an individual whose mother tongue was E, because his initial speech act was always in E and also because of his noticeable native E accent and grammatical difficulties when speaking F. The salesman as well could always be identified as a Québécois because of his noticeable native F Canadian accent and syntactical errors when speaking E.

A questionnaire was handed out to the subjects on which they indicated their impressions of each speaker and speech act. The questionnaire was set up and administered in F, since it was addressed to a French speaking population. Subjects were encouraged to give their reactions to the customer and the salesman after they had listened to the speech acts. Rating scales were straight horizontal lines with the extreme left always labelled 'not at all' and the extreme right always labelled 'very much'. The following scales were used: for the customer—'friendly', 'intelligent', 'comfortable', 'customer respects salesman', 'customer masters F' and 'customer makes an effort to speak F'; for the salesman—'friendly', 'intelligent', 'salesman masters E' and 'salesman makes an effort to speak E'.

The ratings on each scale were analyzed individually using ANOVA analysis of variance. The main variables were language used by the salesman (E, F), language used by the customer (E, F) and nationality of the customer.

The results of this study show that newly developed linguistic standards, which establish F as the official language in Québec, played an important role in the subjects' reactions to the use of E and/or F. This in spite of the fact that when asked which of the ethnolinguistic labels would best apply to describe their own identity, only about half of the students (55%) opted for 'Québécois', with almost equal numbers describing themselves as 'Canadian' (24%) and 'French Canadian' (21%). None of the participants of the scenario was viewed more favorably for accommodating the other speaker. It would therefore seem that these findings ought to be interpreted in light of the new linguistic realities in Québec and not in terms of interpersonal accommodation. However, interpersonal accommodation seemed to have influenced the students' impressions with respect to the English speaking customer's sense of well-being, and with respect to their own sense of well-being, had they been in a similar situation. The results indicated that whenever the salesman accommodated the customer and spoke E, the customer was thought to feel more comfortable. This could mean two things: either the French Canadian students were still accepting, in social contexts, the pre-existing language norms which had given the advantage to E or they felt that a customer, and in particular a foreigner, ought to be served in his/her native tongue.

At this point, it is interesting to note that the American customer was regarded as feeling significantly less comfortable than the Canadian customer in a situation where the salesman did not accommodate. This finding constituted the only difference in the subjects' reactions to the American and the Canadian customer. A possible explanation would be that the English speaking

Canadians from Montréal were viewed as having become accustomed to language uncertainty in interpersonal encounters. Concern about feeling 'comfortable' might have been diminished by the basic necessity of being served. This finding might also indicate that French Canadian subjects were more concerned with and more sensitive to a foreigner's well-being than with and to that of an English speaking Canadian. The external pressures on a foreigner to speak the language of another country might be considered less onerous than those on a native whose mother tongue is not that of the majority. Therefore, a less 'comfortable' state of well-being might have reasonably been attributed to the American speaker. The subjects also perceived reciprocal language accommodation as increasing the participants' sense of mutual respect. In such a context, this finding may indicate the qualifying of 'respect' as humanitarian rather than a linguistic value.

That the students' values were in a state of transition was also revealed by their rating on the ease with which the salesman would be able to convince them to buy something. Here, they clearly adopted the position of a French Canadian customer, who would rather be served in F than in E. However, when they were asked how they would feel, had they been in the situation in which the English speaking customer found himself, they adopted the view of an English customer.

There was no evidence of in-group preferences. The French salesman's effort and ability to speak E was not rated higher than the English speaking customer's effort and ability to speak F. The same was true for the personality traits of friendliness and intelligence. In group preferences had still been at issue in studies recently undertaken in Montréal (Genesee & Bourhis, reference note 1; Genesee, Tucker & Lambert, 1978).

In conclusion, the findings of this study indicate that the speakers in this interpersonal encounter were judged primarily in terms of recently established linguistic standards and the growing strong identity with and confidence in the French language and culture in Québec. To a lesser extent, the speakers were judged in terms of accommodative behavior. These findings differ from those of Genesee & Bourhis (reference note 1), where the control group of French Canadian students not only accepted E but also attributed positive value to its use. The state of transition of linguistic standards in Québec society is reflected by the results of the present study. The evaluation of personality traits (e.g. 'friendly', 'intelligent') depended almost exclusively on the new positive perception of F in Québec, whereas the evaluation of behavioral traits (e.g. 'comfortable', 'respect') conformed with the psychological expectations of interpersonal accommodation.

It is suggested that a similar study be done within a reasonable time interval in order to follow closely the changing linguistic and social standards in Québec. It might be of interest also to include English Canadian subjects to see to what extent their values and perceptions of language use in Montréal have changed. A comparison of the results of both groups might reveal interesting insights.

Reference Note

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"Paradigm" or "Current" in the History of Linguistics

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During the past twenty years, interest in the history of linguistics has increased considerably. It is noteworthy that the efforts made in this field of research have been going on since the Twelfth International Congress of Linguists held in Vienna in 1977. The number of publications relating to the history of linguistics has continually mounted over the last few years. However, in spite of this fact, we cannot help stating that as far as the methodological and epistemological bases of linguistic historiography are concerned, the situation is not satisfactory.

At the Twelfth Congress of Linguists, Koerner rightfully pointed out in his plenary report that there are few signs of a unanimous consent among historians of linguistics with regard to the approach to be taken and the methods to be employed in linguistic historiography. Indeed, there is no general agreement about the aims and motives of linguistic historiography.

Many scholars working in the history of linguistics stress the necessity of a general theoretical framework, knowing the limitations of approaches concentrated only on particular details and desiring to avoid an impressionistic or subjective attitude towards theories, views, ideas, notions and explanations of previous schools or thinkers. In order to arrive at a theoretical framework, methodological reflections on writing the history of linguistics are surely indispensable. And in this connection it is recommendable to take into account experiences and criteria at the methodological level which have stood the test in general historiography and in the history of science.

Thomas Kuhn's conception of the history of science, developed in his famous book "The Structure of Scientific Revolutions", first published in 1962, has had a considerable impact on the historiography of social sciences and humanities. This book has surely become so influential for the reason that Kuhn links ideology to science and introduces social aspects into the history of science. In the field of linguistic historiography, too, Kuhn's ideas have aroused a remarkable resonance. On the one hand, Kuhn's model was acclaimed and more or less imitated in some respects by scholars of linguistic historiography. On the other hand, there were linguists who criticized judiciously the application of Kuhn's principles to the history of linguistics. Finally, we encounter some attempts to introduce principals and notions from Kuhn's model into linguistic historiography, whereby they become reformulated in accordance with the requirements of our discipline. One aim of this reformulation is to conceive some key-notions without the implications which they have within Kuhn's

conception.

The fundamental key-notion in Kuhn's conception is doubtless that of "paradigm" which combines cognitive and social aspects and includes a scientific community sharing a "disciplinary matrix". The disciplinary matrix, another term used by Kuhn in his version of 1970 instead of paradigm, consists of four components: symbolic generalization, models, values and exemplars. These components show that Kuhn conceived his historiographical model only for the natural sciences. But it should be possible to easily modify these components adapting them to the investigation of linguistic historiography. As Kuhn formulates, the paradigm "stands for the entire constellation of beliefs, values, techniques, and so on shared by the members of a given community". Characterizing a current or a school, many observations made in this respect by Kuhn prove to be fruitful.

Nevertheless, there is another problem: Paradigm, according to Kuhn, involves the idea of "normal science" exclusively dominating over a long period; or formulated otherwise: normal science is a period in which a paradigm is an established source upon which practising scientists can draw. In this case, paradigm is conceived in a too absolute manner for as normal science it excludes the existence of other paradigms, be it simply an alternative paradigm. There can be only one valid paradigm with its generally accepted theory, techniques and convictions. But in the history of social science and consequently in that of linguistics, we do not encounter examples illustrating this conception of Kuhn. In every period of the development of linguistic thought, besides a prevailing current there have always been other more or less distinct currents and movements. In Kuhn's opinion, the existence of several schools is typical of the pre-paradigmatic periods in the development of a scientific discipline. For him, only before the first paradigm has been established, do a number of schools compete for the domination of a given field. Kuhn identifies paradigm with mature science and distinguishes between disciplines which have attained scientific maturity and those which have not. And there is no doubt for Kuhn, although he does not express this specifically, that the social sciences do not have this maturity and remain in the pre-paradigm stage. From this standpoint, linguistics, too, belongs to this field in which scientific maturity is absent, or else, as Keith Percival formulates, "linguistics is a field which (though scientific) intrinsically eludes analysis in terms of paradigms".

Indeed, the notion of paradigm, as conceived by Kuhn, with all its implications is not good for the history of linguistics. It seems preferable to me to use the traditional notion of current and to distinguish within it certain schools. However, it is necessary to elaborate more exactly than has been mostly done the theoretical base and the methods of a current and its bifurcation in different schools. Furthermore, it is recommendable to attach importance to these social-psychological aspects which Kuhn emphasized. But that does not mean to conceive a scientific community as a closed circle reminding of the "monade" in Leibniz' philosophy. Studying the development of linguistics in the past

century, for example, it seems very difficult to employ the notion of paradigm according to Kuhn's conception. And it is characteristic that all the efforts made in this direction have finally failed. However, operating with the notions of current and school we have a possibility of envisaging the complex evolution of linguistic thought and of linguistics as an academic discipline.

Many historians stress that Kuhn includes the social aspect in the development of science. However, Kuhn understands by this social aspect only social-psychological factors. Such factors are for him the interests, authority and status of scientists. The social aspect consists mainly, in his view, in the behaviour of the scholars, in the interactions within a scientific community. For this reason a French scholar has proposed that Kuhn's paradigm may be conceived as a mixture of theory and "coterie".

Needs resulting from the socio-economic development are not included in the social aspects investigated by Kuhn. Incidentally, as his adversary K. Popper maintained for a long time, he also considers science as an autonomous self-regulating system. But the development of science or of linguistics should not be grasped and explained independently of the socio-economic evolution, for only in this way is it possible to recognize the complexity of scientific life with its results and interdependence vis-a-vis social phenomena. Science is not restricted to a stock of factual knowledge together with theories, rules and techniques. It constitutes a socio-economically determined system of specific social activities which produces, reproduces, applies and spreads knowledge.

A history of linguistics, too, should not be only a presentation of theories succeeding one another with its epistemological components and its methods. In a history of linguistics there must be taken into account at the same time the premises and conditions connected with linguistic research, and this requires references to the socio-economic evolution. Therefore the aims of linguistic historiography with special regard to the different periods should consist in elaborating 1) the needs of social practice relevant to the development of linguistics; 2) the main concepts of linguistics in their connection with the thoughts of the general intellectual movements; 3) the internal processes in linguistic investigation, its epistemological problems and employed techniques. There can be no doubt that an important task is to disclose the cognitive, namely internal scientific processes effective in the approach to the object of linguistics which has changed in the development of our discipline. The most important and at the same time the most difficult task consists, in my opinion, in grasping the mediations between these three levels. Only to the extent to which we succeed in overcoming the juxtaposition of these three levels do we approach the aspired aims.

Starting from this framework I have tried to describe in my paper, it should also be easier to characterize different currents with their ramifications into schools and to explain changes of attitude or method within the development of linguistics.

Historische Perspektiven und künftige Anforderungen an die Fachsprachenforschung

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Würde sich ein linguistischer Laie in einige dieser zahllosen Vorträge verirren, müßte er bald feststellen, daß hier und dort nicht nur über ihm unbekannte Dinge gesprochen würde. Auch die Sprache selbst wäre ihm eigenartig und fremd, eine Sprache, die offensichtlich mit den Dingen zu tun hat, mit denen die Linguisten umgehen.

Auch die Linguisten haben seit einigen Jahrzehnten bemerkt—nachdem sie die Fachsprachen mittelalterlicher Handwerke und Zünfte, vorindustrieller Techniken und Verkehrsformen, neuzeitlicher Produktions- und Wissenschaftsbereiche untersucht haben—, daß auch sie eine Fachsprache benutzen. Die heutige Fachsprachenforschung spiegelt in ihrem Umfang und in ihrer zunehmenden Bedeutung in der Konkurrenz linguistischer Interessengebiete die Rolle wider, die die Fachsprachen auf nationalsprachlicher Ebene und in der internationalen Kommunikation einnehmen. Als Folge der modernen arbeitsteiligen Aufgabendifferenzierung, als Folge der Entwicklung und Aufsplitterung menschlicher Tätigkeitsfelder und Interessensbereiche in fachlichen Zusammenhängen haben sich zahlreiche Fachsprachen als spezifische Kommunikationsmittel historisch allmählich entwickelt, und entwickeln sich zunehmend neue Spezialsprachen. Sprachgeschichtlich nachweisbar ist das entstehende Bewußtsein von fachlichen und fachsprachlichen Bereichen im Deutschen am Auftauchen der Wörter *Fach*, *Fachmann* und *Fachsprache* im 19. Jhd. Doch nicht erst dieses Datum markiert den Beginn der Fachsprachenforschung.

Die antike Rhetorik als Theorie und Kunst der wirkungsvollen prosaischen Redegestaltung unterscheidet die Redeweisen der *perspicuitas* (Klarheit, Durchsichtigkeit, Verständlichkeit) und der *obscuritas* (im Sinne von 'Dunkelheit', 'Schwerverständlichkeit'). Der Begriff *obscuritas* ist bei Quintilian in seiner „*institutio oratoria*“ (Buch VIII, Kap. 'De perspicuitate') explizit eine Beschreibungskategorie auch für fachliche Redeweise und *termini technici*, die der Forderung nach Allgemeinverständlichkeit und Klarheit der Rede widersprechen. Überzeugende Argumente stellen einen ideengeschichtlichen Zusammenhang her zwischen dieser rhetorischen Dichotomie der Antike und der theoretischen Zweiteilung von „*Gemeinsprache*“ (Standardsprache) und „*Fachsprache*“ in der heutigen Fachsprachenforschung.

Ein weiteres Kriterienpaar der Antike, *prodesse* und *delectare* (Horaz) als die beiden untrennbaren Funktionen der Literatur, ist erst in der modernen

Literaturwissenschaft wieder aufgegriffen und auch für die Beschreibung von Fachtexten nutzbar gemacht worden. Wurden in der Antike und bis zum Beginn des bürgerlichen Humanismus (wie auch in der späteren klassischen Philologie) unter dem Begriff „Literatur“ noch alle geschriebenen Texte in Dichtung, Philosophie und Geschichtsschreibung, ja in jeglicher Wissenschaft verstanden, so wurde dieser Literaturbegriff, beeinflußt durch idealistische Auffassungen in der Ästhetik, schon bald auf fiktional-poetische Texte und auf die Funktion des *delectare* eingeengt. Die in dieser Konzeption ausgeschlossene Sach- und Gebrauchsliteratur (auch 'Fachschrifttum', 'Zweckliteratur' genannt), die u.a. alle Fachtexte umfaßt, ist erst heute anerkannter Untersuchungsgegenstand der Literaturwissenschaft.

Von historischen Gebrauchstexten ist bis heute das deutsche Fachschrifttum des Mittelalters am intensivsten bearbeitet. Nachdem zu Beginn des 20. Jhds. Mediziner (Vgl. Karl Sudhoff und sein „Archiv für Geschichte der Medizin“), Rechtswissenschaftler, Mathematiker, Astronomen und Philosophen aus wissenschaftshistorischem Interesse Neudrucke historischer Fachtexte besorgt hatten, begann in den 40er und 50er Jahren die Phase der intensiven Erforschung ma. Fachprosa in der deutschen Literaturwissenschaft. Der herausragende Forschungsgegenstand war die ma. Artesliteratur, die das antike Wissen über die 7 freien Künste (*artes liberales*), die 7 unfreien oder Eigenkünste (*artes mechanicae*) und die 3 verbotenen Künste (Magie, Mantik, Gaunertum) vermittelte und erweiterte.

Eine besondere Rolle für die heutige Fachsprachenforschung haben die sprachanalytischen Untersuchungen in der Wissenschaftstheorie des logischen Positivismus und der Philosophie der formalen Logik (Ende 19./Anfang 20. Jhd.) gespielt. Ausgehend von Überlegungen von G. W. Leibniz, mündeten die begriffstheoretischen Arbeiten von G. Frege schließlich in einer radikalen Kritik der natürlichen Sprache im Wiener Kreis und in der Forderung nach einer eindeutigen, logischen, formalisierten Einheitssprache für Philosophie und Einzelwissenschaften durch Carnap, Wittgenstein und Russell. Die sprachanalytischen Erkenntnisse der formalen Logik haben direkt eingewirkt auf die Praxis der gegenwärtigen Terminologielehre und Terminologiarbeit. Neben dem erkenntnistheoretischen Interesse in der Philosophie entstand zu Beginn dieses Jhds. in der sich rasch entwickelnden Technik das praktische Bedürfnis nach systematischer Klassifizierung, nach eindeutiger und differenzierter Bezeichnung und Dokumentation technischer Gegenstände, Materialnormen, Produktionsverfahren und Erzeugnisse. Die Pionierarbeiten von E. Wüster und dem Verein Deutscher Ingenieure (VDI) sind die Grundlagen der heutigen Normungsarbeit und Normungsempfehlungen der „International Organization for Standardization“ (ISO) und ihrer nationalen Normenausschüsse.

In den 20er und 30er Jahren dieses Jhds. entstand in einem weiteren nichtphilologischen Bereich, und zwar an den europäischen Welthandelshochschulen, die sog. Wirtschaftslinguistik (E. J. J. Messing, H. Siebenschein). Sie entstand aus der Situation eines verschärften Konkurrenzkampfes auf den

Weltmärkten nach dem 2. Weltkrieg, in der der steigende Fremdsprachenbedarf der Wirtschaft und die Vielfalt wirtschaftlicher Teilbereiche das Bewußtsein für die besonderen funktionalen Sprachmittel der Wirtschaft schärften und nach methodischer Ausbildung und Forschung verlangten. Sie forderte Untersuchungen, die über die bis dahin üblichen Wortschatzuntersuchungen hinausgingen, indem sie die Wirtschaftssprache als spezifisches, strukturiertes Kommunikationsmittel für wirtschaftliche Zwecke betrachtete, das in seinen verschiedenen Funktionszusammenhängen als Ganzes zu untersuchen sei.

Der funktional-strukturelle Ansatz der Wirtschaftslinguistik zeigt die Verwandtschaft mit der sich etwa gleichzeitig entwickelnden literaturwiss. linguistischen Theorie der 'Schriftsprache' in der Prager Schule, bekannt auch als Funktionalstiltheorie (Havránek u.a.). Hier bemüht sich ein Teil der Linguistik erstmals systematisch um methodologische und theoretische Untersuchungen über fachliche kommunikative Formen und Funktionen von Nationalsprachen. Eine der Prager Funktionalstiltheorie vergleichbare, wenn auch theoretisch weniger konsistente Auffassung entwickelten die Vertreter der London School (Neo-Firthians) mit ihrem *register*-Konzept (bes. M. A. K. Halliday).

Trotz der bisher genannten vielfältigen hist. Ansätze nahm die Fachsprachenforschung erst in den letzten 20 Jahren ihren immensen Aufschwung. Das Interesse hat sich von historischen Sprachzuständen auf die modernen Nationalsprachen in der Gegenwart verlagert. Ausgehend von einer lexikalistischen Position, wurden zunächst überwiegend die verschiedenen Fachwortschätze analysiert. Nach der Erweiterung des Fachsprachenkonzepts durch die modernen Vertreter der tschechoslowakischen Fachsprachenforschung (Fachsprache als Subsystem der Standardsprache) und nach der Gegenstandsbestimmung von „Sprache im Fach“ durch die Leipziger Schule (L. Hoffmann u.a.) sind die fest erbauten Bastionen ins Wanken geraten: trotz des rapiden Zuwachses neuer Fachsprachen und der drängenden Forderung nach linguistischer Analyse und Hilfestellung für die Praxis entsteht das Bewußtsein fehlender methodologischer und theoretischer Grundlegung der Fachsprachenforschung. Rein innersprachliche Kriterien haben sich als nicht ausreichend erwiesen. Damit bleibt eine Grundfrage der Fachsprachenforschung ungelöst. Um dieses Dilemma zu überwinden, sind an die künftige Fachsprachenforschung bestimmte Forderungen zu stellen: 1. Eine Lösung der methodologischen und wissenschaftstheoretischen Probleme der Fachsprachenforschung kann nur im außersprachlichen Bereich, im Konzept des *Fach*es gefunden werden: Für den Nachweis, daß bestimmte Kommunikationsformen fachsprachlich sind, ist der pragmatische Handlungsbereich des Faches mit seinen konstitutiven Elementen zu beschreiben, und es sind spezifische Relationen zwischen diesem Handlungsbereich und den in ihm verwendeten Kommunikations- und Sprachformen nachzuweisen. 2. Die Methodologie der Fachsprachenforschung muß sich neben linguistischer Methoden weiterer, insbesondere empirischer sozialwissenschaftlicher Methoden bedienen. Dies kann die Linguistik nicht allein

leisten, vielmehr müssen hier weitere Disziplinen wie Soziologie, Psychologie, Kommunikationswissenschaft, Verhaltensforschung, Arbeitswissenschaft, Wirtschaftswissenschaften und Geschichte zu einer Fachsprachenforschung und -theorie beitragen. 3. Das linguistische Interesse muß sich stärker auf mündliche Kommunikationsformen im Fach richten, die den Handlungszusammenhang des Faches eher darstellen und stärkere methodologische Hinweise liefern auf die fachkonstitutiven Experten, ihre Ziele und Interessen, Gegenstände und Produkte, Instrumente und Verfahren, sozialen Normen und Verhaltensweisen. Nur wenn sie diesen Anforderungen gerecht wird, kann die künftige Fachsprachenforschung einen Beitrag leisten zum Verständnis der in der heutigen Zeit zunehmenden spezialisierten Kommunikationsprozesse und ihrer gesellschaftlichen Ursachen.

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Case Grammar Theory, 1982

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Case grammar is not a grammar and does not deal directly with surface case. It is a semantic valence system used to describe the internal semantics of a clause in terms of a central verb and the deep case roles required by the meaning of that verb. Within internal semantics case grammar includes propositional elements but excludes modal and performative elements.

Case grammar theory has been developed by Charles Fillmore (1968, 1971, 1975, 1977), Wallace Chafe (1970), John Anderson (1971, 1977), Jeffrey Gruber (1965) and Ray Jackendoff (1972, 1976), and by some tagmemicists, including John Platt (1971), Robert Longacre (1976) and Kenneth Pike (1977). Although case systems have been developed independently within original or existing syntactic frameworks, it is possible to abstract the case systems and compare them apart from the particular syntactic models in which they are presented.

Comparison of the various case grammar models proposed reveals essential differences which present contrasting alternatives for anyone interested in case grammar theory. Some of the ways in which these models differ are (1) logical structure, (2) the list of cases used, (3) possible case frames, (4) the ways of relating forms by means of semantic derivation, (5) the use of covert case roles.

1. Logical Structure

What is the scope of a case analysis? What elements are to be included? What elements are to be excluded? Fillmore's position is that case analysis deals with the proposition, a tenseless set of relationships between a central verb and the nouns required by the meaning of the verb. In effect, the logical structure underlying a sentence is given in terms of a case-labelled predicate logic. It differs from predicate logic in that the arguments are labelled to indicate the role each argument plays in the situation described by the predicate. These case roles are imposed from the verb's semantic valence, and are completely independent of surface syntactic positions and surface case marking.

2. Case Systems

What is the correct list of cases? How are they to be defined? How can we know when the list of cases is complete? Fillmore's revised position (1977: 65) is that cases are relational not categorial notions. Cases are not to be defined in terms of features, but can only be defined in relation to the verb with which they occur. Nouns are not cases. Case roles are read onto the noun from the verb when the noun occurs with the verb in context. Most case systems are

mutually translatable. All systems have an Agent and some kind of neutral Theme. Localistic systems add Location, Source, and Goal cases (Anderson, Gruber, Jackendoff); nonlocalistic systems prefer Experiencer, Benefactive, and Locative cases (Fillmore, Chafe, Longacre). Most systems, except Fillmore, do not contain an Instrument case.

3. Case Frames

Given a list of cases, how do these cases combine in the description of predicates? How many cases may appear in a frame? Can any case appear more than once? Is there an obligatory neutral Theme? Which cases are mutually exclusive? One key to case assignment is the notion of verb type. State verbs express a static situation and are [-progressive, -imperative]; Process verbs express a dynamic nonagentive situation and are [+progressive, -imperative]; Action verbs express a dynamic agentive situation and are [+progressive, +imperative]. If one first asks: How many nouns are required? then, what type of verb is it?, and only then what case roles are to be assigned? the problem of case assignment is considerably reduced.

4. Semantic Derivation

When the verbs of a language are classified according to their case frames, it is evident that there are often State, Process, and Action forms derived from the same root. What case theory requires is a derivational system which will indicate how these forms are related. Chafe (1970) proposed a bidirectional system which links State, Process and Action forms by means of four derivational units: inchoative, causative, decausative, and resultative. Others assume the State form to be the basic form and use an unidirectional system with abstract predicates, such as BECOME for inchoative and CAUSE for causative. The bidirectional system is language specific; the unidirectional system is more universal, more analytic, and eliminates the need for choosing a base form.

5. Covert Case Roles

There are case roles in the logical structure that do not always appear in the surface structure. These include deletable, coreferential, and lexicalized roles. Deletable roles sometimes occur and sometimes do not occur, as in: *Mother is cooking*, with deleted object. The proper case frame can only be assigned when deleted roles are restored. Coreferential roles are two roles applied to the same noun phrase, as in: *Max went to Chicago*, where Max is both Agent and moving object. All case grammars, except early Fillmore and Chafe, use coreferential roles. Lexicalized roles are essential noun phrases which are incorporated into the verb form, as in: *Roger watered the lawn*, where the verb contains an incorporated object noun and means: *put water on*.

6. Case Grammar Theory

Case grammar is not a grammar and does not deal directly with surface

case. It is a semantic valence system which is used to describe the essential meaning of a clause in terms of a central predicate and a series of case-labelled arguments required by the meaning of that predicate. Case grammar is a case-labelled predicate logic.

Comparison of the various case grammar models, divorced from the syntactic context in which they are found, suggests that case theory requires a well-defined logical structure, a definite list of case roles, tactics for combining cases in case frames, a method of semantic derivation, and a coherent theory of covert case roles.

Case grammar theory is more than "a convenient, if somewhat arbitrary way of classifying the roles that noun phrases play in sentences." (Newmeyer 1980: 132) If these roles originate in the semantic valence of the verb, then they impose a classification upon the verbs of a language. This classification not only distinguishes different meanings of the same verb, but allows the grouping of verbs of similar meanings into well-defined verb classes. Verbal dictionaries and a thesaurus of verb types are possible within a case grammar analysis.

The separation of syntax and semantics in case analysis is only temporary. The most fruitful insights of a case analysis are in the syntactic correlates that follow from its verbal classification. It is this correlation of case analysis with syntactic form, in any model of syntax and in any language, that offers the brightest prospects for the theory that has come to be known as case grammar.

Case grammar research has already resulted in viable localistic and non-localistic systems having a small number of cases, universal in scope, necessary and sufficient for the classification of all the verbs in any language. Prospects for the future are not so much in the creation of new systems but in the application of existing systems to the fields of translation, speech pathology, child language acquisition, and language teaching, especially in the field of teaching English as a second language. All of these applied fields can profit from the systematic approach to meaning offered by a case grammar analysis.

Gabelentz und Sekiguchi

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1. Georg von der Gabelentz (1840-1893) ist in der neueren Geschichte der Linguistik als derjenige Sprachwissenschaftler bekannt, der mit seinen Sprachbegriffen „Einzelsprache“, „Rede“, „Sprachvermögen“ die Saussureschen Begriffe „langue“, „parole“, „(faculté) du langage“ vorweggenommen hat. Andererseits ist es von ihm allgemein bekannt, daß er in seiner Sprachauffassung zur Humboldtschen Tradition der Sprachwissenschaft gehört. Des weiteren gilt er als derjenige Forscher, dessen originellster Beitrag zur Sprachforschung in der Idee der „synthetischen Grammatik“ besteht, die er in seiner „Chinesischen Grammatik“ (1881) als erster in der Geschichte der Sprachwissenschaft realisiert hat. Die Begriffe des „psychologischen Subjekts“ und „psychologischen Prädikats“, die er im Rahmen dieser Grammatikkonzeption entwickelt hat, sind heute im Zusammenhang mit der sog. „funktionellen Satzperspektive“ aktuell geworden. Dies bedeutet, daß er in einer Zeit, in der man sich im Namen der Sprachgeschichte vornehmlich mit der Geschichte der indogermanischen Lautsysteme beschäftigte, eine Sprachforschung betrieb, deren theoretische Konzeption und praktischer Inhalt heute noch als bedeutsam und aktuell angesehen werden können. Bedenkt man außerdem, daß Gabelentz an der Formalisierung grammatischer Sachverhalte besonders interessiert war und in seinem sprachwissenschaftlichen Hauptwerk „Die Sprachwissenschaft“ (1891, ²1901) Beschreibungsversuche gemacht hat, die an Chomskys Transformationsregeln erinnern, so erhalten wir zunächst ein fast bizarres Bild von diesem Sprachforscher.

2. Tsugio Sekiguchi (1894-1958) gilt in Japan als ein hervorragender Vertreter auf dem Gebiet der deutschen Grammatik, dessen Werke mit ihrer z. T. recht originellen grammatischen Terminologie heute im dortigen Sprachstudium und -unterricht breite Verwendung finden. Seine Grammatikkonzeption gründet sich dabei auf ein eigenes Verständnis des Gegenstandes der grammatischen Beschreibung, den er in der von ihm sogenannten „Imikeitai“ (Bedeutungsform) sah. Unter der „Bedeutungsform“ verstand er in erster Linie den sich in Verbindung mit bestimmten formalen Mitteln jeweils im realen Kontext des Sprechens realisierenden Typ des einzelsprachlichen Inhalts, der weder mit dem einzelsprachlichen Inhalt („Bedeutung“ i. e. S.) noch mit dem konkreten sprachlichen Inhalt im Kontext („Sinn“) zusammenfällt. So sind „Indirekte Rede“, „Irrealis“, „Heischsatz“ solche „Bedeutungsformen“, die mit bestimmten Mitteln (Konjunktiv I/II, Satzadverbien, Modalverben, usw.) realisiert werden können. Einzelsprachliche „Bedeutungen“ dieser Formen selbst sind nämlich in einer viel abstrakteren Form gegeben, als man gemeinhin

annimmt; sie werden nicht als solche, sondern in bestimmten „Bedeutungsformen“ realisiert. Sekiguchi unterschied im übrigen als Voraussetzungen dieser „Bedeutungsform“, die der Hauptgegenstand der Grammatik sein soll, zwei weitere Arten der „Bedeutungsform“: die „Bedeutungsform I“ als grammatische Determinierungsrelation im Satz und die „Bedeutungsform III“ als lexikalische „innere Sprachform“ („Wortbild“, „Bedeutungsbild“), während die primäre „Bedeutungsform“ ggf. „Bedeutungsform II“ genannt wurde.

3. Sowohl Gabelentz als auch Sekiguchi waren Einzelgänger in ihrer Zeit. Gabelentz war in Leipzig, wo er von 1878 bis 1889 an der Universität als Professor wirkte, in seinem junggrammatisch orientierten Kollegenkreis recht isoliert; Sekiguchi blieb zeit seines Lebens ein Privatgelehrter und hatte keinen akademischen Wirkungskreis um sich. Wenn nun in den Ansichten der beiden Forscher etwas Gemeinsames festzustellen ist, so scheint es z. T. auf diesen Umstand zurückzuführen zu sein, der ihnen ein freies Sprachstudium ohne zeitbedingte Festlegung des wissenschaftlichen Interesses weitgehend zuließ. Und nach den Epochen der sprachwissenschaftlichen Entwicklung seit Anfang des 19. Jahrhunderts, die eine konsequente Abfolge einiger grundlegender methodischer Neuansätze darstellt, scheint es heute geboten zu sein, sich anhand der persönlichen Leistungen solcher unabhängiger Forscher auf das zurückzubessinnen, an dem die bisherige Sprachwissenschaft—nicht zuletzt infolge ihrer stofflichen und aspektuellen Festlegung durch die idg. Sprachen—vielleicht vorbei gearbeitet hat und noch vorbei arbeitet.

4. Der entscheidende Gesichtspunkt, den wir bei Gabelentz und Sekiguchi als ihre leitende Forschungsidee feststellen können, ist der, daß durch die Sprachforschung vor allem der Zusammenhang zwischen der Einzelsprache und der Rede geklärt werden muß. Gabelentz sagt: „Der Gegenstand der einzelsprachlichen Forschung, die Erscheinung, die sie erklären will, ist—dies sei nochmals hervorgehoben—die Sprache als Äußerung, das heisst die Rede. Wie kommt in der zu bearbeitenden Einzelsprache die Rede zustande, und warum gestaltet sie sich gerade so?“ (Die Sprachwissenschaft, 2. Aufl., S.59). Es geht ihm also in der synchronischen Sprachforschung nicht um die Erkenntnis der Einzelsprache selbst als autonomen Systems oder der Rede als Widerspiegelung und Materialisierung der Einzelsprache, sondern um die Klärung des Zusammenhangs zwischen ihnen, genauer: des Prozesses, wie die Rede aufgrund der Einzelsprache zustande kommt. Diese ist dabei nach Gabelentz „nicht sowohl die Gesamtheit aller Reden des Volkes, der Classen oder des Einzelnen, —als vielmehr die Gesamtheit derjenigen Fähigkeiten und Neigungen, welche die Form, derjenigen sachlichen Vorstellungen, welche den Stoff der Rede bestimmen“ (ebd., S.3). Die Einzelsprache als Komplex von sprachlichen Kompetenzen und Intentionen realisiert sich in der Rede jeweils auf eine bestimmte Weise. Dies muß wissenschaftlich erklärt werden. In anderen Worten: Der Vorgang des einzelsprachlichen Sprechens und nicht das Sein des einzelsprachlichen Systems ist der eigentliche Gegenstand der Sprachforschung. Hier zeigt sich eindeutig der grundlegende Unterschied zwischen

der Gabelentzschen „Einzelsprache“ und der Saussureschen „langue“, obwohl Gabelentz durchaus den Systemcharakter der Sprache bejaht und sogar betont (vgl. ebd., S.9). Und eindeutig ist auch die Nähe seiner Idee der Sprachforschung zum Humboldtschen Denken und zur transformationellen oder funktionellen Idee der Grammatik festzustellen.

5. Genauso geht es Sekiguchi mit seiner Bedeutungsformgrammatik darum, zu untersuchen, wie die in der Einzelsprache gegebenen grammatischen Formen und lexikalischen Inhalte, die als solche etwas äußerst Abstraktes darstellen, auf eine bestimmte Weise genutzt werden, um eine eigene sprachliche Welt bzw. Projektion in die Welt zu ermöglichen. Wenn etwa in der deutschen Sprache die zweierlei, systematisch bildbaren Konjunktivformen für die drei kommunikativen Modi der indirekten Rede, des Irrealis und des Heischsatzes genutzt werden, die in manchen Bereichen des sozialen Lebens durchaus eine wesentliche Rolle spielen, so wird im Japanischen die eine Diatheseform mit dem Hilfsverb *-(ra)reru* für die vier Modi der Leideform (*Ukemi*), der Höflichkeit, der Möglichkeit und der Spontaneität genutzt, die wesentliche kommunikative Zwecke im Sprachleben der japanischen Menschen erfüllen. All diese „Bedeutungsformen“ entstehen im einzelsprachlichen Sprechen selbst regelmäßig je nach dem Kontext, der durch das Subjekt des Sprechens als solcher erfaßt wird. Das sind sprachliche Formen, die nicht als solche gegeben sind, sondern grundsätzlich die Aktivität und Spontaneität des Menschen voraussetzen. Von hier aus ist auch die Idee der synthetischen Grammatik, die den Standpunkt des Sprechers statt des Rezipienten repräsentiert, zu verstehen. Die Sekiguchische Bedeutungsformgrammatik war tatsächlich eine solche.

6. Das Anerkennen des einzelsprachlichen Sprechens als eigenen linguistischen Gegenstandes impliziert ein Aufgeben des dichotomischen linguistischen Denkens, das die Linguistik seit Saussure bis Chomsky beherrscht hat. (Bei Chomsky selbst sind allerdings bereits Ansätze zur Überwindung der Dichotomie feststellbar). Die Sprache gestaltet sich jeweils im Sprechen durch die Aktivität des Menschen auf eine spezifische Weise, die als solche erkennbar ist. Dies gilt nicht nur für das Syntaktische, sondern auch für das Phonologische, Morphologische und das Semantische. Man hat es hier mit einem eigenen, sprachspezifischen Phänomen zu tun, das nicht in angrenzenden Disziplinen der Linguistik auflösbar ist. Durch das bewußte Festsetzen des eigenen Bereichs des einzelsprachlichen Sprechens erhält gleichzeitig das System der Einzelsprache einen erhöhten Zugang zum Universellen, indem dessen typologische Charakterisierungen eher möglich werden. Damit kommt der Linguistik eine erhöhte Chance zu einem Kulturstudium zu. Gabelentz und Sekiguchi waren Linguisten, die diesen fruchtbaren Weg gewiesen haben.

Saussurean Structuralism and J. R. Firth

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1.0 Introduction. An examination of J.R. Firth's writings, taken as a whole, reveals his ambivalence toward the tenets underlying Saussurean structuralism. But of the 6 major features in Saussure's work which Firth condemned, only one proves to be incompatible with Firth's position in his late works.

2.0 Mentalism. In Firth (1935b:36) the positing of mental structures in Saussure's work is condemned, suggesting a fundamental and irreconcilable difference between Firth and Saussure. However, a close reading of Firth's work from his later years reveals that the whole question of mental structures became a non-issue for him. Referring to the purpose of his 5-level analysis of meaning, Firth stated: "In doing this I must not be taken to exclude the concept of mind, or to imply an embracing of materialism to avoid a foolish bogey of mentalism." (1951:192) This statement is not so much a shift in position on Firth's part as an admission that the concept of mind had never been excluded from his view of necessity. Indeed, returning to 1935, we find an implicit indication of the admissibility of mental structures, provided that they are not treated in isolation (1935a:53).

2.1 The Duality of the Linguistic Sign. Far more vehement than Firth's criticism of the assumption of mental structures was his opposition to the dualist view of the linguistic sign, as expressed by Saussure's *signifiant/signifié* opposition. Firth makes what may be construed as a call for the abolition of the dualist theory of the sign, a call for the resolution of the mentalist/behaviorist dichotomy (1948:398). Firth's reaction against the dualist view seems to have led him to an extreme form of naive determinism. But only 3 years after putting forth such a view, he approvingly cited long passages from Hjelmslev's *Structural Analysis of Language* (1948), calling it 'dualism properly so-called.' The Hjelmslev work is inseparable from a dualist view of language, but it puts the emphasis on relational processes rather than on the entities related by processes, and it is for this reason that it meets with Firth's approval.

2.2 *Langue and Parole*. Saussure's *langue/parole* opposition was anathema to Firth because of its inherent abstraction and positing of collectivity. This antipathy dates from 1935: "There is no such thing as *une langue une* (sic.) and there never has been." (1935a:68) There is even indirect reference to Firth's conviction about the irrelevance of *langue* as late as Firth (1957a).

Firth's criticism of the concept of *langue* is consistent with his principles

but not with his practice. It ignores the obvious fact that whereas speech activity may reveal systems, the systems are not that activity itself. If the linguist is to look for something 'in' speech, as Firth himself says, that is, something recurring in samples of speech, that something must be distinct from the speech activity and, therefore, it must be treated abstractly. The 'suitable language' which Firth began to develop in 1935 was *serial contextualization*, which proved to be as much of an abstraction as Saussure's concept of *langue*. And though Firth could declare that science should not impose systems on language, he imposed a 5-way split on meaning in his first paper on semantics (1935a).

It is paradoxical, therefore, in light of Firth's vigorous and sustained objection to the abstraction of *langue*, to find that he ultimately characterized a key concept in his work, that of collocational meaning, as an abstraction: "Meaning by collocation is an abstraction at the syntagmatic level and is not directly concerned with the conceptual or idea approach to the meaning of words." (1951a:196)

2.3 *Synchrony/Diachrony*. Firth was extremely critical of Saussure's synchronic/diachronic opposition, calling it a fallacy (1935a:51) and demonstrating the irrelevance of the opposition, if not universally, at least to Firth's own method (1935a:51). But Saussure's concept does not explicitly or implicitly deny the "stretches of personal biography and cultural history... in which past, present, and future all meet" of which Firth spoke. In fact Saussure (1959:8) stated: "Speech always implies both an established system and an evolution; at every moment it is an existing institution and a product of the past."

2.4 *The Reification of Language*. In distinguishing *langue* from *parole* Saussure characterized it as product, thing, and object. Firth attacked this view. But when Saussure stated that he was defining language as a thing, he went on to say that he was defining the thing itself, i.e., the phenomenon of language, and redefining it, rather than relying on existing definitions and existing terminology, which he viewed as ambiguous. There was, therefore, no intention on Saussure's part to reify language, even when he spoke of it as object and product, for he added (1959:14):

Language is the social side of speech, outside the individual who can never create nor modify it by himself; it exists only by virtue of a sort of contract signed by the members of a community.

2.5 *Monosystematicity/Polysystematicity*. Monosystematicity, the analysis of language as one system, rather than as a system of systems, was Firth's chief objection to Saussurean structuralism and indeed the only such objection maintained with logical consistency. Interestingly enough, Firth identified the monosystemic approach with what Saussure had termed paradigmatic analysis (1948a:121). By contrast, his own polysystemic approach aligned with the syntagmatic (1948a:128). Firth called attention to the shortcomings of the monosystemic

approach, charging that it had been pushed beyond the limits of its applicability (1948a:137). Firth's criticisms may be construed not as a condemnation of the principles of Saussurean structuralism but as a condemnation of the failure to make full use of the syntagmatic/paradigmatic as complementary modes of analysis.

3.0 Conclusion. Despite the compatibility and even similarity between many of Firth's notions and those of Saussure, Firth insisted that he did not consider himself a structuralist, that he was not using the term *language* in the Saussurean sense of *langue*, and even that he was not a Saussurean "in the Russian pejorative sense." (1949:170) Such statements are difficult to explain when the essential methodological differences between Saussurean structuralism and Firth's position are seen to reduce to the difference between monosystematicity and polysystematicity. They are all the more difficult to explain in light of Firth's ultimate admission that conventional structuralist descriptions of language are a part of structural linguistics as he defined it.

The distinction between structuralist and structural was not a trivial point of terminology for Firth but a fundamental issue. He identified the theoretical basis of structuralism with Saussure and objected above all to the assumption of collectivity in the Saussurean concept of *langue*. Firth was equally critical of the practitioners of structuralism in North America, viewing monosystematicity of approach as their chief shortcoming. Closer to home, he found the roots of a wholly acceptable method—structural linguistics properly so called: "Expressions such as grammatical structure and phonetic structure have quite a long history. . . . We get nearer to structural concepts if we turn to our own country at a much earlier date, even as far back as Sweet. Everyone interested in structural as distinct from *structuralist* linguistics should study his 'Words, Logic and Grammar'." ([1955] Palmer 1968:36)

The reference here is to the 19th century Oxford phonetician Henry Sweet, in whose work Firth apparently wished to identify the origins of his own approach. The degree of importance which Firth attached to this identification with Sweet must remain a matter of speculation. He was, in any case, more obviously concerned to dissociate himself from both Saussurean and American structuralism, so much so as to have overemphasized the differences between the theoretical assumptions of his work and that of Saussure.

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The Speech-Communication Model in 20th-Century Linguistics and its Sources

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Communication, so far in human history, has taken one of three basic forms. The first and most primitive involves actual physical proximity of the participants. The second involves what has been summarized under the head of 'runners, riders and pigeons' (Rickards 1972)—that is to say, the use of a go-between or the transmission of a go-between object such as a written document from one participant to another. The third involves the development of a communications technology using electrical or electronic devices. We may call these three categories 'direct communication', 'communication by proxy', and 'telecommunication'. They apply to all kinds of communication, verbal and non-verbal.

Until the nineteenth century, speech communication fell entirely into the first of these three categories, and most of human speech the world over still belongs indisputably to the 'direct' type, involving face-to-face interaction. In spite of this, it is interesting to observe that when speech communication first came to be analysed for purposes of modern linguistic theory, the model employed was, I shall argue in this paper, derived from the more sophisticated modalities represented by the 'proxy' and 'telecommunication' types. Paradoxical though it may sound, modern linguistics has never conceptualized speech as a form of 'direct' communication at all. It is not an idle question to ask why.

The classic statement of the speech-communication model adopted in modern linguistics is given in Ch. III, §2 of the Introduction to the *Cours de linguistique générale*. It is what Saussure referred to as the *circuit de la parole*, or 'speech circuit', which is described as follows:

'The starting point of the circuit is in the brain of one individual, for instance *A*, where facts of consciousness which we shall call concepts are associated with representations of linguistic signs or sound patterns by means of which they may be expressed. Let us suppose that a given concept triggers in the brain a corresponding sound pattern. This is an entirely *psychological* phenomenon, followed in turn by a *physiological* process: the brain transmits to the organs of phonation an impulse corresponding to the pattern. Then sound waves are sent from *A*'s mouth to *B*'s ear: a purely *physical* process. Next, the circuit continues in *B* in the opposite order: from ear to brain, the physiological transmission of the sound pattern: in the brain, the psychological association of this pattern with the corresponding concept. If *B* speaks in turn, this new act will pursue—from his brain to *A*'s—exactly the same course as

the first, passing through the same successive phases...'

When linguistic theory came under the influence of behaviourist psychology in the U.S., the speech circuit model was not so much discarded as re-labelled. Concepts and mental images disappeared. The preferred explanatory terms were 'stimulus' and 'reaction'. But if we look at Bloomfield's exemplary parable of how Jack responds to Jill's request for an apple (*Language*, 1935, ch.2.) we find that, apart from the behaviourist labels, the isomorphism with Saussure's model is unexceptionable.

After the behaviourist interlude, the Saussurean model was reinstated in its fully-fledged form by American transformationalists. Thus in the 1960s we find (Katz 1966:103-4) a basic account of speech communication which could stand almost verbatim as an explanatory comment on Saussure's 'speech circuit':

'The speaker, for reasons that are linguistically irrelevant, chooses some message he wants to convey to his listeners: some thought he wants them to receive or some question he wants to ask. This message is encoded in the form of a phonetic representation of an utterance by means of the system of linguistic rules with which the speaker is equipped. This encoding then becomes a signal to the speaker's articulatory organs, and he vocalizes an utterance of the proper phonetic shape. This, in turn, is picked up by the hearer's auditory organs. The speech sounds that stimulate these organs are then converted into a neural signal from which a phonetic representation equivalent to the one into which the speaker encoded his message is obtained. This representation is decoded into a representation of the same message that the speaker originally chose to convey...'

In short, the mainstream of linguistic theorizing throughout the present century has relied basically upon the same model of speech communication, irrespective of the differences separating the various schools. Such a model, it might be supposed, must have extremely powerful sources of support, not only within the academic discipline of linguistics but outside it too.

Linguistic theorizing cannot be divorced from its social and intellectual context (Harris 1980), and this is nowhere better illustrated than by the answers we are led to if we ask where this somewhat restricted and implausible model of speech communication came from and why it managed to dominate the linguistic scene for so long.

Let us take a closer look at the model itself. The first point to note is a negative one. Although it explicitly divides the circuit into psychological, physiological and physical sections, it is quite obvious that Saussure never based his model upon contemporary findings in psychology, physiology or physics. The neurological, motor and acoustic processes involved were at that time not sufficiently well understood to offer the basis for such a model. Apart from one passing reference to Broca, there is nothing in the *Cours de linguistique générale* to suggest that Saussure's analysis of speech communication was in any way based on nineteenth-century advances in the sciences dealing with the actual mechanisms of speech. Its origins are much older.

In fact, Saussure's speech circuit is essentially a diagrammatic representation not of the directly observable facts of speech activity, nor even of the unobservable micro-events known to underly speech activity, but of a philosophical theory about something else assumed to be achieved through speech. The ancestry of this model is no mystery: it can be traced directly to what has sometimes been called the 'translation theory' of understanding, propounded in the 17th century by John Locke.

This theory of understanding has been analysed many times, and it would serve no useful purpose to recapitulate it in detail here. What it is relevant to point out is that although Saussure adopted this Lockean model for his account of the 'speech circuit', the rationale of the account is not the same in Locke's case as in Saussure's.

The first point to note is the complete symmetry postulated between encoding and decoding. For Locke, understanding what you hear another man say is simply a mirror image of what happens when you express an idea aloud by speaking. Hence the expression 'translation theory' of understanding. Understanding is explained as translating back into ideas what had previously been translated from ideas into sounds. Men talk, says Locke, 'only that they may be understood; which is then only done when, by use or consent, the sound I make by the organs of speech excites in another man's mind who hears it the idea I apply to it in mine when I speak it.' (3.3.3.)

The same symmetry is postulated by Saussure. The only difference between expression and understanding that his speech circuit allows, other than a difference in physiological processing, is that the direction of flow, as it were, between *concept* and *image acoustique* is reversed.

Now in Locke there is a reason for this. It is grounded in Locke's general account of how the mind works, of which understanding what other people say is only one part. But Saussure gives us no general account of how the mind works which could provide a comparable anchorage. So that what was originally a reasoned feature in Locke's account survives as an article of faith in Saussure's.

Secondly, both Locke and Saussure take it that the basic unit on which the understanding operates is a single, indivisible verbal item of some kind: the word in Locke's case and the *signe linguistique* in Saussure's.

Again, in Locke's case there is a reason for this. According to Locke's account, when we hear a word such as *man*, or *horse*, or *sun*, or *water*, or *iron* 'everyone who understands the language frames in his mind a combination of those several simple ideas which he has usually observed, or fancied to exist together under that denomination' (2.23.6). No such doctrine is supplied by Saussure, and the lacuna is once more explained by the fact that, unlike Locke, Saussure was not engaged in the larger enterprise of constructing a general, internally consistent account of how the mind works.

The reason why Locke was particularly interested in what we understand by words such as *man*, *horse*, *sun*, *water* and *iron* has to do with Locke's intellectual role as epistemologist to the founders of the Royal Society (Aarsleff

1982, Harris 1982). Saussure played no comparable role: he was not committed to any particular alliance with contemporary philosophers of science, much less to taking sides in a debate with very considerable religious implications.

The reason why a Lockean model was attractive to Saussure was that by adopting such a model linguistics could guarantee its own disciplinary autonomy. By representing *parole* as the mere implementation of resources provided by *langue*, the study of *langue* independently of *parole* automatically took priority. And it is the same reason, one suspects, that has continued to recommend the model to linguists ever since.

That still leaves unanswered the question of how a seventeenth-century theory of the understanding could plausibly be resuscitated for the purposes of a new twentieth-century science. Part of the answer lies in another nineteenth-century revolution, not in philosophy but in a different field altogether: technology.

The model we are considering is a mechanical model. It represents communication as a succession of phases and items arranged in linear progression along a track or pathway. In short, it envisages the process as a journey or transmission of information from one point in space to another point in space.

There can be no doubt about how powerfully the metalinguistic terminology of everyday English predisposes the layman to accept any transmission model of speech communication (Reddy 1979), and a similar case can be made out in respect of other European languages. But this metalinguistic support alone hardly suffices to explain the particular structure of the model in question.

Ostensibly it is a circuit model. In principle, such models must be distinguished from at least two other types: from rectilinear models on the one hand, and from helical models on the other. Now only helical models (Dance 1967) are formally appropriate to capture the dynamic or developmental aspects of speech communication. Not being a helical model, the speech circuit can make no allowance for the progressive modification of the communication situation through time. But in fact the Saussurean circuit is a circuit in appearance only: it is made up simply of two rectilinear sections joined together. That is to say, it allows for no feedback of any kind, except a verbal reply exactly comparable to the verbal message first transmitted. In a word, the circuit is precisely what the terminology of Saussurean linguistics suggests it is: a static model.

It is, however, a special kind of static model. Somehow or other, what started off as an idea has been converted into a physiological process, which is in turn converted into sound, which is in turn converted back into a different physiological process and back again into an idea. The origin of this type of model is not difficult to discover. It has been universally employed in all the natural sciences to account for observed and measurable correspondences in spatio-temporally connected processes involving continuity between different forms of energy. The prototype, in short, is the concept of energy-conversion.

But why should energy-conversion be a plausible exemplar for explaining communication? To answer this, we need look no further than the major

technological innovations in communication which transformed everyday life in Western industrial society during the course of the nineteenth and early twentieth centuries. They were telegraphy, telephony and broadcasting: all forms of energy-conversion applied to the transmission of verbal messages. It is no coincidence that the well known illustration of two people talking in the *Cours de linguistique générale* shows them schematically linked by what looks suspiciously like telephone wires. Nor is it a coincidence that the term *circuit* which Saussure borrowed in order to coin his vivid metaphor 'speech circuit' (*circuit de la parole*) comes from the technical vocabulary of the electrical engineer.

In short, speech communication is conceived of as a closed, causally determined process in every way analogous to the energy-conversion processes of physics and chemistry. Adopting such a model provided linguistics in advance with a forged *carte d'entrée* to the prestigious palace of modern science.

Finally, we may note the socio-political implications of the way this model conceptualizes the role of the individual in speech. It is a model which relies on the existence of a pre-established code (Harris 1981), and that code belongs to the community as a whole. Thus in speech, the individual merely makes use of a verbal communication system institutionalized collectively, in just the same way as he has access to public transport, the post office or any of the other organized communication services of modern society. Even as a sender of messages, his initiative is curiously limited. The model simply postulates that a message comes into the mind of the sender already pre-programmed, as it were, for public transmission. How this is possible is never explained. But that is an obscurity which lies outside the point of entry to the speech circuit itself. So even in this humble initiative which the individual is allowed as a sender of messages, he is already mysteriously indebted to the community.

In short, we are dealing with a model which assigns to the individual vis-à-vis the language a role which matches exactly the socio-political role assigned to the individual vis-à-vis the institutions of the modern nation-state. In both cases, sovereignty belongs to the collectivity. As a member, the individual can do no more than what the community makes it possible for him to do.

In conclusion, it need hardly be emphasized how heavily this picture of speech communication is indebted to the cultural paradigms of a particular phase in Western civilization. Whether it would appear at all convincing if seen against a totally different cultural background must be doubtful. Its persuasiveness derives essentially from the fact that, at a particular time and place in human history, all the relevant analogues and justifications—metalinguistic, philosophical, technological and political—came together to provide what passes in that context for both a necessary and a sufficient framework for the analysis of speech.

What is instructive about the model is not its explanatory power, for it has virtually none; but its exemplification of how linguistic theory is moulded by the preoccupations and assumptions predominant in the civilization of which

it is a product.

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Content and Expression from Saussure to the Present

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1114 It is Hjelmslev, one of the most interesting of the post-Saussurians, who develops the terms *content* and *expression*. Content is an extension of the Saussurian term *signifié*; expression is an extension of the Saussurian term *signifiant*. Content is therefore meaning, and expression the morpho-syntactic means of conveying that meaning (Hjelmslev 1935:xii). The content systems of a language are the systems of meaningful grammatical contrasts, and the expression systems are the paradigms (i.e. morphology) which present those meaningful contrasts.

Hjelmslev consequently conceives of expression, the conveyor of content, as playing a subordinate role: for him a grammatical system is first and foremost a content system, a "système de valeurs," to put it in Saussurian terms, where each *valeur* draws its meaning from its position in the system and the contrasts that it presents with the other *valeurs* of that system:

"Tout fait linguistique est un fait de valeur et ne peut être défini que par sa valeur." (1935:20)

"La grammaire est la théorie des significations fondamentales ou des valeurs et des systèmes constitués par elles..." (1935:84)

"Une catégorie est définie par la valeur, non par l'expression." (1935:77)

This view, that the structural systems of a language are systems of meaning, is in direct contrast to the viewpoint of the American "structuralists" (who were behaviorists rather than structuralists). These latter sought to find the "structure" of language in the expression systems, in the directly observable morphology (a positivist or behaviorist prejudice which is ultimately unworkable as I shall demonstrate). They consequently tended to abstract or ignore the truly fundamental element, the element of content. Greenberg, for example, is recorded as saying: "By structure is meant here facts about a language as an abstract calculus without reference to meaning" (Hoijer 1954:16), and Hockett at the same conference declared: "...we may have to use semantic evidence in order to find out what the linguistic system of a language is, but... the system does not include the semantics." (Hoijer 1954:152).

Within a generation after the publication of Saussure's *Cours*, therefore, linguistics had gone off on two radically different paths. The work of Hjelmslev (1935) and Jakobson (1936) in the 1930's on case systems as content systems followed in the Saussurian tradition. Saussure had said "La langue est une forme, non une substance" by which he meant that the essential things in a

language are the sets of meaningful relationships, not the substance, the related paradigms, the directly observable morphology. In his analogy of the game of chess the system of the game does not lie in the directly observable chess pieces, the substance. The system, furthermore, is rigorous, is coherent, so much so that if a chess piece is lost it may be replaced with "any conceivable object of a convenient size" (Hjelmslev 1959:28)—an excellent analogy for a suppletive morph, a morph which does not fit the morphological set, but which in no way changes the underlying meaning of the content system. The form *went*, in English, presents the systemic meaning of *go + past* just as the form *talked* presents the similar meaning of *talk + past*. The meaning is regular, systemic, coherent; the morphology is not.

The behaviorists' choice of a radically different path, attempting to find the system in the directly observable morphology, is consequently doomed to failure, since the morphology is only partially systemic, and what system it has is only a reflexion of the content system that it presents. In fact another European, Gustave Guillaume, in his *Leçons de Linguistique*, which were published posthumously in the 1970's draws up two laws: (1) *la loi de cohérence* which says that only a content system will be fully coherent, and (2) *la loi de simple suffisance* which says that an expression system will be coherent only so far as is sufficient to delineate the related content system (Guillaume 1971:140-1). Guillaume says further that an expression system, where it is irregular, is under constant pressure to change, to reflect the regularities of its related content system. (We know this happens from the kind of regularizing analogies that children make). Where the difficulties are too great, he says, other morphological means, always sufficient, will be found to delineate the content system. This is in fact a new and interesting way of expressing "Humboldt's Universal" that "language has a general iconic tendency, whereby semantic sameness is reflected by formal sameness." (Antilla 1972:89).

Attempts at finding a coherent system in the morphology, therefore, regrettably end in fudging, as when Bloch analyses the "structure" of the English preterit *took*; to bring it into line with the morphology of the weak preterit he ignores the ablaut and gives *took* a zero suffix to mark the past tense (Bloch 1947). One cannot but agree with Nida, who commented "... it appears to me as strikingly contradictory to treat overt distinctions as meaningless and covert distinctions as meaningful." (Nida 1948). What *took* and *talked* share in common is a meaning, not a suffix; they have a common content, not a common morphology, and the attempt to create a common morphology is an error of method that stems from a mistaken view of the nature of language.

The Transformationalists broke with the behaviorist tradition, but nevertheless maintained many unexamined behaviorist assumptions. Bach's attempt (1964:24-6) to derive all English plural forms, both regular and irregular, from 12 sets of Chomsky-Halle type ordered phonological rules emulates Bloch's attempt to regularize an irregular morphology, with the same result: the creation of fictions, the refusal to face fact. Why concoct fictions to make a morphology

regular, when it is not? Why not simply recognize that coherence lies in the content-system, that the forms *mice*, *lice*, *geese*, *teeth* have the same kind of plural meaning as do the forms *cats*, *dogs* and *horses*.

The divorcing of syntax from semantics in the transformationist tradition, furthermore, is a perpetuation of the behaviorist opinion on the role of meaning, eliminating it from all consideration of system in language: meaning was originally left out altogether from Chomsky's system, then later added in a black box on the sidelines. The proponents of Generative Semantics rebelled against this view; they perceived that the whole purpose of language usage is to convey a meaning, a message; if the morphosyntax is the *means* of conveying this message, then meaning must be the point of departure of the linguistic process, and hence the foundation of the whole system. But they failed to take into account prior European work on underlying meaning, got hopelessly entangled in confusions over polysemy, reference, and the role of logic, and ultimately foundered on another behaviorist principle: the definition of a language as a set of sentences. This completely untenable definition leads to the attempt to generate sentence meaning, an enterprise that is pragmatically impossible in any systematic way, given that what appears to be the same sentence can mean quite different things in different situations.

The Generative Semanticists, however, helped to establish a trend that has continued: that of taking into account the role that meaning plays in grammatical function. The European tradition stemming from Saussure has also survived in the work of the followers of Jakobson and Hjelmslev, in Form-Content Analysis, in Prague Functionalism and in the Psychomechanics of Guillaume. Gradually moving to join this tradition are the proponents of Dependency Grammar, where it is possible to see the relationship of adjective and noun as one meaning dependent upon another. Another move in this direction is the establishing of quasi-universal hierarchies, whose underlying structure is unquestionably semantic. As an explicit example of this kind of work we may point to the very recent article of Tsunoda (1982) which relates a whole range of different grammatical phenomena to a single hierarchy of meanings. Many of the papers of the Plenary Sessions of this Congress have followed a similar theme: that grammar is institutionalized meaning. The analysis of content is becoming a major interest of linguists and promises to be a profitable area of research in the next decade.

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The Phoenix of Evolution

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To Edward Sapir, changes in morphology and syntax had nothing to do with the sociocultural condition of the speakers: "When it comes to linguistic form, Plato walks with the Macedonian swineherd, Confucius with the head-hunting savage of Assam" (1921:219). As an affirmation of human equality, Sapir's reaction was certainly overdue. Yet there are facts about language adapting to its environment, i.e. about linguistic evolution, that speak for themselves. Moreover, the view of evolution as progress, which was current in Sapir's time, is a thing of the past in today's evolutionary thought, so that the facts can now be addressed without the racist overtones of an earlier age. Neo-evolutionist work is advancing on several fronts.

One relevant area is the study of syntactic differences between writing and speech. It has produced quite a crop of research reports lately, some appearing recently in a collection edited by Deborah Tannen (1982). In that volume, Wallace Chafe points out that written as opposed to spoken English is characterized by a much larger proportion of nominalizations, of participles, and of attributive adjectives. At the same time, Chafe notes that such syntactic devices are entirely absent (in an overt form at least) in Seneca. May it not be the case that languages like English have evolved such devices by adapting to the appearance of writing in their speech community? It seems at least reasonable to investigate this question seriously (Kalmár, forthcoming). Chafe is now ready to answer in the positive (personal communication).

More directly relevant to Sapir's antievolutionary stand are studies of how language adapts to increasing complexity in a society. Discussing some Australian languages, Paul Kay suggested that embedded as opposed to adjoined relative clauses appear only in complex societies (1977:26). In his 1979 book *On understanding grammar*, Givón distinguished between a syntactic and a pragmatic mode in language. The syntactic mode is a later development, developing as societies of intimates give way to mass societies. Similar statements have been made by both well-known and beginning authors. The volume *Sociocultural dimensions of language change*, edited by Ben Blount and Mary Sanches and published in 1977, included among its contributors researchers like Brent Berlin, Ian Hancock, Frances Karttunen, Joan Rubin, and Gillian Sankoff. The most thorough examination of the subject to date has been undertaken in a doctoral dissertation by Revere Perkins, which lists extensive statistical correlations between sociocultural and linguistic features.

However, criticisms of the evolutionary position can and have been raised. I for one am not too happy with the frequent implication that sociocultural

conditions have a direct, deterministic effect on language evolution. It is very likely that linguistic, like biological, adaptation proceeds via the selection of the best designed of several competing features. How exactly this happens is one great potential research topic that may be generated by the neo-evolutionist attitude.

A more serious criticism of the evolutionary position was advanced in Roger Lass' recent book, *On explaining language change* (1980). Lass complains that "If the only criterion of function is implementation, then we could argue that all changes are (analytically) functional: otherwise they wouldn't have occurred" (86). In effect, Lass is saying, "We recognize the fittest only by their survival, not by any independent test."

To deal with this objection, I borrow the reply of the life scientist Stephen J. Gould to a similar attack on the Darwinian theory of evolution. Gould counters that we do have an independent criterion for "fitness": the criterion of design (1977:45). In principle, it should be possible to predict that, for example, an animal with steady body temperature is *a priori* better designed for survival in a period of drastic temperature changes in the environment. It should also be possible to predict that embedding relativization has a better chance to last than adjoining relativization in a society requiring context-independent forms of communication like writing (Kay 1977). In the Canadian Inuit dialects, I know of no example of relativization in (oral) texts published before 1930, but today adjoining relativization is well documented (Creider 1978), and I have found a few examples of embedding relativization in written texts appearing in a national Inuit magazine. (Kalmár, forthcoming). As literary and publishing activity among the Inuit intensifies, the embedding relative clause might start displacing the adjoining. Test cases like this are not lacking; one just has to look for them.

In this lies the real value of the evolutionary analogy: it has the potential for stimulating new types of research. The idea of language adapting to culture and society will not disappear this time without serious investigation.

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A Critique of Recent Histories of Linguistics

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By the mid-1960s, T. S. Kuhn's (1962, ²1970) morphology of a 'scientific revolution' had become a widely-accepted view of the development of science, even outside those fields to which it was meant to apply. Especially in linguistics the Kuhnian philosophy of science, according to which disciplines do not evolve by accumulation of knowledge and refinement of technique but rather progress in quantum leaps, was particularly influential. The social component added by Kuhn to explain changes in scientific world view led, it would seem, to particular interpretations and indeed applications which led Percival (1976) to reject the suggestion that Kuhn's ideas could in any way be applied to the history of linguistics.

Even though Percival's critique of Kuhn's proposals is far from adequate, it is true that Kuhn (e.g., pp. 90, 121) made suggestions that could not have failed to gain attention in certain quarters. Bierwisch (1971) and many others following him (e.g., Smith & Wilson 1979; Newmeyer 1980) have been depicting the advent of transformational-generative grammar as a definite break with the past, a 'revolution' of the field brought about by a single genius who, realizing the inadequacy of previous theories of language, set out to establish an entirely novel approach to the subject in general and providing for a superior framework or 'paradigm' (in the Kuhnian sense) for conducting linguistic research.

Where the actual writing of the history of linguistics is concerned, Chomsky's *Cartesian Linguistics* (1966) has been taken as a model by many of those interested in the development of transformational-generative grammar (TGG) from the mid-1950s to the present. Together with Chomsky's idiosyncratic and even polemical manner of rewriting the history of linguistics, ideological zeal on the part of transformationalists has led to a depiction of the development of 'modern linguistics' which, both by omission and commission, distorts the history of linguistics in North America of the past fifty or so years, ironically enough, even where the development of TGG itself is concerned.

In view of the widespread endorsement that Whig histories—not to say hagiographies—of the kind Newmeyer's recent book exemplifies are receiving by the linguistic public (e.g., McCawley 1980, Napoli 1981), it appears important to redress the history of linguistics in North America during the past fifty or so years. The present paper, therefore, proposes prerequisites for linguistic historiography in general and offers refutations of certain claims reiterated in the transformationalist literature regarding the revolutionary status of TGG as against previous structuralist theory and practice. In addition, it draws attention to factors outside the confines of the discipline itself which have had,

at one time or another, an important impact on the acceptance and the enduring following of TGG.

To begin with, the author holds that, in addition to both distance and impartiality, a certain 'hypothetical sympathy' (Kuhn 1977:149) should prevail on the part of the historian. Indeed, a kind of 'broad positivism' concerned with the reconstruction of "wie es eigentlich gewesen" (Ranke) is advocated. To this end, both the theoretical linguistic framework and the general intellectual climate of a given period needs to be recaptured. In addition, sociological, political, and even economic factors should be considered; scientists like any other human being do not live in a vacuum, and the theories they are proposing usually carry the imprint of their time. And each period is probably influenced by different extra- and intradisciplinary factors that have to be accounted for by the historiographer. For instance, the tremendous increase in membership in the Linguistic Society of America (1960: 1768 members; 1970: 4383 members) can not simply be cited to demonstrate the popularity of linguistics during that period because of the appeal of TGG. Demographic statistics would show that this growth picture is about the same in almost any other field of the period in which universities increased in size, with more money being poured into post-secondary education, and student enrolment tripled. (Conversely, the leveling-off in the number of LSA members and the regular decrease since 1971 would indicate that TGG had lost its original impact, something which may indeed have been the case, but which could not be demonstrated conclusively by those statistics.)

The paper suggests that the funding of university programs during the 1960s and 1970s in general and of those in linguistics in particular had much to do with the importance attached to and the success experienced by TGG. This factor had been thoroughly researched by Newmeyer & Emonds (1971), though in his 1980 book, Newmeyer has chosen to reduce it to a mere footnote. Indeed, the acknowledgements of funding received from the Pentagon through its various agencies are well documented beginning with Chomsky's *Syntactic Structures* (1957) and Lees' review of the same year, and running at least until the late 1960s.

Following the research done by Murray (1980), a number of standard myths of TGG history-writing are demolished, including Chomsky's 'publishing woes' (Murray), his alleged lack of access to linguistic journals (where in fact the editor of *Language*, Bernard Bloch (1907-65), supported Chomsky personally and published the—frequently polemical—work of his associates throughout his life). It also shows the dependence of Chomsky's 'transformation' theory on theories proposed by Harris (1954 and earlier) and Hockett (1954), thus at least weakening the traditional picture of 'novelty', 'creativity', etc. on the part of Chomsky's proposals. Indeed, the neo- or post-Bloomfieldians welcomed *Syntactic Structures* as an outgrowth of their own endeavours, and it was not until the mid-sixties, when Chomsky, Halle, and others attacked the phonological work done by the older generation that the cleavage between the 'Old

Guard' (now labeled 'taxonomists' and 'structuralists'—as if TGG was not structuralist!) and the 'Young Turks' occurred.

The paper shows that there was in fact no revolution in the Kuhnian (or any other socio-political) sense occurring in mid-20th-century American linguistics but that instead Chomsky and his followers had effectively engaged in revolutionary rhetoric, including polemic and confrontation in matters concerning ideology and research practice, and indeed had embarked early (cf. Lees' 1957 review of *Syntactic Structures*) on a collision course (cf. also Newmeyer (1980:50-51), who takes this 'missionary zeal' as a positive device in proselitizing for the new 'faith'). In short, there was (and still is in certain quarters) much talk about 'revolution', although no true scientific revolution did take place.

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[Note added in proof: For a much more detailed study of the subject outlined in the above statement, see now the author's paper, "The Chomskyan 'Revolution' and Its Historiography". *Language & Communication* 3/1983 (in press).]

12-2

The Future for Unit-in-Context: the Tagmeme

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I. THE EARLY SEVENTIES: Four Features of the Tagmeme

What kind of notation would best portray features of the grammatical unit-in-context—the grammatical *tagmeme*? (See early development in Pike 1954, 1955, 1960.) I wanted a notation which would specify (1) the *slot* in larger structures in which such a unit could occur. But the unit itself would be a member (2) of a *class* of units occurring in such a slot. In addition, it must show the relevance—(3) the *role*—which such a class of units plays in the kind of context referred to, and (4) the *cohesion* between such a tagmeme and other tagmemes in the containing texts or in the system which serves as a background to them. An emic construction—a syntagmeme—at any level would be made up of a sequence of such four-component tagmemes. For example, in *the tigers were shot by the hunter* the subject slot is filled by *the tigers*, which manifests a class of noun phrases. The role of that subject, on the other hand, is under-goer (goal) of the shooting. As for cohesion, the plural form of that subject forces on the verb phrase a plural form also.

In 1977 my wife Evelyn and I published our textbook *Grammatical Analysis* showing how this four-cell concept of the tagmeme could be applied to a *discourse as a whole*, by having both slot and role explicitly in view at various levels of the grammatical hierarchy, from *discourse* to *morpheme* class. In addition, we emphasized, in that book, the sharp difference between the structure of the referential *happening* itself—the sequence of events and characters being described—and the grammatical order *telling* of those events.

II. THE EARLY EIGHTIES: Observer Levels, Experimental Syntax, and Field Structure

In a 1981 volume, I have made explicit the fact that there can be *levels* of embedded observer relationships (e.g. when one person talks about the speech of another person, who has been talking about the speech of a third). These different levels of observer relationship, all integrated within a single analytical viewpoint, can be handled in terms of a notation using a four-cell tagmeme.

In another section of that 1981 monograph, I showed that there is a sharp difference between grammar and reference in the analysis of discourse, by taking a simple story, and telling it (for example) backwards. This approach I call *experimental syntax*. By it one sees more clearly what kinds of changes in conjunctions, word order, dependent clauses, or particle signals, are forced upon the writing. This allows one, experimentally, to find many alternatives which

would otherwise require a very large number of texts to find.

At the time of writing this article the University of Nebraska Press is in the process of publishing my *Linguistic Concepts: An Introduction to Tagmemics*. It is aimed at a nonlinguistic audience, and attempts to make available to it a dozen of the more basic tagmemic presuppositions or principles.

III. THE NEXT FIVE YEARS?

We have two further questions. The first: With the extension of tagmemic analysis to detailed four-cell analyses of reference at all hierarchical levels, is it now appropriate to say that we are working towards a systematic understanding of the tagmemic *exegesis of texts*? My answer is yes. The concepts have been shown to apply equally to nonverbal and verbal behavior, so that background experience, belief, history, and physical environment can all be studied within the same framework. And different narrators of the same tale can each be looked at from the point of view of their intent (their own role structure) and their biases (the cohesive features of their own personal referential structure). Both hero and villain find their places as having a role in the culture, with the possibility, in relation to different observers, for an individual being seen as a villain from the point of view of one observer, but as a hero from the point of view of another.

This would seem to suggest that there could be advances in the *theory of translation*. Different cultures have a different set of observers. The different observers represent different cultural backgrounds, and have different expectations. Since form and meaning are not to be found as isomorphic across two languages, one must have a theory of translation which will allow for contexts to modify the meaning of particular words sufficiently to allow for a comparable impact of a translation on the new hearer, meeting the intent of the original speaker to an adequate degree. (The words *sufficiently* and *adequate* leave an indeterminacy not discussed here.)

The second question: If we accept basic tagmemic principles of observer perspective, the nature of an emic unit, hierarchical structure (of phonology, grammar, and reference), and the necessity for context in all analysis, will it be possible in the next few years to start building a new philosophy on them—which I am calling an *integrative philosophy*—which will give us a new perspective on human nature itself? Starting from these principles, *person*, not *logic*, must be the basic metaphysical priority and starting point. *Person would be above logic*. And, with the emic perspective, one would not make an absolute dichotomy between subjective and objective, between person and thing. Rather the perception of things would be modified by the person's emic structuring of the perception of his environment. That is, it would not reject the concept of an "object" as existing; nor would it reject the existence of a "person" as being real. It would accept both. But it would reject the possibility of knowing the "thing-in-itself" apart from an observer or apart from his imagination or memory.

It would be exciting, indeed, if this framework should make it possible to discuss, in a new way, some of the very old but deep theoretical issues of philosophy. It should help us to start with a postulate of person-to-person-in-physical-context. It would provide an emic (participant-observer-oriented) view of reality accompanied by the concrete scientific research technology of tagmemic linguistics. The observer perspective would leave people with choice—genuine choice. And the world would continue to be waiting to be discovered—but discovered in part through a taxonomy created by the observer, but different from the observer.

Observers—linguists or scholars—who choose to have different interests would in fact have differing but related taxonomies or theories. And eventually the theories which survive would be those which contribute to the interests of those and other individuals. For those scholars who may have been a bit uncomfortable with a mechanistic philosophy which did not leave them the stated capacity to be able to choose a theory to decide that one statement about their data was true and another false, this approach might prove to be a relief.

In this next five years I hope to be working on such an integrative philosophy. And I shall try to build it on concepts of chosen perspective, of emic units, of multiple hierarchy, and of integrating contextual requirements and frame of reference.

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The Abstractness Paradox in Hjelmslevian Linguistics

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According to late glossematic theory (Hjelmslev 1954) language has four strata, viz. two more central ones: expression form and content form (which are mutually connected by the sign function) and two more peripheral ones: expression substance and content substance (each of which is related to a form stratum, e.g. by elementary manifestation rules). The criterion for distinguishing two strata is *non-conformity*. (Thus, in the case of French *bon*, it is possible to posit two non-conform representations, one with a nasalized vowel and one, more abstract, with vowel plus nasal consonant.) This seems to imply that the relationship between strata is of a transformational nature (although the more recent technical use of this term in linguistics may render such a characterization somewhat misleading).

There is a difference in abstractness between form and substance. In glossematic expression analysis the outside observer may find it useful to distinguish abstractness in two senses, viz. (i) formal notations chosen for the purpose of establishing generalized rules of combinatorics (sometimes with the result that the notation is highly unnatural, as when Danish postaspirated *p* is rendered as *hb*) and formal categories defined without reference to phonetics (on the feature level, the choice of purely algebraic terms instead of substance-based distinctive features as the ultimate constituents of the formal hierarchy), (ii) abstractness in a morphophonemic sense, i.e. formal notations chosen so as to minimize allomorphy.—As for type (ii) abstractness there is a seeming discrepancy between (late) glossematic theory and the analytic practice of the whole era of Hjelmslevian linguistics, the former favouring a rather autonomous form stratum and the latter favouring morphophonemic considerations. The relative autonomy of each stratum is probably an innovation which may be chiefly due to Uldall (Fischer-Jørgensen 1966, pp. 17-23), which may explain why it is not quite successfully integrated in Hjelmslev's conceptual framework.

According to Hjelmslev (Prolegomena and elsewhere) two kinds of representation of expression form are possible, viz. an ideal (roughly speaking: morphophonemic) notation and an actualized notation (exhibiting what might today be called the output of neutralizations, rather than the underlying figurae). One may find it hard to accept that two such representations can be subsumed under one stratum since they sometimes clearly exhibit non-conformity. (Hjelmslev's handling of the Danish *stød* in terms of latent consonants is a good example of such a discrepancy between ideal and actualized notations.)

A major goal for glossematics was the establishment of a language typology based on strict criteria. Among the components of such a typological framework are: presence or absence of syllables defined by accent (and hence of vowels and consonants defined with reference to such a syllable), systematized inventories of vowels and consonants, etc. In this context the actualized notation seems a much more meaningful choice than the ideal notation since notions such as syllable and accent, or possible versus impossible consonant clusters, are difficult to apply in an intuitively meaningful way to representations which exhibit latent (non-surfacing) segments and, on the other hand, lack various surface segments (e.g. anaptyctic vowels) because the latter are predictable and hence not part of (ideal) expression form. Some of the problems vexing the analyst using a rigid glossematic approach might have vanished if, towards the end of the developmental era of glossematic theory, the potentials of actualized expression notation had been exploited more. Such a development would seem to call for a synthesis of *the notion of actualized notation with the notion of a self-contained form stratum* (whose figurae are set up solely on the basis of an analysis of intrinsic=intrastatal units, not on the basis of an analysis of signs, cf. Hjelmslev 1954).

As for the content plane *both* the abstract properties of syntactical relations and certain aspects of lexical meaning are handled in terms of content form. As with expression form the content form stratum is explicitly distinguished from sign-based structure (*in casu* sign-based syntax and lexicon). This is apparent, for example, from the treatment of the inflectional material characteristic of verbs: in content form the corresponding elements appear as exponents of a larger structure of sentence status, and their morphosyntactic association with the verb stem is fortuitous from this point of view. It is intriguing, then, to find that in actual practice the analysis of content form mostly proceeds in rather close conformity with the overt morphosyntactic structure. It certainly has not been convincingly demonstrated that the specification of content form can be performed without regard to the specific inventory of lexical items of the language under consideration (on the contrary, assertions about content form at the word level seem directly dependent on the specific sign structure). Likewise, the handling of syntactical relations in content form seems closely linked to sign-based morphosyntax in a number of respects. The latter point may be illustrated by Hjelmslev's (1956) treatment of concord in noun phrases. If, say, several members of such a phrase agree in case, one may choose (a) to make a distinction between head and attribute(s) and to have the case form of the head control that of the attribute(s), or (b) one may consider one occurrence of the case element to occur as an exponent of the whole phrase, or (c) one may consider each member of the phrase to be furnished with a case element which is directly controlled by external factors. The last-mentioned analysis is preferred by Hjelmslev who thus emphasizes the "independence" of the attribute. A major argument in favour of this solution is furnished by the observation that the attribute(s) respond to the syntactic demands with the

proper case form even if the head exhibits a syncretism which makes its case form obscure. (To take an example from German, we find the difference between *ein guter Mann* and *einen guten Mann* under the same conditions as the difference between *ein guter Junge* and *einen guten Jungen*, although it is only in the latter case that the head exhibits an overt difference of case.) This argument hinges on the contention that inflectional classes differ in *content form*, viz. by having different case subsystems. But if this is a matter of content form one would expect the case syncretisms to be dominated (triggered) by some content factor. The only candidate for such a factor is the sign-based property of inflectional class membership, which—at least in the just cited German case—has no discernible semantic correlate. Is this content proper?? There would seem to be a case here for arguing in favour of a more “ideal” content form (in which *Mann* has different case exponents in the two examples above, not just a syncretism), and to specify syntactical relationships with reference to such a level of specification.

Interestingly enough, the distinction between “actualized” and “ideal” notation suggested here for the *content form* would entail a reversal of the orientation vis-à-vis central and peripheral strata, as compared to the distinction between actualized and ideal notations of the *expression form*. In the expression plane the actualized notation has more affinity to substance, and the ideal notation more affinity to sign structure; in the content plane the proposed distinction works in the reverse since the actualized notation would exhibit more affinity to sign structure in this case. This strange skewness is not just a matter of mistaken terminology and theoretical prejudice. As has been observed by earlier scholars there is a genuine asymmetry in that expression syncretisms (neutralizations) are reflected as such in expression substance but can be dissolved (disambiguated) by reference to a sign-based expression form, whereas content syncretisms are reflected as such in a sign-based specification of content form or indeed in the overt structure furnished by sign expressions. It seems logical, then, to assume that the latter syncretisms can be dissolved in a less sign-based framework of content specification (possibly in content substance, as would seem intuitively natural to the extent that the content elements in question have obvious semantic correlates).

In this particular sense the sign function is to content form (and hence ultimately to content substance) what expression substance (phonic or graphic or other) is to expression form, i.e., the allegedly central strata of content form and expression form are *not* on a par in the sense that they are both unconditionally more abstract than the allegedly more peripheral strata of content substance and expression substance. It seems likely that this skewness has contributed to making it difficult to grasp exactly what glossematic “form” really means, and to apply statements about glossematic form hierarchies and categories to actual language typology.

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The Future Paradigm of Linguistics

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The development of linguistics has so far proceeded by evolution. The transformational generative grammar may be regarded not as a revolution, but rather as a variant of structuralism. Because of the multitude of different schools modern linguistics resembles the pre-paradigm stage of T. S. Kuhn. At the Conference on Current Approaches to Syntax held 1979 in Milwaukee 14 different linguistic theories were represented, and this did not include all existing ones.¹⁾ In general language has been regarded as something which is more or less outside man's will and which changes according to its own somewhat mystic laws, regarding which there is no consensus among linguists. Linguistics has been diachronic and synchronic, both of which are descriptive in the widest sense, ignoring evaluation. The same is valid for modern linguistic theories which also treat pragmatic, psychological and sociological aspects and for the so-called "new empiricism". This is also valid for the great part of language planning research, which is in general excluded from linguistics. Besides research results and abstract theories, modern linguistic literature includes non-scientific dogmatic views or myths without any empirical foundation ("all languages are equally complex", "one expression is as good as another" etc.). Dell Hymes (1972a:417; 1972b:322) and others have pointed out the absurdity of such statements.

Besides the above views prevalent among linguists there also occurs the view that linguists must also deal with evaluation. The modern point of view goes back to Jespersen (1894). In 1907 (394) Baudouin de Courtenay declared that we have the right and duty to improve language. In 1912 the Hungarian linguist F. Kaiblinger suggested that linguistics must change into creative linguistics and its task must also be to construct new, better forms. In 1914 Jespersen expressed the view that theoretical linguistics should be the means and language improvement the end (Jespersen 1933:103.²⁾ In 1924 the Estonian linguist Johannes Aavik outlined a revolutionary linguistic philosophy: as language is an instrument, man can and must improve his language as he does with other tools. Language improvement must be extensive, free and methodi-

1) According to Robert B. Kaplan "linguistics is a medieval science, dealing in mystery and superstition based on a smattering of theology and a modicum of fact." (In *Linguistic and literary studies in honor of Archibald Hill*, 4, The Hague 1979, 320.) Geoffrey Sampson compares Chomskyan school with astrology and alchemy (*Schools in linguistics*, Stanford 1980, 163-).

2) Cf. Charles A. Ferguson in 1975: "For many years I have been arguing that the findings and approaches of linguistic science should be used—along with expertise from other fields—in the solution of language problems in our society and elsewhere, and that this use constituted one of the reasons for doing linguistic research in the first place." *Linguistic Reporter* 17:43)

cal. We can construct new words and grammatical morphemes by arbitrarily combining phonemes. Implementation of Aavik's language reform proved that there is no limit to what one can deliberately change in the linguistic code and usage. This fact is now acknowledged by several linguists (e.g. Fishman 1977, Rubin 1977). In the 20th-century several linguists have suggested the establishment of a special branch of linguistics for language planning, e.g. A. Peškovskij 1931 (*Actes de second congrès international de linguistes*, 75), Tauli 1938, H. Spang-Hansen 1944, O. S. Achmanova, Ju. A. Bel'čikov, V. V. Veselitskij 1960. Some named this new discipline *practical linguistics*, others *applied linguistics*. In 1968 I outlined a *theory of language planning*. Several linguists have advocated language improvement. Whatmough (1956:238) maintained that language must be adjusted to the present age, for that purpose it must be improved and redesigned. Sauvageot (1960:538) sees the LP problem in a new perspective. He considers the computers. He foresees the time, when the superiority of the machine to language will be extensive and may replace language as a support of thought. A language which best accommodates itself to an harmonic collaboration with the machine will impose itself on all men. Here is the task future linguists must tackle. Sauvageot is sure that the role of linguistics in the history of civilization thus gets an importance that nobody has been able to imagine (cf. also Sauvageot 1959, 1978).

But the present language planning research has failed to influence language planning practice (Fishman 1974, Tauli 1981) because most researchers ignore evaluation. In most developing countries language planning is founded on anachronistic views of purism and conservatism.

A new epoch and 'paradigm' will arise in linguistics when linguists accept the instrumentalistic view on language and Jespersen's view that theoretical linguistics is the means and language planning the end. This means that *language planning theory must be integrated into linguistics*, whereas diachronic and synchronic linguistics must be based on the instrumental point of view on expression. It is interesting that according to the representative of a modern linguistic school, the equational grammar, G. A. Sanders (1980:232), "a linguistic theory must take crucial account of the instrumental functions" of expressions. According to E. A. Moravcsik (1980:17) "a syntactic theory that provides a view of syntax that contributes to the characterization of human language as an instrument, or as a kind of goal directed behavior is better than one that does not achieve this". According to Sauvageot (1969:18) language planning is one of the most important problems of linguistics. If language planning theory is integrated into linguistics the latter becomes an applied science. Cf. in medical science on one side anatomy, physiology, pathology, on the other side therapeutics. By language planning I mean the activity which is also called cultivation or corpus planning, whereas language policy and pedagogical problems are excluded, but the problems of construction of international auxiliary languages is included.

In the new linguistics the sphere of interest and the methodology will be

different from the present. The new linguistics will be more empirical and pragmatic. In the diachronic linguistics one realizes that there are no laws governing the structural change of languages, but only partly mutually opposite tendencies (which have plain natural explanations), that a written and standardized language is not changed in the same way as an unwritten language, and language planning can favour or hinder a spontaneous tendency (as it has done since the beginning of writing). In synchronic linguistics the abstract theories will be replaced by empiric analysis of "surface" structure. Morphology will again obtain an important place in linguistic analysis. The unscientific dogmas of present linguistics will be replaced by empiric research of the complexity and allomorphy and objective evaluation of different structures and expressions (cf. Ronneberger-Sibold's /1980/ evaluation of inflexional, agglutinative and isolating structures from the viewpoint of economy) among others by quantitative methods. The paramount goal of linguistics will be to make the language as efficient as possible a means, proceeding from the basic structure of the respective language. Language planning theory must also use experimental methods and investigate the sociological problems. The present state of language and linguistics does not match the modern culture and technology. It must be considered that it is not language which changes according to its own laws, but it is man who changes and develops his language as his other tools. It would be a curious anachronism indeed if in the epoch of electronic information and communication man lets language, the most important instrument of communication, stagnate at the horse-and-buggy stage.

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From the History of Russian Linguistics

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In the last 25 years historians of the Russian language have discovered in the Soviet archives and libraries a number of valuable linguistic sources of the XVII-XVIII centuries and published them. Previously one could get an idea of some them by the infrequent references in the classic researches by I. V. Yagich, S. K. Boulich, N. K. Grounsky, A. I. Sobolevsky. But the writings proper were hardly known to a wide range of specialists, for they were not in the general use of historians of the Russian language and Russian philology.

The situation is different now, for valuable linguistics writings and sources have come into researchers' general use. They deepen and widen our notion of the history of the Russian literary language in the XVII-XVIII centuries and of the formation of Russian philology.

The corpus of the new linguistic sources can be divided into 3 parts: 1. The writings containing information on historical lexicology of Russian, including dictionaries; 2. Grammars of the Russian language and works on grammar; 3. Rhetorics and works on style and the art of public speaking.

Here in a review of the writings published.

1. The central place among the works is rightly taken by "The Dictionary of the Russian language of the XI-XVII centuries" published by the Soviet Academy of Sciences (the 8 volumes of the Dictionary have come out). It is to contain 60,000 of the most commonly used words of ancient Russian including those pertaining to social and economic realitions, everyday life, agriculture, crafts and arts, and culture. In compiling the Dictionary, many new linguistic sources were heavily drawn upon.¹⁾

The Russian Language Institute of the Soviet Academy of Sciences has published the Russian handwritten newspaper "Vesti-Couranti" of the XVIII century, which was specially put out for Csar Alexei Mikhailovich and his court. The last volume is to come out soon. The publication of the Russian handwritten newspaper is an event of great scientific, cultural and historical importance, since the texts contain various and valuable information on the history of Russian in the XVII century, on the history and etymology of the Russian wordstock, on the history of forming the spelling and pronouncing as well as on the history of Russian culture and its links with Western Europe.²⁾

1. Словарь русского языка XI-XVII в. Вып. 1-8. М., 1975-1981.

2. Вести-курanty 1600-1639 гг. Издание подготовили Н.И. Тарабасова, В.Г. Демьянов, А.И. Сумкина. Под редакцией С.И. Коткова. М., 1972; Вести-курanty 1642-1644 гг. Издание подготовили Н.И. Тарабасова, В.Г. Демьянов, А.И. Сумкина. Под редакцией С.И. Коткова. М., 1980.

Noteworthy is the publication of two so far unknown handwritten dictionaries of the XVIII century, compiled by V. P. Tatischev, geographer and philologist.³⁾

Pieces from a dictionary of Russian compiled by M. V. Lomonosov have also been discovered in the archives.⁴⁾

2. Newly found and published facts on grammars of Russian and grammar writings.

In the Leningrad library there has been found the so far unknown "Grammar of the Russian language" written in Russian and French by a translator, teacher of the Academic gymnasium (grammar school) in Petersburg and lexicographer I. S. Gorlitsky. This is the so-called sixth Russian grammar published for teaching purposes before the "Russian Grammar" by M. V. Lomonosov.⁵⁾

A description of the little known handwritten "Slavonic—Russian Grammar" by I. W. Paouse, a grammarian of the Peter I, has also appeared.⁶⁾

B. A. Ouspensky discovered in the fund of the Soviet Academy of Sciences Library a so far unknown handwritten grammar of the 30-s of the XVIII century and published it. Its author is V. E. Adodurov, grammarian, mathematician, the first Russian adjunct of the Academy of Sciences in Petersburg. B. A. Ouspensky believes that the "Russian Grammar" by Adodurov was printed anonymously in Stockholm, by the Swedish expert in Slavonic studies, M. Gröning.⁷⁾

There has been found and published an archives copy of the Russian text the work on grammar by V. K. Trediakovsky "On the Plural of Full Adjective Names Endings", devoted to the norms of Russian.⁸⁾

In 1981 the Moscow University Press published the "Russian Grammar" by A. A. Barsov which is the fullest description of the Russian grammar of the late XVIII century.⁹⁾

3. Аверьянова А.П. Рукописный лексикон В.Н. Татищева. — В кн.: Ученые записки Ленинградского государственного университета. № 197, Л., 1957; Аверьянова А.П. Рукописный лексикон первой половины XVIII века. Л., 1964.

4. Макеева В.Н. Русская лексикография 40–50-ых годов XVIII столетия и Ломоносов. — В кн.: Ломоносов. Сборник статей и материалов. т. 4, М.–Л., 1960.

5. Вомперский В.П. Неизвестная грамматика русского языка И.С. Горлицкого 1730 года. — Вопросы языкознания, 1969, № 3.

6. Михальчи Д.Е. И.В. Паузе и его славяно-русская грамматика. — Известия Академии наук СССР. Серия литературы и языка. т. 23, М., 1964; Михальчи Д.Е. Листы белой рукописи «Славяно русской грамматики» И.В. Паузе. — В кн.: Вопросы грамматики и словообразования. М., 1968 (Труды Университета дружбы народов им. П. Лумумбы т. XLI, вып. 4).

7. Успенский Б.А. Первая русская грамматика на родном языке. Доломоновский период отечественной русистики. М., 1975.

8. Вомперский В.П. Ненапечатанная статья В.К. Тредиakovского «О множественном прилагательных целых имен окончении». — Научные доклады высшей школы. Филологические науки, 1968, № 5.

9. «Российская грамматика» Антона Алексеевича Барсова, Подготовка текста М.П. Тоболовой. Под редакцией Б.А. Успенского. М., 1981.

Written almost 200 years ago, the Grammar by Barsov was a worked up record of a lecture course in the Russian language read by him at the University. The grammar was a manuscript, therefore unavailable to a wide range of readers, though it was often turned to by F. I. Bouslayev, M. I. Soukhomlinov, V. V. Vinogradov, P. S. Kousnetsov, L. Newman, Y. Clark and others.

The work by Barsov influenced the formation of theoretical studies of the Russian language. Barsov's grammar terms came into stable general use in Russian language studies. It was Barsov who introduced the notion of stem as opposed to root, into the Russian linguistic tradition; he also dealt with differentiating between word-building and form-building.

In Barsov's Grammar there are linguistic ideas in there with the present-day linguistics. He defined the notion of grammatical form, later to be developed by F. F. Fortunatov. In phonetics and word-building Barsov attempted to qualify the characteristics of a language unit through its relations with other units, thus anticipating the structural approach to language, formulated in the works of F. de Saussure and N. S. Troubetsky. Barsov makes of transformational analysis.

The grammar by Barsov is one of the first Russian grammars in Russian, one of the first attempts to codify the norms of literary Russian. It is a most valuable source for the history of literary Russian in the second half of the XVIII century. The stylistic characteristics worked out by Barsov are elaborate and always reliable.

3. Rhetorics and writings on literary style and art of public speaking.

It will be noted that 25 years ago the rich tradition of writing rhetorics in Russian, Ukrain and Bielorusii, so well studied now, was not known in full. Stylistic theories of the Eastern Slavs were known only by the "Rhetoric" of M. V. Lomonosov and some scanty information about the teachers of rhetoric in the Kiev-Mogilyansk and the Moscow Slavonic-Greek-Latin Academies.

Now there has been found thoroughly studied and published the first Russian "Rhetoric" of 1716, by Makariy, the Vologda bishop, which was established to have been related to the Polish rhetorical tradition and to the rhetorical writings by F. Melanchton.¹⁰⁾

Among the studied and published works is the "Rhetoric" of 1699 by a Moscow teacher of rhetoric, M. I. Ousachev.

New names have appeared in the history of the country's science, such as A. Ch. Belobotsky, teacher of rhetoric, philologist, translator of the late XVII-early XVIII centuries, author of two "Rhetorics", one lengthy the other concise, written in the spirit of Lullian philosophy which sets forth a universal method of knowledge; such as Lavrentiy Krstchonovich, teacher of rhetoric, translator,

10. Бабкин Д.С. Русская риторика начала XVII века. — В кн.: Труды отдела древнерусской литературы Института русской литературы Академии наук СССР. Вып. 8, М.-Л., 1951; Вомперский В.П. Стилистическое учение М.В. Ломоносова и теория трех стилей. М., 1970; Die Makariy — Rhetorik. Von R. Lachmann. Rhetorica Slavica. Bd. I. Köln-Wien, 1980 (Slavistische Forschungen. Bd. 27/1).

publisher in the time of Peter I, author of the printed Latin rhetoric of 1698, written to order of Count B. A. Golitsin, one of the rulers of Russia.¹¹

Much has been done to publish the scientific legacy of Feofan Prokopovich, philologist, poet and statesman of the time of Peter I: the lectures on rhetoric read by him as a professor of the Kiev-Mogilyansk Academy have been published, as well as his "Poetica".¹²

The new linguistic sources are of great importance for studies in the history of literary Russian of the XVII-XVIII centuries, of its lexis, morphology, syntax, of the formation of styles system, expositive and representational means of literary speech. They deepen and widen our knowledge of the history of Russian culture, of Russian cultural links with Slavic countries as well as with the countries of Western Europe.

11. Вомперский В.П. Стилистическая теория А.Х. Белобоцкого. — В кн.: Лингвистические аспекты исследования литературно-художественных текстов. Издание Калининского государственного университета, 1979; Вомперский В.П. Неизвестная «Риторика» Лаврентия Крщоновича 1698 года. — В кн.: Стилистика художественной речи. Издание Калининского государственного университета, 1982.

12. Феофан Прокопович. О поэтическом искусстве. — В кн.: Феофан Прокопович. Сочинения. Под редакцией И.П. Еремина. М.-Л., 1961; Про риторичне мистецтво. — В кн.: Феофан Прокопович. Філософські твори. т.І, К., 1979.

The Principles of Pragmatic Word-Semantics

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In the history of word-semantics roughly 3 different main types of theories have been developed: (1) the "realist" theory of semantics conceiving the meaning of a word as being the object or class of objects for which the word stands, (2) the "idealist" theory which has determined meaning as the ideas or images of the relevant properties of classes of objects, and (3) the younger "operational" theory explaining the meaning of a word as its rule of use. Wittgenstein and his followers have refuted the "realist" and "idealist" theories and have shown them to be utterly false for obvious reasons: there are words like *and* or *perhaps* which "stand for" no object or idea at all and words like *freedom* or *virtue* which can be said to have corresponding objects or ideas only by philosophers having a confused conception of ontology, and to say that a word *has* a meaning and that its meaning is *part* of the sign¹⁾ is just a way of hypostatizing mental entities *behind* the thing or *underlying* it.

Ontologically meaning is nothing that would be "associated with" the sign or "attached to" it. Presumably it was this ontologizing manner of speech of semantics that bothered Wittgenstein above all and caused him to introduce his concept of "use" (PI §43). Every sign carries out a typical, conventional effect which every language-participant has learnt to understand correctly in the course of his socialization by what Wittgenstein calls "training" ("Abrichtung", see PI §5 f.), and he has also learnt to react adequately to such symbols. Moreover, he will usually be able to describe (or circumscribe) this effect by means of other signs, but there exists nothing that would be "associated" with or "correspond" to it: only in a dictionary such correlations are established in the form of oppositions of lexical signs and their paraphrases that are verbal themselves and can go proxy for the former, at least in metalinguistic contexts. It is, however, not justified to conclude by the analogy of the oppositions of signs and paraphrases in a dictionary that a "relation of consubstantiality"²⁾ exists between the body and the content of a sign. Whoever conceives of

1) See Ferdinand de Saussure: *Grundfragen der allgemeinen Sprachwissenschaft*. Herausgegeben von Charles Bally und Albert Sechehaye. Unter Mitwirkung von Albert Riedlinger. Übersetzt von Hermann Lommel. Mit neuem Register und einem Nachwort von Peter von Polenz. Berlin 1967, 2. Aufl. p. 76 ff.

2) Cf. Klaus Heger: "Die methodologischen Voraussetzungen von Onomasiologie und lex. grifflicher Gliederung", in: *Zeitschrift für romanische Philologie* 80 (1964), p. 486-516, *ibid.*, p. 489; Klaus Heger: *Temporale Deixis und Vorgangsquantität. ("Aspekt" und "Aktionsart")*, in: *Zeitschrift für romanische Philologie* 83 (1967), p. 512-582, *ibid.*, p. 525; Helmut Henne:

meaning as a relation of "association" between a sign and its content or of "correspondence" between a sign and the facts obviously thinks, as Wittgenstein puts it, "of the meaning as a thing of the same kind as the word, though also different from the word. Here the word, there the meaning. The money, and the cow that you can buy with it." (PI § 120)

And then Wittgenstein adds in brackets: "But contrast: money and its profit."³ Meaning corresponds to the profit, the effect, the informational value of a word that can be paraphrased—and not to the cow. Meaning is nothing ontical, but just something that can be transformed into a semantic paraphrase, i.e. that can be replaced by equivalent substitutes. From the fact that a word has a meaning—as our language tempts us to say—it does not follow that the meaning it "has" is something "really" existent, something that would be *part* of the sign in the sense of its "bilateral" Saussurean conception. Meaning, in the sense of Wittgenstein, is not "a pure intermediary between the propositional sign and the facts" (PI § 94), or between a word and the corresponding objects in the world, but the replaceability of signs by paraphrases that are equivalent to it in their effect:

"The meaning of a word is what is explained by the explanation of the meaning."
I.e.: if you want to understand the use of the word "meaning", look for what are called "explanations of meaning". (PI § 560)

A paraphrase is the closest we can get to meaning. And what do we paraphrase when we are "explaining" the meaning of a word or sentence?—We give a description of its use. Thus, *meaning* shows itself to be but a heuristic category.

The cardinal defect of the operational theory of word-semantics is, however, to overlook that—leaving aside functional (grammatical and communicative) words and particles—a word has an explicable meaning or profit apart from its mere use, that the mere knowledge of the rules of use does not always suffice to enable a speaker to produce and understand words and sentences. The operational theory is, e.g. incapable of describing a speaker's linguistic competence in foreign languages, for he can be able to use a word correctly without knowing exactly what it means, just knowing under which linguistic and situational circumstances it may be uttered correctly. From this it clearly follows that the meaning of a word cannot be identical with its use.

This observation suggests that meaning and use (or function) are in fact diverse, but that they are nevertheless two components of the same thing, namely the signification of signs. *Signification* here is to be understood as what someone has learnt when he knows how to use and understand a word (or other sign) correctly. This question must go beyond the mere question of use.

Semantik und Lexikographie. Untersuchungen zur lexikalischen Kodifikation der deutschen Sprache. Berlin/New York 1972. p. 20.

³) My translation. The word *profit* seems to me to be a much more adequate translation for the German *Nutzen* than Anscombe's *use* which is too weak and less precise.

and its pursuit presents itself as the attempt to conceive a semantic-pragmatic theory on the level of "langue". It is the question of what a speaker normally knows before he actually uses or understands a word he is already familiar with. And—apart from the intensional aspect of word-meaning usually called its "lexical meaning"—what a speaker knows before the actual usage of a word will turn out to be co- and contextual rules and lexical presuppositions⁴⁾.

Let us have a look at an example: the German verb *fällen* has the intensional meaning 'to cause s. th. to move from the vertical to the horizontal'—I must admit that this is not really a satisfactory semantic description—, it implies or presupposes, in addition, that the object in question is a tree (whereas e.g. in Middle High German also soldiers, i.e. human beings, could be felled⁵⁾), and its cotextual rules define its semantic and syntactical environment: it must have an animate and human subject. With regard to the situational context there is no restriction in this case: the verb *fällen* can be used under any circumstances whatsoever to talk about cutting down trees⁶⁾.

In the case of the English adjective *little* we have a very strange cotextual restriction, for we can only use it attributively, as in e.g. *the little boy*, but not predicatively, as in **The boy is very little*.

It may also be of interest here to note that although the German *Leute* and *Personen* and the English *people* and *persons* are intensionally equivalent you would hardly ever read *Drei Leute wurden bei einem Verkehrsunfall verletzt* in a German newspaper whereas *Three people were injured in a car-crash* is perfectly good press English.

Many other words have stylistic or social restrictions: you would not talk about *your father* and *your old man* under the same circumstances, in the same social context. Performative verbs, for example, normally have such contextual rules or restrictions: you can only *order* s.o. to do s. th. if you are of a higher social or institutional rank than your addressee, you can *reprove* s.o. only if you are a person of authority, and there is no way of *baptizing* others or *passing sentence upon* s.o. unless you are a Christian priest in a service or an official judge at court. Moreover, you cannot *sentence* s.o. to death unless capital punishment is introduced by law—except in a totalitarian society.

Here it becomes important to take into account Wittgenstein's concept of the "grammar" of a word. As I shall also include the intensional aspect in my conception of meaning I should, however, prefer to call the meaning of a word on the level of "langue" its "grammatical meaning" which has to be strictly differentiated from its "topical meaning" in actual speech (parole). "Grammatical meaning" has to be differentiated itself into lexical meaning (intensional and presuppositional meaning + evaluative aspect), "depth gram-

4) Cf. Gerd Fritz: *Bedeutungswandel im Deutschen. Neuere Methoden der diachronen Semantik*. Tübingen 1974, p. 10 f.

5) According to my dictionary this can still be the case in English speaking countries.

6) To all this cf. Gerd Fritz: *Bedeutungswandel im Deutschen*, loc. cit., p. 45 ff.

"mar" (contextual rule) and "surface grammar" (cotextual rule)⁷⁾. Whereas the "depth grammar" of a word regulates its adequate usage with regard to the situational context (stylistic level, sociolect, situational type⁸⁾, in short: the possibility of a word's use in different types of situations), "surface grammar" regulates the possible sentence-positions and collocations.

We come to the conclusion that "lexical meaning"—being one branch of "grammatical meaning"—may be subdivided into "connotational meaning" (evaluative aspect), "denotational meaning" (intension) and "presuppositional meaning⁹⁾". From this the following tree diagram may be derived.

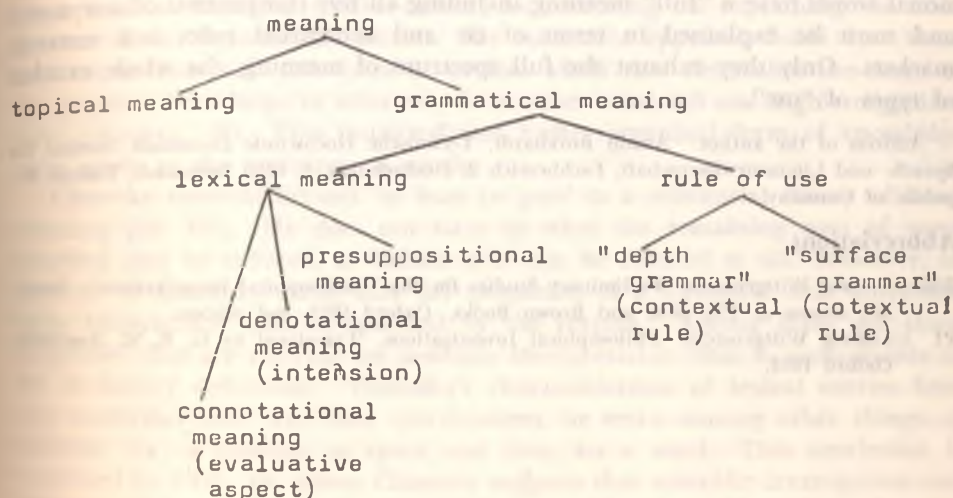


Fig. 1. Components of meaning as the "explanation of a word"

Now, if meaning consists in (not "of") these five components, how can we still adopt Wittgenstein's explanation of meaning as the rule of use of a word which, according to our figure, is only one of the two main general components of meaning? The operational theory may be kept insofar as it can be said: the meaning of a word is how it works or functions in the language, i.e. in the variety of language games. But then we still have to take into account that different lexical units or types of such units can be employed in different ways to the different fields of signification, and semantic theory has to be subdivided and

7) Wittgenstein has introduced the concepts of a "surface grammar" and a "depth grammar" in PI § 664.

8) See also Armin Burkhardt: "Über die Möglichkeit der Frage nach der Bedeutung—und welche Antwort sich darauf ergibt", in: *Zeitschrift für germanistische Linguistik* 7 (1979), p. 129–150, ibid., p. 145; Gerd Fritz: *Bedeutungswandel im Deutschen*, loc. cit., p. 11.

9) It has to be noted that it is very often these three components of lexical meaning that determine the rules of use.

specified¹⁰). I believe that at least three different semantic theories are needed: one for proper names, one for functional words and one for intensional words. As for proper names I understand their meanings—against Wittgenstein's view (cf. PI § 39 ff.)—to be their bearers in the standard case, only in very exceptional cases something like Fregean "sense" can be conventionalized (e.g. in the case of *Aristotle* or *Moses* where the only thing we have is definite descriptions and not their bearers themselves)¹¹). Functional words are words expressing grammatical or communicative functions, therefore these words—very often particles, e.g. the German structuring or colouring (modal) particles—can only be explained by a functional paraphrase describing their "profit". Only intensional words have a "full" meaning including all five components of our model and must be explained in terms of co- and contextual rules and semantic markers. Only they exhaust the full spectrum of meaning, the whole number of types of "use".

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Abbreviations

BBB=Ludwig Wittgenstein: Preliminary Studies for the "Philosophical Investigations". *Generally known as The Blue and Brown Books*. Oxford 1975, 2nd edition.

PI =Ludwig Wittgenstein: *Philosophical Investigations*. Translated by G. E. M. Anscombe. Oxford 1953.

10) Cf. Armin Burkhardt: "Über die Möglichkeit der Frage nach der Bedeutung — und welche Antwort sich darauf ergibt", loc. cit., p. 141 ff.

11) Cf. Armin Burkhardt: "Über die seltsame Notwendigkeit von Freges 'Sinn'-Begriff für Kripkes Theorie der Eigennamen", in: Ursula Wolf (ed.): *Beiträge zur Semantik der singulären Termini*. Frankfurt/Main 1984.

12) Cf. Armin Burkhardt: "Gesprächswörter. Ihre lexikologische Bestimmung und lexikographische Beschreibung", in: Wolfgang Mentrup (ed.): *Konzepte zur Lexikographie. Studien zur Bedeutungserklärung in einsprachigen Wörterbüchern*. Tübingen 1982, p. 138-171.

Chomsky and Wittgenstein on Word Meaning

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In his linguistics Chomsky commits himself to a certain outlook, rationalism, and to certain hypotheses, e.g. the innateness hypothesis. He works with the concept of an idealized speaker-hearer, concentrating on explaining the competence of this hypothetical person. His rationalist approach holds that 'there are innate ideas and principles of various kinds that determine the form of the acquired knowledge in what may be a rather restricted and highly organized way.' (*Aspects*: 48). This restricted and highly organized form of knowledge must apply to the meanings of words.

Chomsky commits himself 'at least in part' to a referential theory of word meaning (SS: 103). He does not state to what the remaining part of word meaning may be reduced, or indeed if it can be reduced at all. However, in *Aspects*: 87 Chomsky states that 'all properties of a formative that are essentially idiosyncratic will be specified in the lexicon.' Among these are those properties 'that are relevant for semantic interpretation (that is, components of the dictionary definition)'. Chomsky's characterization of lexical entries here also establishes that, with four specifications, he seeks—among other things—a semantic 'fix', a constant in space and time, on a word. This conclusion is confirmed in *PKF*: 18, where Chomsky suggests that scientific investigation can determine 'what words really mean'. Indisputably, then, Chomsky seeks constants under specified conditions.

Another remark on the same page has an interesting implication. 'We can easily imagine', says Chomsky 'how an organism initially endowed with conditions on the form and organization of knowledge could construct a specific system of interconnections among concepts... on the basis of scanty evidence.' Possibly the organism would have access to intersubjective data, but Chomsky does not mention this. His view of word meaning therefore allows for, if not demands, a private language. There are strong philosophical arguments against the possibility of private language, some advanced by Wittgenstein.

If the sketch drawn here of Chomsky's view of word meaning is correct, there is no way within it to account for synchronic variation of collocational meaning. Nor are diachronic shift and geographical variation explicable. Finally, espousal of a referential theory commits Chomsky to an essentialism involving meanings as mental objects in the brain (cf. *RR*: 12-14).

Unlike Chomsky, Wittgenstein does not commit himself to theses. He directly examines the ordinary, everyday use of language. His approach to word meaning has an anti-referential, anti-essentialist thrust. Wittgenstein's

other lines of argument are all subsidiary to the view that 'the meaning of the expression consists entirely on how we go on using it.' Before Chomsky espoused it, his semantics of reference had been satirised by Wittgenstein as the 'here the word, there the meaning' view of meaning. 'The money, and the cow that you can buy with it. (But contrast: money, and its use.)' (*PI*: § 120). Wittgenstein consistently denied that words mean anything by themselves, or are mental images or objects: '...if we had to name anything which is the life of the sign, we should have to say that it was its *use*.' (*BB*: 4; cf. also *PI* § 340).

Wittgenstein went on to ask why it is that we cannot clearly define ordinary concepts. His answer: not because we don't know the definitions, but because there is no real definition to them (*BB*: 25). It is this indeterminacy in word meaning that allows for the conclusion that meanings depend on how we go on using expressions (*BB*: 73). Several examples are provided. For instance, it is not to be expected of the word 'to think' that it have a unified employment: rather the opposite. (*Z*. § 112; cf. similar and related themes in *PI* §§ 182, 162-4, 156, 122, p. 224^e, p. 190^e).

The realist assumes uniformity in the meaning attached to a word. But consider applications of words like 'to have'. "'A has a gold tooth" means that the tooth is in A's mouth. This may account for the fact that I am not able to see it. Now the case of his toothache, of which I say that I am not able to feel it because it is in his mouth, is not analogous to the case of the gold tooth. It is the apparent analogy, and again the lack of analogy, between these cases which causes our trouble. And it is this troublesome feature in our grammar that the realist does not notice.' (*BB*: 49; cf. also *PI* p. 224^e).

The lack of 'surveyability' in our use of words is clarified by Wittgenstein with a distinction between 'surface' and 'depth' grammar. On the surface is that part of the use of a word 'that can be taken in by the ear.' But the depth grammar of a word, say 'to mean', cannot be 'surveyed' easily, perhaps not at all (*PI* § 664; cf. also § 122). (Chomsky's attempt in *TTGG* to co-opt Wittgenstein's depth and surface grammar with his own very different 'deep and surface structure' indicates a superficial reading of Wittgenstein, a matter whose consequences for linguistics are worth exploring).

Wittgenstein presents an analogy between a symbolic machine with all its movements predetermined and the realist view of word meaning. A real, actual machine can have its parts modified, its functions totally changed, during the course of its operations. This is analogical to the 'depth grammar' of a word: the realist view sees but the surface grammar. (Cf. *PI* §§ 191-194).

Abbreviations

Chomsky:

- | | | |
|----------------|---|---|
| <i>Aspects</i> | : | Aspects of the theory of syntax, 1965. |
| <i>PKF</i> | : | Problems of knowledge and freedom, 1971. |
| <i>SS</i> | : | Syntactic structures, 1957. |
| <i>RR</i> | : | Rules and representations, 1980. |
| <i>TTGG</i> | : | Topics in the theory of generative grammar, 1966. |

Wittgenstein:

PI

BB

Z.

: Philosophical investigations, 1953.

: The blue and brown books, 1958.

: Zettel, 1967.

Some Problems of Homonymy

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The purpose of our paper is to summarize certain current views of *homonymy*, conceived as a formal identity of lexical items covering the spoken and/or the written substance of language. Homonymy and other lexico-semantic terms make up a single terminological "Bedeutungsfeld" with a considerable history⁽¹⁾. As a common term in Renaissance logic, homonymy was defined by T. Wilson (*Logic*, 1551) as the "doubtfulness of one word, when it signifieth diversely"—a statement not easy to interpret. Whether *tokens* of the same phonological shape are grouped under one or under more lexicological *types*, the procedures we adopt are the same: intuitive judgements are made about the (dis)similarity of meaning, whether we establish a single entry for, say, *HOT* (~ *water-bottle*, ~ *Mexican food*, ~ *music*, ~ *watch*, ~ *number*), or, though it is less often stressed, when establishing several entries for e.g. *PLAIN* (~ *s*, ~ *fabric*, ~ *talk*, ~ *and purl*). The *type-token problem* in lexical semantics cannot be sidestepped by discarding *polysemy* and using *homonymy* as an over-all term. The theoretical distinction between homonymy and polysemy, which has attracted a great deal of attention, is a minor quibble beside the major issue of the identification of *linguistically relevant semantic units* (meanings, homonymic sets).

Bréal defines polysemy as a process of constant multiplication of meanings ("valeurs") in language use⁽²⁾, and Weinreich reveals, in fact, the vulnerable point of Bréal's definition, *infinite polysemy*, in his criticism of KF's semantic theory⁽³⁾. Katz dismisses the alleged possibility that the differentiation of sub-meanings can be infinite: the dictionary is supposed to be the reconstruction of an aspect of the speakers' *finite* semantic competence; on the other hand, Katz claims that Weinreich has failed to make the important distinction between semantically irrelevant referential features and genuine semantic considerations, though, he admits, the line is hard to draw⁽⁴⁾. At present, we have neither a broad theoretical basis, nor practical procedures for the delimitation of relevant semantic units, and we are in need of a *principled conception of homonymy and the related lexico-semantic terms* (polysemy, synonymy, paronymy, antonymy, hyponymy).

Homonymy is said to bear on the communicative efficacy of language, consequently, it is supposed to be an important factor inducing language change. The generalization that homonyms tend to be self-destructive is based on the assumption that linguistic change does not occur irrespective of communication needs. A clear statement of this view was first made by Jakobson⁽⁵⁾ and served

as a counterpoint to the Saussurean concept of sound change, claiming that change has a *teleological/therapeutic nature*. Although it is clear that basic notions of structural lexicology are, in fact, teleological ("functional pull" in discarding/differentiating synonyms, filling lexical gaps), we have no reason to believe that the teleological criterion forms a safer background to the study of lexical structure, than to the study of sound change. Almost certainly, *teleology is not a principle, but a variable* (with values 0/1) to be considered when investigating language change. In a computerized study of homonymy in English⁽⁶⁾, based on the material of five dictionaries, an exhaustive list of homonymic sets of monosyllabic entries was compiled. About half of the 5757 homonymous items was involved in at least one homonymic set. Despite these figures, it is possible to defend the traditional concept of homonymic collision arguing that formal identity does not necessarily involve an excess of mutual interference, if the partners pertain to different word classes or distant varieties of the language, incapable of occurring in identical contexts. On intuitive grounds, however, this explanation accounts for a fragment of homonyms only and *we have no way of proving the teleological factor against actual cases of semantic interference*, both being unknowns in the same equation. Assuming the case to be real, we would be able to analyse diachronic processes, if we knew what relevant semantic properties compete in a clash. A promising attempt to establish exact criteria for identifying and rating homonymic clash was made by Japanese linguists using a corpus of 48,000 words⁽⁷⁾.

Another problem is that *universal rules of homonymic interference can hardly be established*, as the communicative significance of homonymy is probably not independent of the phonotactic freedom and the writing system of the language in question, e.g. homonymous forms are more likely to occur in Mandarin Chinese where the total of morphs only amounts to about 1400, or in Japanese where Chinese readings of kanji (kango), forced into the Japanese phonological system, resulted in an enormous number of homophones, cp. occurrences of *shōhō* (31 entries), *shōko* (46) or *kōshō* (85) according to Nihon Kokugo Daijiten.

In a recent study Malkiel⁽⁸⁾ argues that speech communities adopt four characteristic patterns in the event of a homophonic clash: *maintenance, elimination, lexical split* (semantic polarization) and *merger*. The classification has neither explanatory nor predictive power, neglecting answers to questions of *when, which and why*. And again, it is extremely difficult to interpret concepts like *semantic polarization* or *merging*, the latter "producing a third word, usually of extended semantic ambit". How can the statement that the senses of the English adjectives *light* have become contiguous over a shared semantic "ambit" be (dis)proved? A par excellence example of contextual interference ought to be polysemy⁽⁹⁾, or is there a degree of semantic similarity beyond which homonymic clash ceases to exist? How can a parallel between the semantic differentiation of synonyms (e.g. *lift*: *heave*) be shown? The descendants of Latin *amāre* 'love' and *aestimāre* 'value, esteem' show an oft-quoted

morpho-syntactic convergence in French, usually explained as a case of semantic attraction. However, semantic interference as following homonymic coincidence will not necessarily be thought of, if the instances of formally conditioned convergence (in defiance of regular sound change) are considered—as described by Coates⁽¹⁰⁾. Coates proved that three lexical items of nearly identical phonetic shape, but rather dissimilar in meaning ('beam, pole'/'adze'/'thistle') developed identical forms over two or three of these senses: German *Deichsel* (var. *Dechsel* 'adze'), Dutch and Afrikaans *dissel*, Frisian *tiksel*, Swedish *tistel*. We have no way of judging the significance of semantic (vs. phonetic) factors in any of the cases mentioned.

We may also challenge the view that similarity of meaning might be a decisive factor. In a dialect study by Kolb⁽¹¹⁾ the case of English *sun* and *son* is reconsidered. Although these items have been homonymous in large parts of England, certain dialects have eliminated this "irksome" homonymy, either by lexical replacement or by a secondary differentiation in vowel or initial consonant. Furthermore, some English dialects have retained the OE verb *erian* > *ear* 'plough', while the obsolescence of the verb in standard English is usually explained by a homonymic clash (with *ear* 'ripen' ← *ear* 'of a corn', and, probably, *ears*, *hear*). What is responsible for these differences? What is responsible for "delayed timing", i.e. the maintenance of homonymy over a long period, as in the case of the ME homonymic collision of OE *lettan* 'hinder' and *lāetan* 'allow, permit'? The supposedly extinct sense occurs in Hamlet's warning to those who try to prevent him from following the ghost: "*Unhand me gentlemen . . . I'll make a ghost of him that lets me*" (1. 4. 84). The earlier loss of one member in each of the following pairs would suggest that comparative delay may be connected with the antonymy of the partners, cp. OE *helan*/*hāelan* 'cover, hide'/'heal, cure' or *herian*/*hēran* 'praise'/'hear' → MoE *heal*, *hear*. Another possible inference that may be drawn is that *timing* as *putative evidence for homonymic loss is highly unreliable and uncertain*.

However, we may rightly assume the existence of homonymic clash, if the area of lexical replacement is limited by an isogloss marking out the boundary of the region where homonymic forms have resulted from sound change. Out of the stock examples of *dialectological evidence*: only those dialects of English have retained *quean* 'whore', where it is distinct from *queen*, i.e. where the {/ɛ:/-/e:/} → /i:/ merger did not work; or the "Gascony case", i.e. the descendants of Latin *gallus* 'cock, rooster' were replaced by obvious makeshifts in areas where sound change would have rendered them homonymous with the reflexes of *cattus* 'cat'⁽¹²⁾.

Nevertheless, our concern has not been to (dis)prove the existence of the phenomenon *homonymic clash*, just to show that even the best proven instances of homonymic clash are completely obscure and, in themselves, self-referent—just like a patient's symptoms if we do not know the illness they indicate—, and so they remain, until we have a clearer understanding of the interaction of semantic properties on the synchronic plane.

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A Lexical Entry for Function Words: The Italian Preposition *da*

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1. The semantic part of a lexical entry consists of a set of senses. These senses are not completely isolated from each other; generally, there is some relation between two different senses. Since I will talk about the Italian preposition *da*, I give the following example: *da* has a spatial sense in:

(1) *Vengo da Milano* 'I come from Milano'
and a temporal sense in:

(2) *Aspetto da ieri* 'I am waiting since yesterday'
but it is not an accident that the same preposition expresses just these two senses; rather, there exists a semantic rule:

(3) [+ spatial] ~ [+ temporal]

This rule is a universal, i.e. it belongs to the cognitive equipment of the human mind.

In Jackendoff 1975 semantic rules are discussed mainly in relation with derivational morphology. They have the status of redundancy rules between lexical items, similarly to the rules which link the form of the items. But in his example *smoke*, he studies the same type of semantic rules within a single lexical item. I will follow him in this respect and discuss not "separate but related" lexical entries, but "separate but related" senses of one entry. As is well known, redundancy rules are not obligatory: languages may differ in the use they make of them. This is clear from the English gloss of my examples (1) and (2), which have *from* and *since* respectively, corresponding to two different senses of *da*. Now, not all senses of one item are linked in the same way. We have the following possibilities:

(1) Senses are linked by semantic rules of a general, language independent nature to some basic meaning, i.e. a universal cognitive primitive. An example is the source meaning of *da*.

(2) Senses have undergone an autonomous development by a process of semantic drift, similar to the process of lexicalization in morphology. E.g. the type *pendere da* 'to hang on', which develops the extent type of the source meaning in a vertical way, while many other languages choose other ways of expressing the same content.

(3) Senses are linked to a non-existent basic sense. An example is the place and direction sense of *vado da una persona* 'I go to a person' and *vado da una parte* 'I go to one side'. Linking these two senses, as seems indicated, involves a disjunctive pair of conditions: the NP must indicate either a person or the

notion of *side*. The disjunction is an indication of a missing basic meaning, obtainable by eliminating the conditions: this basic meaning would be Proximity, comparable with one meaning of English *by*, which by the way existed in older stages of the language (see De Felice 1963)

(4) Senses are completely unrelated: then we have pure homonymy. But cases (3) and (4) are sometimes hardly distinguishable; the choice depends on decisions on how far semantic drift can go without obscuring the basic meaning.

For *da*, I would like to distinguish at least six different meanings: Source, Proximity, Passive Agency, Characterization, Comparison and Destination, where Passive Agency and Characterization may or may not be linked to Source. Source is the only full-fledged set of meanings; the other sets are limited and can be seen as intrusions upon the domain of some other basic meaning.

In the remainder of this talk I will discuss the source senses and give one example of a non-source sense.

2. For the representation of Source, I follow Jackendoff 1976, who assumes a basic predicate $GO(x, y, z)$, which formalizes the movement (GO) of an object (x , called the Theme) from a location (y , or the Source) to another location (z , or the Goal). The most concrete kind of movement indicated by GO is that of a physical object from a spatial location to another spatial location: this is called the positional mode of movement (GO_{Posit} , where Posit is a feature ascribed to the movement predicate). In order to be able to state most of the semantic relations which can be derived from spatial source, we need the following parameters:

(1) The dimension of the location, which distinguishes between

(4) Il treno parte da Milano 'The train leaves Milano'

(5) Il treno esce dalla stazione 'The train leaves the station'

This distinction opposes a non-dimensional source (a point) to a dimensional source (i.e. something that has an inside and an outside). The Italian preposition *da* ignores this distinction, which, however, opposes e.g. English *out of* to *from*.

(2) The distinction between movement and extent, which accounts for

(6) Siamo a 10 km dalla meta 'we are 10 km from the goal'

Extent is the result of a previous or imaginary movement. Jackendoff 1976 treats extent as a 'hypermode' of location.

(3) The distinction between concrete and abstract moving objects. Instances of the latter are voices, looks, smells, etc. They are viewed in the same way as concrete objects which move along a path. The path is concrete in these cases.

(4) The distinction between concrete and abstract locations. Possession is an instance of an abstract location, which in Jackendoff's model has a privileged status as a separate mode of location. There are, however, other abstract locations, such as control or power. I used control in my paper on the Italian verb *lasciare* (De Boer 1981) for the sense of 'let go', where we have a movement from WITHIN CONTROL to OUT OF CONTROL. We need something

similar for combinations such as *liberare da* 'to free from'.

The combination of abstract objects and abstract locations accounts for cases of movement of knowledge (e.g. *imparare da* 'to learn from'). The distinction between space and time is another case of concrete versus abstract location. (5) The distinction between concrete locations and events considered as locations. This distinction explains a case like

(7) *Mi astengo dal fumare* 'I refrain from smoking'
which can be construed as 'I keep away from (I smoke)'

(6) The configuration of the path. Not all movements have a path, the most celebrated case being Galilei's dictum:

(8) *Eppur si muove* 'And yet it moves'

Similarly the path can be absent in the type of movement expressed by verbs of detaching or separating, which can be seen as a movement from *x* to not-*x* without intermediate stages. When a path is present, it can be defined by one location, as in:

(9) *passare dalla porta principale* 'to pass through the main door'

What is interesting in this case, is that Italian uses the 'source' preposition by a semantic rule which consists of ignoring the feature [+starting point], normally associated with [+path] in the source meaning. Thus we have an example of a new sense, which intrudes upon a notion more often expressed by prepositions of the *through* type.

The path can also be defined by two locations, which coincide with the source and goal of the movement.

Finally it can be defined by an ordered range of (mostly abstract) locations, as in counting or in the passage from one rank to another e.g. in military life. The source and goal coincide then with determinate locations within the range of possibilities, which is given independently, e.g. by the meaning of the lexical items concerned. I mean by this that in

(10) *essere promosso da tenente a capitano* 'to be promoted from lieutenant to captain'

the path consists of the whole system of ranks, but the source and goal of the movement only of the ranks mentioned explicitly. If the path is temporal, this case accounts also for examples like *cominciare da NP* 'to begin with NP'

3. The senses which are not explained by the parameters discussed up to now, are those which are usually labeled as 'origin' or 'cause'. Some examples are:

(11) *Il bambino è nato da genitori ricchi* 'The child is born from rich parents'

(12) *I suoi mali vengono dal fegato* 'His ailments come from the liver'

I propose for the analysis to introduce another kind of abstract movement, called the causative mode, which expresses the transition of objects from non-existence to existence. An example would be

(13) GO_{Cause} (AILMENTS, LIVER, *z*)

where the unspecified goal is to be interpreted deictically as 'this world' or

something similar.

This leads me to some remarks on the notion Cause. I think we should sharply distinguish between at least three different notions:

(1) *Agent*, as in

(14) Paolo viaggia da Milano a Roma 'Paolo travels from Milano to Rome'
The Agent is the person which acts deliberately.

(2) *Causative Agent*, as in

(15) Paolo manda Giorgio a Roma 'Paolo sends Giorgio to Rome'

Causative agency is an argument of a causative predicate, as in

(16) CAUSE (PAOLO, GO (GIORGIO, γ , ROMA)

The distinction between Agency and Causative Agency liberates us from absurdities as Paolo causing his own journey by doing it deliberately.

(3) The causative made of location, as treated before.

4. One of the most frequent uses of *da* is Passive Agency. It accounts for approximately one third of the cases in my corpus. We should note, however, that it is not a semantic, but a syntactic category. It has the same range of semantic interpretations as a subject. Thus the term 'agency' is a misnomer; it should be replaced by a syntactic notion like demoted subject. Although this category is fairly common in the world's languages, there does not seem to be any specialized preposition for it in any language. This means that it is not among the basic relations such as Source, Goal, Comitative, etc., but it is a typically derived relation. The lexical means which indicates the demoted subject is different for each language, but there are some preferred candidates, among which the source preposition and the path preposition. This means that for Italian there is a semantic rule which links the semantic notion of source to the syntactic notion of demoted subject of the verb.

5. Among the non-source senses, I mention only one, namely the type

(17) Il ragazzo dai capelli neri 'The black-haired boy'

It expresses the notion of characterization, that is identification of an object by means of a particular sign. The same notion, however, can be expressed by *con*. I propose to consider the characterization sense of *da* as a highly marked use, intruding upon the domain of *con*, with the following arguments:

(1) It is always possible to substitute *con* for *da* without rendering the sentence anomalous.

(2) The syntactic use is very restricted: the only possible frame is

(18) [___ def. art. N A]

It is not possible to omit the article (unlike what happens in its Spanish counterpart), nor to omit the adjective. Adding a locative phrase renders the sentence unacceptable, cf.

(19) *Il ragazzo dal cappello nero in testa 'The boy with the black hat on his head'

With *con* all these things become possible.

(3) The choice of the preposition is language specific, cf. French *à*, Spanish and Portuguese *de*. Moreover, only these four languages have the construction; Germanic languages and Rumanian have only the equivalent of *con*.

This sense can possibly be linked with the causative source in

(20) *Lo riconosco dalla voce* 'I recognize him by his voice' in which *la voce* is also a sign, but the semantic rule would be very marginal and have as its input a sense which in itself is already marginal within the set of source senses, so that it is not very clear what is to be gained by linking it to the source notion, instead of setting up a homonymous meaning.

In the 15 minutes allotted to me I have only been able to give a very sketchy outline of the kind of research I am conducting with respect to the description of the meaning of prepositions. I hope nevertheless that I have been able to show some differences with other approaches. First of all, contrarily to structuralist approaches, I take seriously the set of senses we find in dictionaries, but I concentrate on the semantic rules which link the single senses, while generally the links are taken for granted. Secondly, I have no a priori need for a general meaning, or *Gesammtbedeutung*, covering all senses; on the contrary, if the link is not clear, I prefer assuming more meanings. Thirdly, not all the senses are on the same level: there is a hierarchy among them, dependent on the number of semantic rules involved, and there is a distinction between senses integrated in a basic meaning and marginal or intrusive senses. Finally, since I insist on the language independent nature of the more general basic meanings and semantic rules, I hope to offer a basis for comparison among languages.

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“Onomatopoeic Expressions in Japanese and English”

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The purpose of the present paper is to make a sort of parallel classification of onomatopoeic expressions in Japanese and English in terms of the four degrees of lexicalization, and at the same time to state the co-occurrence of the Japanese particles, *to*, *ni*, *da*, and *no*, with regard to onomatopoeias.

I. Four degrees of lexicalization

(a) Nonce words (Degree 1)

Onomatopoeic nonce words are created for an occasional use and it is usual that they are not found in the dictionary. Each of them describes a unique situation, e.g., in English, *boozle*, *chu-chu*, *zzzm*, etc. and in Japanese, *bakyuun*, *gachoon*, *mowa-mowa*, *run-run*, etc. Such expressions are widely spread throughout our daily conversations, comic strips, and advertisement. Very few of them, however, survive to establish themselves as regular linguistic forms.

(b) Onomatopoeic words proper (Degree 2)

These words constitute onomatopoeic expressions proper, which describe the phenomena ranging from physical sounds to figurative manners of actions or mental states. They are usually found in the dictionary, but in almost all cases they lack grammatical inflections. The Japanese language abounds in this type of words.

To cite only a few from animal cries, we find in English *bow-wow*, *ban*, and *cock-a-doodle-doo* and in Japanese *wan-wan*, *mee*, and *koke-ko'koo* as their counterparts. As far as these expressions are concerned, in Japanese the juxtaposition of an onomatopoeic word, the quotative particle *to*, and a verb is the commonest pattern, e.g., 'Inu ga *wan-wan to naku*,' 'Hitsuji ga *mee to naku*,' and 'Niwatori ga *koke-ko'koo to naku*.' In the English language, on the other hand, more lexicalized expressions, e.g., 'A dog *barks*,' 'A sheep *bleats*,' and 'A cock *crows*,' are considered central, and their onomatopoeic versions tend to be rather peripheral. Notice in English a specific animal has its own verb to show its cry, while in Japanese it is onomatopoeic words that are directly associated with respective animals, with the neutral verb *naku* attached to them.

(c) Partially and fully lexicalized words (Degrees 3 & 4)

The English language has such group of words that are etymologically onomatopoeic and are more or less lexicalized enough to be inflected. This group further falls into two subgroups: the one that contains those words fully lexicalized, e.g., *chatter*, *scream*, *whistle*, etc. (Degree 4); and the other that contains those words which still retain something of the onomatopoeic tones,

e.g., *bang*, *crash*, *plop*, etc. (Degree 3). To be more precise, the latter group could be ranked between degree 2 and degree 3, because this group of words can be used in quoted forms, such as "*Bang!*", "*Crash!*", and "*Plop!*"

A similar distinction can be made in Japanese. In the case of degree 3 words, the combination of onomatopoeic elements and the verb *-suru* is so tight that normally the quotative particle *to* is not inserted between them, and the *-suru* part is inflected as a verb. Examples are: *bi'kuri-suru* (*be surprised*), *ga'kari-suru* (*be disappointed*), *u'kari-suru* (*be careless*), etc. The degree 4 group consists of such words as *odoroku* (*be surprised*), *sawagu* (*make a noise*), *sosogu* (*pour*), etc. They are fully lexicalized as a verb; therefore, as is often the case with the onomatopoeias at this level, few of the native speakers realize that they are etymologically sound imitative.

II. Occurrence and non-occurrence of the Japanese particle *to*

(a) Occurrence of *to*

The principal function of the Japanese particle *to* is to indicate that its preceding part is a quotation. So *to* goes not only with the quoted speech in direct narration but also with onomatopoeias, the combined forms functioning as adverbials. As to the latter, it is pointed out that some occurrences of *to* are obligatory and others are optional, of which we can tentatively set up the following rules.

- i) *To* is obligatory after the words ending with /Q/, /N/ (Japanese moraic phonemes, represented here phonetically as ['], which occurs between vowels and consonants as [V'C], and is identified with the following consonant, in this case [t]; and [n.], respectively), or after long vowels.

Examples: *basa'*, *do'*, *gyu'*, *kiri'*, *mu'*, *pa'*, *so'*, etc. + *to*

ban., *den.*, *gan.*, *kichin.*, *pon.*, *ton.*, *tsun.*, etc. + *to*

gaa, *gee*, *gii*, *goo*, *guu*, *gyaa*, *gyuu*, etc. + *to*

To is also obligatory when /Q/ occurs between identical syllables, as in:

ka'ka, *sa'sa*, *se'se*, *pa'pa*, etc. + *to*

- ii) If the words ending with *-ri* do not take the reduplicated form nor contain either /Q/ or /N/, *to* is obligatory as in: *darari to tareru* (*dangle*), *katari to hareru* (*clear up*), *porori to koboreru* (*trickle down*), etc.
- iii) *To* is optional either when the words follow a certain phonological pattern which is typically identified as onomatopoeic, such as reduplication, e.g. *chibiri-chibiri (to) nomu*, *kusu-kusu (to) warau*, etc.; or when the words contain /Q/ or /N/ at the non-final position and also end with *-ri*. For example, *po'kiri (to) oreru*, *bon.yari (to) mieru*, etc.

Thus we have the entire series of words with both obligatory and optional occurrences of *to*.

For example: *bata' to*, *batan. to*, *batari to*, *ba'tari (to)*, *bata-bata (to) + taoreru*

(*fall with a thud*, or *fall one after another*)

putsu to*, *putsun. to*, *putsuri to*, *pu'tsuri (to)*, *putsu-putsu*

(to) + *kireru* (snap off, or be cut into pieces)

In spite of the slight differences, stylistic or others, which might exist in the series, we are inclined to classify all these into degree 2 words, regarding the forms with *to* as more general, therefore unmarked, ones. This position might further be supported by the obligatory occurrence of *to*, when we operate the post-posing transformation¹⁾ to the words with optional *to*. Compare:

- 1 a. Kare wa *chibiri-chibiri* (to) sake o nomu. (He enjoys drinking sake little by little.)
- b. Kare wa sake o nomu *chibiri-chibiri to*.
- 2 a. Kanojo wa *kusu-kusu* (to) wara'ta. (She giggled.)
- b. Kanojo wa wara'ta *kusu-kusu to*.
- 3 a. Kinoeda ga *po'kiri* (to) oreta. (The branch of a tree broke with a snap.)
- b. Kinoeda ga oreta *po'kiri to*.
- 4 a. Fune ga *bon.yari* (to) mieru. (The ship is seen dimly.)
- b. Fune ga mieru *bon.yari to*.

(b) Non-occurrence of *to*

As stated earlier, the Japanese language has a special type of combination of onomatopoeic elements and the verb *-suru*, e.g., *bi'kuri-suru*, *ga'kari-suru*, *u'kari-suru*, etc. The cohesion in these words is so tight that the construction cannot be separated by the particle *to*. The post-posing transformation, therefore, generates ungrammatical sentences.

- 5 a. Watashi wa *bi'kuri-shita*. (I was surprised.)
- b. *Watashi wa shita *bi'kuri to*.
- 6 a. Kare wa *ga'kari-shiteiru*. (He is disappointed.)
- b. *Kare wa shiteiru *ga'kari to*.
- 7 a. Anata wa *u'kari-shiteita*. (You were careless.)
- b. *Anata wa shiteita *u'kari to*.

So far as this group of degree 3 words is concerned, the onomatopoeias with optional *to* (cf. II-a-iii) are combined with *-suru*. The combination of onomatopoeias with obligatory *to* (cf. II-a-i & ii) is also possible. Examples:

- 8 a. Watashi wa *biku'-to-shita*. (I was startled.)
- 9 a. Kanojo wa *tsun.-to-shiteiru*. (She is proud looking.)
- 10 a. Seikaku ga *karari-to-shiteiru*. (He is straightforward.)

Note that the particle *to* is obligatory in the above *-suru* forms according to the rules stated in II-a-i & ii, yet the cohesion, being as much tight as the case with optional *to*, the post-posing generates ungrammatical sentences.²⁾

1) Actually, not only the post-posing transformation but also the pre-posing transformation are applicable to these sentences to fortify our argument. Yet in some of the example sentences, especially in 8 and 9, the application of the pre-posing transformation yields somewhat different consequences. The phenomena, we presume, might be explained from another point of view, such as the problems of topicalization and subjectivization in Japanese. This, however, would require our further elaboration in connection with the positional shift of onomatopoeias.

2) The *-suru* forms here function as [+stative] verbs. However, if we use onomatopoeic

- 8 b. *Watashi wa shita *biku'-to*.
- 9 b. *Kanojo wa shiteiru *tsun.-to*.
- 10 b. *Seikaku ga shiteiru *karari-to*.

III. The alternation of the Japanese particles, *to*, *ni*, *da*, and *no*

Unlike the quotative particle *to*, onomatopoeic words with the particle *ni* indicate the results of actions or mental states presented by the verbs which they modify. Compare:

- 11 a. Byoonin ga *fura-fura (to) aruku*. (The patient is walking *unsteadily*.)
b. Senshu ga *fura-fura ni tsukareru*. (The player is *exhausted*.)
- 12 a. Kazaguruma ga *kara-kara (to) mawaru*. (A pinwheel is *whirling* in the wind.)
b. Nodo ga *kara-kara ni kawaku*. (I am *very thirsty*.)
- 13 a. Pisutoru o *pan.-pan. (to) utsu*. (He shoots a pistol *with a bang*.)
b. Kaban ga *pan.-pan. ni fukureru*. (the bag is *filled* with things.)

Here the particle *ni* is obligatory even with the reduplicated forms. This kind of *ni* can, in turn, be replaced by the predicative *da* (or *desu*, *dearu*) when the onomatopoeias and the verbs are permuted, and *te* (a connective indicating the cause-effect relation) is inserted between them, as in the following sentences.

- 11 c. Senshu ga *tsukare te fura-fura da*.
- 12 c. Nodo ga *kawai te kara-kara da*.
- 13 c. Kaban ga *fukure te pan.-pan. da*.

These *da* can then be replaced by another particle *no*, with the function of adjectives modifying the nouns that come directly after them.

- 11 d. (*tsukare te*) *fura-fura no senshu*
- 12 d. (*kawai te*) *kara-kara no nodo*
- 13 d. (*fukure te*) *pan.-pan. no kaban*

The pitch patterns in these constructions are normally high-low with *to* forms, and low-high with the rest.

The present research is by no means complete and far from being exhaustive. There are a number of fuzzy instances, and exceptions are not difficult to find.

elements with ordinary verbs of action, then *to* is obligatory in the final positions. onomatopoeias in this construction belonging to degree 2. Compare the following pairs:

- 1 a. Kare wa *kose-kose-shiteiru*. (He is *fussy* about trifles.)
b. Kare wa *shiteiru kose-kose to*.
- 2 a. Kare wa *kose-kose hataraku*. (He works *restlessly*.)
b. Kare wa *hataraku kose-kose to*.
- 3 a. Kanojo wa *a'sari-shiteiru*. (She is *frank*.)
b. *Kanojo wa *shiteiru a'sari to*.
- 4 a. Kanojo wa *a'sari hakujoo shita*. (She confessed *frankly*.)
b. Kanojo wa *hakujoo shita a'sari to*.
- 5 a. Kanojo wa *kichin.-to-shiteiru*. (She is *neat*.)
b. *Kanojo wa *shiteiru kichin. to*.
- 6 a. Kanojo wa *kichin. to heya o katazuketa*. (She put the room in *perfect order*.)
b. Kanojo wa *heya o katazuketa kichin. to*.

But we hope we have been fairly successful in illustrating a common framework in which both Japanese and English onomatopoeias can be put in contrast.

Another point we should like to make here is that in Japanese the onomatopoeic expressions are not peripheral but constitute an essential portion of the language. In general, Japanese people do not regard it as childish, uneducated or informal, to express their inner or outer world in onomatopoeic terms. This is partly proved by the fact that, in degree 4 group, the juxtaposition with degree 2 words occurs quite naturally in Japanese³⁾, e.g., *ha'to odoroku* (be taken aback), *gaya-gaya (to) sawagu* (make a racket or chatter noisily), *ta'puri (to) sosogu* (pour amply), etc., whereas in English such redundant expressions as *The dog barks bow-bow and *The sheep bleats baa, are not permissible.

IV. Further problems

Among the tasks which are left for us to perform, the most basic is the one to sort out onomatopoeic expressions both in Japanese and English according to the four degrees of lexicalization established in this paper, and to see how valid our hypothesis might be, though fairly wide spectrum of variation could be expected among native speakers. The rearrangement of onomatopoeias, especially in Japanese, in terms of selectional features, is another difficult but worthwhile subject. Here again, the problem of systematic treatment of the semantic features and their overlap in each onomatopoeia might remain tough with us.

I, however, should like to conclude my presentation by referring to a paper written by a Korean linguist, Kong-On Kim. In his paper entitled "Sound symbolism in Korean," the author asserts, quite contrary to the traditionally accepted view, that "/a/ is associated with smallness and /i/ with largeness." His argument is tenable so far as the onomatopoeic data presented are concerned, and the native check with three Korean informants, one in Hawaii and two in the campus of the University of Kobe, proved in the affirmative. But strange enough, the results of the questionnaire on sound symbolism show that the same Korean informants give a unanimous response which accords with the traditional view⁴⁾. This contradictory reaction is unexplainable unless we set up

3) The juxtaposition of the degree 2 and 3 words is also possible in Japanese. For example: *cho'piri (to) (a little bit) + bi'kuri-suru, ga'kari-suru, u'kari-suru, etc.*

Thus, the higher level of lexicalization of the *-suru* form is confirmed, though the degree 2 words which occur in this construction are limited to the onomatopoeias denoting degrees.

4) The questionnaire is based on Uemura (1965), which consists of five items concerning "large or small/small or large/square or round/bright or dark/male or female." Here listed are all nonsense words, of which, taking *gatulo*, *getulo*, *gitilo*, for example, we get the following results with regard to the question of "large or small."

	Uemura (Japanese informants)		Kakchi (Non-Japanese informants)
	I	II	
gatulo	78%	84%	90%
getulo	19%	12%	7%
gitilo	3%	4%	3%

Three Korean informants are among the 90% of the people who marked *gatulo* as "large."

the zero degree of lexicalization, i.e., the level of such nonsense words as used in the questionnaire. So the reexamination of the universal tendency of the sound symbolism in terms of the degree of lexicalization would be necessary. Especially, the clarification of the relationship between degrees zero, 1, and 2, with enough attention to the sound system of the respective languages, would be the central concern of our next investigation.

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Lexical Semantics in Montague Grammar

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I

The aim of this paper is to develop a possible mechanism for the analysis of lexical meanings in the framework of Montague Grammar. Traditional theories such as componential analysis, or generative semantics are not adequate candidates for the description of lexical meanings, since those approaches do not have any device to assign semantic interpretations to lexical items. As David Lewis (1970) claims, semantics without any treatment of truth condition is not semantics. In the past, Montague grammar was not a possible candidate either, since most Montague grammarians were rather reluctant to expand the PTQ framework to incorporate analyses of lexical meanings into it. Richmond Thomason (1974) claims that we should not expect a semantic theory to furnish an account of how any two expressions belonging to the same syntactic category differ in meaning, since the making of a dictionary demands considerable knowledge of the world. Thus, in traditional Montague grammars, lexical items except proper nouns and logical words translate into numbered or indexed non-logical constants in intensional logic. As you see in the table below, logical words such as *be*, *necessarily*, *every* and *a(n)* are represented by using lambda notation, and non-logical words of the categories IV, CN, and TV are represented by primed expressions which are usually used in place of numbered expressions. Here, we have to note that lexical items of the same syntactic category are assigned the same semantic type. In other words, there is no way except indexing to make a semantic distinction between IL-expressions of the same semantic type. Moreover, although the indexing device guarantees the interpretation of IL-expressions with respect to a given model, it does not reflect what we know concerning the semantic distinction between two lexical items.

EXPRESSION	CATEGORY	TRANSLATION	SEMANTIC TYPE
walk, run, swim, dance	IV	walk', run', swim', dance'	$\langle e, t \rangle$
dog, cat, horse, zebra	CN	dog', cat', horse', zebra'	$\langle e, t \rangle$
kill, love	TV	kill', love'	$\langle \langle s, f(T) \rangle, f(IV) \rangle$
be	TV	$\lambda p \lambda x \lambda y \{ \hat{y}(x=y) \}$	
necessarily	t/t	$\lambda p [\Box^* p]$	
every		$\lambda P \lambda Q \wedge x [R\{x\} \rightarrow Q\{x\}]$	
a(n)		$\lambda P \lambda Q \vee x [P\{x\} \& Q\{x\}]$	

Asa Kasher (1975) argues that the traditional PTQ grammar should be revised so as to let it be an empirically adequate framework for describing natural languages and claims that we have to capture in the theory the difference between the two lexical items. David Dowty (1979) tries to capture lexical meanings in terms of lexical decomposition. However, his primary concern is the treatment of logical words and he is reluctant to the analysis of non-logical words. In what follows, we would like to sketch our system for the description of both logical and no-logical word meanings, based on the philosophy that a lexical meaning of a word can be described without knowing its total concepts.

II

Let us briefly explain how our system is organized. Our system is composed of the set of basic expressions consisting of, what we call, accessible partial expressions which are similar to but more complex than traditional semantic features, compositional morphological rules which combine accessible partial expressions to make larger accessible partial expressions in "bottom-up" fashion, and semantic rules which are the translation rules of their corresponding morphological rules. The general form of compositional morphological rules is given in (1), where a and b are accessible partial expressions, A , B , and C are morphological categories, and F_1 is one of the following operations: concatenation, conjunction, and disjunction.

- (1) if $a \in A$ and $b \in B$, then $F_1(a, b) \in C$.

The general form of semantic rules is given in (2), where a' and b' are accessible partial concepts which are the translation of accessible partial expressions.

- (2) if a and b translate into a' and b' respectively, then $F_1(a, b)$ translates into $a' (\wedge b')$.

Accessible partial expressions shall be determined in terms of the examination of sentences such as the following.

- (3) a. When a person is walking, there is such a moment that both of his feet are on the ground.
 b. *When a person is walking, there is no such moment that both of his feet are on the ground.
 c. *When a person is walking, there is such a moment that both of his feet are off the ground.
 d. When a person is walking, there is no such moment that both of his feet are off the ground.
- (4) a. *When a person is running, there is such a moment that both of his feet are on the ground.
 b. When a person is running, there is no such moment that both of his feet are on the ground.
 c. When a person is running, there is such a moment that both of his feet are off the ground.
 d. *When a person is running, there is no such moment that both of his feet are off the ground.

Sentences in (3) and (4) assure us that the difference between *walking* and *running* depends on the existence of such a moment that both of a person's feet are on/off the ground. Based on this observation, we count (+BOTH FEET BEING ON THE GROUND) as an accessible partial expression which forms a part of the internal structure of the verbs such as *walk* or *run*. In the same way, we shall get some other partial accessible expressions for the description of motion verbs such as (+BOTH HANDS BEING ON THE GROUND), (+BOTH KNEES BEING ON THE GROUND), (+BOTH BUTTOCKS BEING ON THE GROUND), (+ONE'S STOMACK BEING ON THE GROUND), (+ONE'S BODY BEING VERTICAL), and (+ONE'S BODY BEING HORIZONTAL). These accessible partial expressions shall translate into corresponding accessible partial concepts as shown in (5).

- (5) 1. (+BOTH FEET BEING ON THE GROUND)'=APC1
 2. (+BOTH HANDS BEING ON THE GROUND)'=APC2
 3. (+BOTH KNEES BEING ON THE GROUND)'=APC3
 4. (+BOTH BUTTOCKS BEING ON THE GROUND)'=APC4
 5. (+ONE'S STOMACK BEING ON THE GROUND)'=APC5
 6. (+ONE'S BODY BEING VERTICAL)'=APC6
 7. (+ONE'S BODY BEING HORIZONTAL)'=APC7

where: APC's are of the semantic type $f(\bar{IV}/\bar{IV})$.

Semantic types of these accessible concepts shall be determined by the relations given in (6).

- (6) 1. $f(t) = f(t) = t$
 2. $f(CN) = f(CN) = f(\bar{IV}) = f(IV) = \langle e, t \rangle$
 3. For all categories A and B, $f(A/B) = \langle \langle s, f(B) \rangle, f(A) \rangle$

Thus, the formal representation of the relation between the translation of a lexical item and its accessible partial properties can be shown as in (7). Here, APCn denotes set of individuals and those of other APC's denote function from the properties of individuals to set of individuals.

- (7) a. $\bigwedge x \square [\text{walk}'(x) \rightarrow (\text{APC1}(\wedge \text{APCn}))(x)]$
 b. $\bigwedge x \square [\text{run}'(x) \rightarrow (\sim \text{APC1}(\wedge \text{APCn}))(x)]$
 c. $\bigwedge x \square [\text{walk}'(x) \rightarrow (\text{APC6}(\wedge \text{APCn}))(x)]$
 d. $\bigwedge x \square [\text{run}'(x) \rightarrow (\text{APC6}(\wedge \text{APCn}))(x)]$
 (8) a. $\bigwedge x \square [\text{walk}'(x) \rightarrow (\widehat{P}[\text{APC1}(P) \wedge \text{APC6}(P)] (\wedge \text{APCn}))(x)]$
 b. $\bigwedge x \square [\text{run}'(x) \rightarrow (\widehat{P}[\sim \text{APC1}(P) \wedge \text{APC6}(P)] (\wedge \text{APCn}))(x)]$

where: APCn = (+MOVE) and of the semantic type $f(\bar{IV})$.

Meaning postulates in (7) guarantee that the partial lexical meaning of a lexical item can be represented by set of accessible partial properties. (8a) and (8b) show the conjunction of the meaning postulates of each lexical item in (7). In addition to these devices, our system has a set of redundancy rules which show the semantic relation between accessible partial properties and which restrict the occurrence of accessible partial properties in the formal representation:

- (9) a. $\bigwedge x \bigwedge P \square [\text{APC6}(P)(x) \rightarrow \{\sim \text{APC2}(P)(x) \wedge \sim \text{APC3}(P)(x) \wedge \sim \text{APC4}(P)(x)$

- $\wedge \sim \text{APC5}(P)(x)]]$
 b. $\wedge x \wedge P \square [\text{APC7}(P)(x) \rightarrow \text{APC1}(P)(x)]$
 c. $\vee x \vee P [\sim \text{APC6}(P)(x) \wedge \sim \text{APC7}(P)(x)]$, then
 $* \wedge x \wedge P \square [\text{APC6}(P)(x) \rightarrow \sim \text{APC7}(P)(x)]$

Here, for instance, (9a) shows that if APC6 is in the logical form then APC2, APC3, APC4, and APC5 need not be in the logical form. (9c) shows that so long as there is a case such that someone's body is neither vertical nor horizontal, the complementary relation between APC6 and APC7 does not hold.

III

In this paper, we have shown how our system can determine the partial meaning of a lexical item. We have suggested that lexical semantics is not different from compositional semantics.

Notes: a. Each accessible partial concept shall have its equivalent logical form. For instance, $\text{APC1}(x)$ is equivalent to $\vee y [\text{MOMENT}'(y) \wedge \lambda x \text{BOTH } z [\text{FOOT OF}'(z, x) \rightarrow \text{ON THE GROUND}'(z)]]$.

b. What we call accessible partial concepts can be understood as what Karttunen & Peters (1979) calls conventional implicature. This shows that the notion of accessible partial concepts is independently motivated.

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A Sociosemiotic Approach to Contrastive Lexicology: "BEAUTY" in English and French

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Despite the large number of word-field studies on the basis of *language as a system* contrastive, cross-language lexicological studies have remained scarce though there is a growing demand for them from both language typology and foreign language teaching (cf. Kühlwein-Thome-Wilss).—In addition to problems inherent in any lexicological study (e.g. the definition of semantic primes), contrastive lexicological studies are facing two further problems: the question of *tertium comparationis* respectively the equivalence problem, and the question which linguistic models and procedures are suited best for language comparison.

As a consequence of the respective discussion we are witnessing a widening of the scope of contrastive research beyond the confines of systemic linguistics into two further dimensions:

- (a) into the investigation of *language as knowledge* via manifold associative experiments or via a re-interpretation of the findings of contrastive linguistics from a psycholinguistic perspective which is supposed to lead to a conceptual basis for contrastive analyses instead of a formal/functional one; though we need not follow this latter approach to extremes one can look out for language specific conceptual strategies which account for the forming of concepts which interrelate with correspondingly differing language structures; the problem of a universal conceptual grid as *tertium comparationis*, however, remains unsolved—and the long history of onomasiological tradition is telling us sad story in this respect;
 - (b) into *language as behaviour*, setting out from the assumption that different languages are different emanations of different cultures; the core of this approach is the question of how people behave and what is expected from them in a given culture and ties in with Halliday's sociosemiotic argument that we can only say what we can mean and we can only mean what we can do (wherein 'do' should be interpreted in a very wide sense beyond the limited sphere of mere physical or social activities, however); for cross-language analysis this approach will ultimately have to include the study of contrastive communicative performance. The sociosemiotic properties of language compared are in focus.
- As an example we draw on BEAUTY (B) in Englisch (E) and French (F) (restricted to human beings). What should be found out is
- (1) the general inclination or disinclination towards resp. against attributing or expressing B, i.e. its degree of importance in different cultures,

- (2) which sociosemiotic conditions must be met to assign B to somebody in different cultures: the way a person dresses, looks, bears himself/herself ...?
 (3) the language specific possibilities to realize these constellations.

This paper will concentrate on (1) and (2); for (3), for methodology, and statistical evidence of the corpus-bound empirical analysis cf. Nies.

The three most relevant criteria according to which B is assigned to persons in both languages seem to be:

- (a) semantic reference to clothing *vs.* body (P1–P2), (b) aesthetic judgment (P3–5), (c) non-aesthetic judgment (P6–11).

As a whole members of the F speech community are more generous in attributing B to persons than members of the E speech community. In both languages B is attested to females (♀), considerably more frequently than to males (♂), but: the relation [+B] : [–B] equals 2:1 for ♀ in E, whereas it is 4:1 in F—and the only creature that ends up with a global dominance of [–B] is the ♂ in E.

P1, P2 [*± well-dressed vs. ± physical appearance*]. — F♀ *vs.* E♀: For the F♀ adjectives which primarily refer to being well-dressed can at the same time more easily evoke the impression of physical B than E counterparts. The same tendency is evinced even more clearly by F adjectives primarily denoting physical B of a ♀ when they come to be used for being well-dressed. — F♂ *vs.* E♂: for ♂ adjectives primarily referring to being well-dressed evoke the impression of good physical appearance also more easily in F than in E, but the difference is a slight one only; there is, however, a greater reluctance in F than in E to use adjectives which primarily refer to physical B of the ♂ at the same time for being well-dressed of the ♂. — It seems that on the whole the semiotic impact of good clothing is related more closely to that of good physical appearance in F than in E, in particular for ♀. — F♀ *vs.* F♂: The a. m. association of good physical appearance triggered by adjectives that primarily refer to being well-dressed is stronger for ♀ than for ♂ in F. The analogous tendency is even stronger with adjectives that primarily refer to physical B in F: They allow to associate being well-dressed considerably more easily for the ♀ than the ♂. — E♀ *vs.* E♂: In E adjectives primarily used to indicate a state of being well-dressed for the ♀ may be used to refer to physical B just a little bit more easily than this is the case with ♂; when it comes to the effect of adjectives primarily referring to physical B upon the associating of a state of being well-dressed as well there is no distinction between ♀ and ♂ in E. — Obviously sex-specificity plays a more important role in this respect in F than in E.

P3: *Constitution [refined vs. coarse features]*. — F (*vs.* E) is rather strongly inclined towards attributing B-adjectives which primarily refer to physical appearance on the basis of refined features; with coarse features, however, F tends towards attributing B-adjectives which primarily refer to clothing—whereas in E B-adjectives which primarily refer to clothing are used to an equal extent irrespective of refined or coarse features; on the other hand E is more inclined towards attributing B-adjectives which primarily refer to physical appearance

despite coarse features (with the exception of *beautiful*, however). Obviously the distinction refined *vs.* coarse features is more important in F.

P4: [\pm Perfection of appearance], P5 [\pm Harmony of appearance].—Both features are more important in F than in E, especially as regards clothing-related B-adjectives; but: the chances of receiving body-referring B-adjectives are higher for a perfectly appearing person in E than in F, especially for the E δ .

P6: Age [\pm looking one's age (mid-age)].—Altogether in this respect F is rather generous to the ♀, E being somewhat more lenient with the ♂, but harsh towards ♀ who try to look younger than they are.

P7: Sexually typical appearance [\pm feminine/masculine looking].—For ♀: No sex-typicity, no B-adjectives in both languages; for ♂: distinctly ♂ appearance: easy evocation of B-adjectives primarily referring to the body in both languages; only slight evocation of adjectives primarily referring to clothing with E δ .

P8: Vanity [\pm intention to impress by a good appearance], P9: Warmheartedness [\pm amiable, friendly], P10 Naturalness [\pm natural, unassuming *vs.* well-groomed, cultivated], P11: Seriousness [\pm serious, earnest *vs.* cheerful, serene].—P8 is quite relevant for the F♀ (*vs.* E δ and ♀); the impact of P9 and P11 is low in E and F, cheerfulness, however, scoring positively for the F♀; as for P10 F reacts very sensitively towards both natural and cultivated for the ♀.

Ranking of criteria.—The analysis distinguished B-adjectives which are "compatible" with both ♂ and ♀ from those which (largely) go with one sex only. The adjectives of both groups can be evoked by a far greater variety of features for the F♀ than for anybody else. Within the "compatible"-group of adjectives even contrary features are found among the top ones for the F♀ like [+ natural] and [− natural] or [+ harmony] and [− harmony]. In E the attributing of these B-adjectives seems to follow a more clearly profiled image of the ♀, whereas in F it seems to be much more strongly oriented according to the specific person concerned. Within the other group of adjectives psychosomatic features rank considerably higher for the F♀ than for the E♀. As for ♂ aesthetic features rank higher for the F♂ than for both the E δ and ♀.

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A Multi-Level Approach to Word-Formation: Complex Lexemes and Word Semantics

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Complex lexemes differ in a number of respects from simple lexemes. These differences can best be captured by a multi-level approach to word-formation that describes analysable and more or less motivated lexemes and their creation and interpretation.

In his classical book on English word-formation with the subtitle "a synchronic-diachronic approach" Marchand (1969:53ff) offers "a pattern for the description of composites" which distinguishes between morphologic shape, morphologic structure, grammatical deep structure, grammatical and semantic content, and what he called "type of reference". By this term he understands "selectional patterns of information" relating complex lexemes to underlying sentences. Word-formation is thus regarded as pertaining to several interrelated levels.

More recent treatments by Kastovsky (1977) and Dressler (1979) go in the same direction. The former considers word-formation to be "at the crossroads of morphology, syntax, semantics, and the lexicon", while the latter proposes "a polycentric theory of word formation" with basically the same "semi-autonomous components" (1979:426). In the following it will be shown that even more levels and distinctions have to be taken into account and that only an integrated approach can capture all aspects of complex lexical items.

The levels and approaches to be recognized include six points:

1. An analytic and a synthetic procedure must be distinguished, and the latter presupposes the former. The analytic view-point starts from a structured lexeme (e.g. *theatre-goer*) and, by paraphrasing, arrives at an underlying syntactic group or sentence. The synthetic method, often found in generative treatments, takes a proposition or sentence as the starting point (e.g. *someone goes to the theatre*) and derives a reduced syntagma from it. Additional semantic features have to be added in the process, such as [+ Habitual] with *theatre-goer*, or [+ Purpose] in *writing-table*, *drawbridge*.
2. A methodological separation between synchronic and diachronic aspects is absolutely necessary. The speaker has no historical memory. Although the verb *peddle* is genetically derived from *pedlar/peddler*, synchronically *peddler* is an agent-noun derived from the verb *to peddle*. On the other hand, lexicalization and some aspects of productivity can only be explained diachronically. The diachronic yield of certain patterns must, however, not be confused with the present-day creativity of *ad hoc* formations, such as

'contextuals' (in the sense of Clark & Clark) and 'deictic compounds' (in the sense of Downing).

3. "Morphologic shape" could be considered independently of meaning, according to Marchand, by describing *craft/s/man* as 'noun + s + noun', *pott/er* as 'noun + suffix' and *re/write* as 'prefix + verb'. Obviously such a description in terms of morphemes and word classes alone, misses important differences, such as those between *crybaby* and *drawbridge* (both V + N) or *steamboat* and *girlfriend* (both N + N). Semantic aspects must be included in the description, as soon as we compare suffixes like *-er* in *bak/er* (Agent), *blott/er* (Instrument), *sleep/er* meaning 'train with beds' (Adverbial of Place). On the other hand, the following are all agent-nouns in spite of different morphologic shapes: *grave-diggER*, *cut-throatØ*, *car-THIEF*, *cookØ* [where Ø symbolizes a zero-morpheme]. In nominal compounds such as *opera house*, *gunpowder*, and *baking-powder* a common semantic feature [+ Purpose] can be established, but this, obviously, does not exhaust their semantic analysis.
4. The syntactic level is most obviously relevant in nominalizations, which according to Motsch, function as syntactic recategorizations. The examples *his rapid drawing of the picture*, *theatre-goer*, *latecomer*, and *essay-writing* can and must clearly be related to sentences. This is more difficult with composites not containing a verb, and completely unrevealing with lexemes such as *whitish* and *booklet*. Again, a combination of the syntactic description with semantic aspects seems most rewarding. This can be done by assigning the head or 'determinatum' of the complex lexeme to a specific deep case in the underlying sentence. Thus superficially similar words can be distinguished such as *payER* (Agent), *cookER* (Instrument), *mournER* (Experiencer), *containER* (Object) but also ambiguous lexemes such as *dishwashER* (Agent or Instrument), *dinER* (Agent or Locative), *payMENT* (Object or Activity).
5. An independent semantic analysis is required for the lexical aspects of word-formation, i.e. the inclusion of complex words in the lexicon. This is tied up with the naming function of lexemes and is particularly relevant for the gradual 'lexicalization' process and its result. By this term I understand the phenomenon that complex lexical items, by frequent usage, may lose their syntagmatic nature and tend to become units with specific content (cf. Lipka 1981). This involves a demotivation and idiomatization, as in *blackboard*, *holiday*, *watchmaker*, *highwayman*, and *forehead*. Here the border between complex and simple lexemes becomes fuzzy.
6. Pragmatic aspects, finally, must be considered, both with respect to the naming function of complex lexemes and the level of 'parole'. The interpretation by the hearer/reader of 'contextuals', such as *pumpkin-bus*, *to bottle* (*demonstrators*), and 'deictic compounds' such as *the applejuice seat* (meaning 'the seat in front of which a glass of *applejuice* had been placed') clearly depend on context and situation. Often extralinguistic knowledge

of the world is also relevant, e.g. when analysing Downing's *cowtree*, which we would not presumably interpret as 'tree on which a cow sits'. Although one may argue that all such formations show systematic ambiguity, on the level of 'langue', their actual interpretation on particular occasions is determined by pragmatic factors. Functional considerations may also be regarded as belonging within the domain of pragmatics. Complex lexemes serve various functions, such as classifying (e.g. *wine glass*, *beer glass*; *front door*; *bilge water*), naming (e.g. *Reagonomics*, *space shuttle*), pronominalization in texts (as in the sequence: spring thaws begin to *break up* the winter ice. Taking advantage of the *breakup*... etc.), and finally the function of information condensation (e.g. *pen friend* defined in the *Dictionary of Contemporary English* as 'a person, esp. in a foreign country, whom one has come to know by the friendly exchange of letters, but whom one has usu. never met'). Thematic meaning on the basis of a topic-comment structure is also signalled by complex lexemes as in *apple-eating* vs. *eating-apple*.

Only a multi-level approach can do justice to all aspects of word-formation. A morphological and a limited syntactic description may be given independently of meaning. However, an adequate account must recognize the role of semantics on all levels and their interrelation. While deep cases and pragmatics, on the level of 'parole', presuppose a sentence semantics and a text semantics respectively, all the other aspects of complex lexical items fall within the domain of word semantics.

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Hebrew Verbs of Dress: Semantics and Collocation in a Contrastive Setting

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The tendency of contemporary linguistics is to focus on the wider human context of utterance as providing a more satisfying and appropriate source of explanation of the way stretches of language are put together in patterns of sound, sense and linear structure than the context-free "grammar" approach associated with the earlier Chomsky. 113

The study of verb/noun-object collocations within a specific semantic field enables the linguist to direct attention to the constant interaction between the speaker's creative contribution to the models of speech that he hears, i.e. meaning and culture awareness and the impact of the patterns themselves, in the sense of their distributional configuration. In my opinion, these two aspects cannot be isolated, they impenetrate. The interplay of factors is forcefully illuminated by the language of the examples I have chosen—modern Hebrew. It was, as is well known, revived as a spoken, everyday medium within living memory after millenia of a more or less liturgical and literary existence. It thus provides a peculiarly accessible example of the impact of a colloquial register on literary-inspired collocational models which strive to persist through the activities of the scholarly linguistic establishment statutorily represented by the Hebrew Language Academy. The lexicographical apparatus in the standard dictionaries illustrates the clear divide between modern usage and coinage and the Biblical and post-Biblical models.

I survey here the Hebrew verbs of dress contrastively with English as a convenient and necessary meta-linguistic analytical device (Backhouse, 1981: 17). The field of dress in English is governed by the state verb "wear"¹ and the "dummy" action verb "put on"². The field in Hebrew is, on the one hand, not as parsimonious in specialised verbs of dress as English, but by no means as generous as Japanese (Backhouse, 1981), a variable, no doubt, as in colour, kinship, flora and fauna terminology that reflects the needs, past and present of the respective speech communities. I have distinguished, in terms of their object restrictions, four types of such verbs, ranging from a relatively promiscuous non-selectivity to a rigid concatenation with one specific object. Type (1)

1) Ignoring the infrequent use of a dress verb like "don".

2) The distinction between "state" and "action" verbs is diagnosed by the acceptability of their use in answer to the question: "What are you doing?" or, "What is happening?"—"I am wearing/putting on my dress".

the dummy action verb *sam* equivalent to the English "put on"; (2) the specific but unrestricted dress verb that heads the semantic field: *lavash* approximating to "wear" but unlike English clearly operating as a combined state/action verb; (3) a series of three other specific state/action verbs that are restricted in their objects to articles of clothing associated with different body parts and/or mode of attachment—headgear, footwear and accessories, respectively; and (4) what Palmer (1976: 97) terms "strictly collocational" or what might be defined as idiomatic verb/noun-object combinations that are totally restricted.

Type (1), which is incidentally documented too in Biblical Hebrew, is naturally the most frequently used verb in the colloquial register followed by type (2), the characteristic dress verb: *lavash*. In the informal register it is used with all articles of clothing. It would seem plausible to assume *ex post facto* that the verb which is associated with the most common articles of dress: trousers, skirt, coat, etc., that cover the body as a whole, being most frequently used would tend to become the maid-of-all-work. The noun-objects are the focal point in the sense that their commonality and frequency of concatenation with the verb tend to de-specialise the latter and lead to it becoming the archetypal verb of dress. The appropriacy of the characteristic dress verb *lavash* recedes in the mind of the speaker to the extent that the articles involved are less associated with covering the person as with accessories such as jewellery but *lavash* remains a possible option with head and leg coverings. When I outlined this paper I had not read Bolinger's stimulating paper on "Meaning and Memory" (1976) which reinforces the point I am trying to make, that the distributional holistic patterns, the statistical frequencies of occurrence interact with the creative meaningful operations of the speakers, i.e. memory with meaning to produce what Bolinger describes as "the heterogeneous but tightly organised character of some aspects of language".

When we come to the third and fourth types we are confronted very vividly by the differences between the colloquial and educated register. Headgear is governed by the Hebrew verb *xavash*, originally reflecting the characteristic mode of attachment: winding turban-wise round the head but extended to all headgear including wigs and masks. The radio campaign for better Hebrew continually reminds listeners to use these specialised dress verbs: *xavash* for headgear, *na'al* for footwear, *garav* for socks, the latter an interesting example of a deliberate back-formation coinage from the noun *gerev* (sock), *'anav* for ties, *hirqiv* for spectacles. The universal principles of economy of effort, lexical simplification and social tension all combine alternately to blur and perpetuate the differences between the colloquial register that less frequently resorts to the specialised verbs and the educated purist one which strives to maintain their use.

Another perennial question which such a study evokes is the relation between the meaning of the verb and so-called selectional restrictions on its object (McCawley, 1970: 1967; Newman, 1975: 78). The circularity of the problem is what we require to emphasize—once we know the patterns—which

Table of Dress Verbs and Objects in Hebrew

Hebrew verbs	"WEAR" and "PUT ON"							
	<i>lavash</i>	<i>xavash</i>	<i>na'al</i>	<i>'anad</i>	<i>'garav</i>	<i>*anav</i>	<i>'xagar</i>	<i>hirqiv</i>
Type of object	dresswear ¹	headgear	footwear	jewellery	socks	tie	belt	spectacles
General	verb: "PUT ON", <i>sam</i> + unrestricted object ²							

1. All articles of clothing in informal or colloquial register
2. informal register

is the starting point of speaker operations—we can and so consciously and sub-consciously set up in our minds as natural speakers or "discover" on paper in our unnatural role as aspiring linguistic scholars such useful devices as semantic markers or meaning components. The attribution of the meaning "putting on headwise" to *xavash* may be regarded as both the cause and effect of it being used with headgear. This mutual feedback between both the implicit or explicit noun-object and verb is brought into sharper relief when we consider the variable collocative acceptability (in the educated register) of the same verb in its respective marked and unmarked uses—a point we have made in the field of cooking terms in English (Newman, 1975: 57):

- (1) *mah att loveshet?—kova', me'il veba-na'alayim ha'-adumot sheli*
What are you wearing?—a hat, coat and my red shoes.
- (2) *mah att osah?—ani *loveshet 'et ha-na'alayim sheli*
What are you doing?—I am putting on my shoes.

In (1) no specific article of dress is implied; therefore *lovesh*, the unmarked dress verb, is in order. In (2) however the restriction to one object evokes the acceptable collocation, more especially so in the context of an action, and the verb that is called for is the one used with shoes (*no'el*). The specificity of the object is matched by the specificity of the verb and vice versa. There is no clearcut distinction to be made between semantically and collocatively based syntagmatic units (cf. Matthews, 1981: 5). The collocation both expresses and creates meaning and this is a dynamic process affected by a host of factors that some would prefer to keep out of linguistics proper, relegating to such preserves as pragmatics, socio- and psycho-linguistics. I prefer a pluralistic interdisciplinary approach with a separation of levels and disciplines adhered to when it is useful and appropriate.

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Ordinary Language as Metalanguage in Word Semantics

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Lexical heterogeneity

1145 The principles of organization in the lexicon have attracted relatively less attention than non-lexical branches of linguistic theory. This fact is probably due to the common view of the lexicon as merely a list of idiosyncratic entities, in particular such phenomena in a language as cannot be captured by rule-writing. Furthermore, the very complexity and heterogeneity of authentic lexical inventories present serious obstacles to the analysis of the lexicon as a subsystem of our language competence in general. This might be the reason why the typical lexico-semantic study deals with single lexical items or small groups of semantically related items (word-fields or semantic domains).

The heterogeneity of spontaneously developed lexicons is manifold. It has been suggested that word semantics in different languages may be sensitive to a whole array of typological features, such as relative analyticity (i.e. the degree of semantic transparency characteristic of the total lexical system), taxonomic depth (i.e. the proportion between particular and generic terms), patterns of meaning extension, areas of synonymy, collocational patterns, etc. (Fillmore 1978). It is obvious that different domains *within* the vocabulary of a single language may vary extensively in these respects, too. Other types of differences in word-semantics displayed by different words may be associated with the role played by the respective items in the grammatical system. Thus there is a gradient ranging from more or less purely grammatical operators and structure-dependent items all the way to conceptually very loaded items. Lexical heterogeneity is likewise brought about by sociocultural conditions: one may find particular vocabularies used in specific social settings, honorific language, religious language, secret languages and jargon, technical language in various fields etc. Methods for reducing all this lexical heterogeneity and redundancy are needed in order to come to grips with the essential lexical system.

Controlled vocabulary

Several factors have lately influenced lexical study. Although usually designed for other than theoretical purposes, existing dictionaries offer abundant lexical information, largely in implicit form. A number of ongoing lexical projects have been directed towards recovering and systematizing such information. Recent lexicography has itself relied more on solid theoretical considerations than was earlier customary. Converseley, a host of theoretical

problems tend to arise from practical lexicographical work in this vein.

Another field which has had a strong impact on lexical study is computational linguistics. Not infrequently, the computational linguist has to solve lexicological problems, whether his task is basically linguistic or not, because his system requires some kind of lexicon to function properly. My own experience of lexicology stems primarily from computer-based lexicological investigations. A rather extensive project at the University of Gothenburg, called Lexical Data Base, aims at establishing a well-structured, machine-readable data base containing a wide variety of lexical information, stored in network form, about some 150,000 words. An integrated computational system is being developed, covering the whole procedure from implementation and storage to computer-based production of practical dictionaries. The data base may jointly serve as reference material in lexicographic work and be taken as a point of departure for machine dictionaries with more specific scopes.

The basic lexical entity is the lexeme. As taken here, it is based on kernel senses. The potential semantic range of a kernel sense, hence of a lexeme, is defined by a set of principles for sense extension and sense restriction. The notion of kernel sense and the rule system for defining its maximal semantic range are fitted into a grammatical model of the case grammar type.

The lexemes could be said to be partially ordered sets of defining propositions with conventionalized expressional labels (see Järborg to appear). Their semantic definitions are, in the data base, formulated in ordinary language, albeit in a controlled manner. Besides being given in precise formats, the definitions are expressed in terms of a minimal *defining vocabulary* decided on.

This is a device that has occasionally been utilized in practical lexicography. By crystallizing a very restricted subset of the vocabulary and defining all other words in the language in terms of this minimal vocabulary, one can keep track of the circularity in the definitions, traditionally a vicious problem to lexicographers. When dictionaries have made use of defining vocabularies, it has usually been for pedagogical reasons; "difficult" words are explained by means of "easy" words. It is evident, however, that a defining vocabulary—in particular when used in a machine-readable dictionary—enables us to trace semantic relations through the whole vocabulary.

Defining vocabularies are intuitively attractive. They seem to capture the notion of basic vocabulary, the general lexical subset mastered by everyone. The lexico-semantic decomposition into definitions also seems to parallel a sort of undertaking which native speakers of various languages keep performing constantly: reformulation within the boundaries of the same natural language, at a more or less precise level. Indeed, one might argue that, supposing that there are substantial semantic primitives in the first place, actual words are very natural entities from the language-user's point of view (as does, e.g., Wierzbicka 1972).

Although the units of a defining vocabulary comprise a finite set of terms used, by definition, axiomatically, it is easy to realize some important

differences between them and a conceivable set of semantic primitives. For one thing, there can be no question of universal validity. Actual defining words remain under the sway of the natural language they define, more in some parts of the vocabulary than in others (e.g. flora, fauna, culture-dependent areas etc.). Most importantly, there can never be established an optimal defining vocabulary, in any theoretical sense. Every choice is arbitrary, or at least purpose-dependent. The use of a restricted defining vocabulary remains primarily a *means of investigating* word semantics, the semantic relations holding between the units in a lexical network, *and of detecting* lexical patterns; it offers an entrance to the dynamics of a lexicon and helps clarify, e.g., what conceptual areas and which grammatical mechanisms are basic to the lexicon of a given language.

Semantic strata

When defining vocabularies have been used in pedagogical dictionaries, a number of disadvantages have been noted. In determining the defining vocabulary, the abstractness problem is notorious. An explanation of "glasses of water" in terms of "vessels of liquid" does not imply a pedagogical achievement. It has also been emphasized that semantic decomposition creates syntactic complexity. Compare, e.g., the following fragment of an English translation of an old Chinese poem with its counterpart in Stefan Themerson's "semantic poetry" (Themerson 1975:26, 31; the visual organization of the poem has been modified here):

This joy shall last for ever, / Moon, hear my lay, / My shade and I can caper / Like clouds away.

"The existence of this emotion shall continue for a period of time which is greater than every assignable quantity / Body attendant on the Earth! Let the vibrations of my short lyrical song stimulate thine external ear-drum & be conveyed to thine internal ear-drum & thence to thine internal ear fluids / Let them cause impulses to pass up thine auditory nerve to the hearing center in thy brain / I & the patch of darkness produced by the intervention of my body between the surface of the Earth & thy light / we can move rapidly like the masses of minute droplets of water suspended at high altitudes in the gases of the air away."

A strict use of a defining vocabulary gives, in principle, the same absurd effect as can be observed in "semantic poetry". An expression like *to angle* means approximately 'to fish with a hook and line', not 'to try to get hold of fish with a hook and line'; the latter paraphrase resulting from a successive decomposition of *to fish* into 'to catch fish' and of *to catch* into 'to try to get hold of'. The depth in the linguistic analysis is levelled out, if the *lexicon* is considered exclusively on two fixed levels: that of the lexical entries and that of the metalanguage.

Instead, we need a set of defining terms taken from different semantic

strata, much in the same way as has been suggested by Weinreich (1962:38). The most adequate defining word is sometimes a word which must itself be further defined (e.g. Binnick 1971). Words that are not themselves members of the metalanguage proper, but eventually reducible to this sort of indivisible defining primitives, are occasionally allowed in definitions, which gives a much more flexible system. The defining vocabulary is rather assigned the status of a semantic checklist, where ultimate semantic disambiguation is guaranteed. We find it intuitively attractive that the lexico-semantic system is built on extensive cross-referencing between concepts that are actually lexicalized—since this is the normal way of thinking of word-meaning—and that the semantic specifications of words directly reflect the character of the lexicon as an associative network.

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Linguistic Universals as General Tendencies

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Introduction

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In sciences such as physics or chemistry, it is commonly agreed upon that if a law applies to 75–80% of the cases, it is acceptable. In a human science such as linguistics, we clearly cannot require more, and would rather be satisfied with less. This is because languages, as scientific objects, are much more complicated than any of the objects that the so-called exact sciences deal with, since they are spoken by human, i.e. highly changing, societies. Consequently, the search for language universals, a quite legitimate search in itself, implies that the term “universals” should not be taken literally, but only as an incentive to the broadest possible inquiry. The only true universals are those which we include in our definition of what a human language is. But since a definition is the very minimum for which one does not have to provide a demonstration, such *definitional universals* do not by themselves lead to the discovery of unknown properties of language. To get at that, one has to test *general tendencies*.

1. Justifying the Notion “Tendency”

Two facts can justify such an enterprise: first, all languages undergo pressures, both internal (because of the existence of structures at all levels), and external (because of the physical, mental and social characteristics of human beings); second, a message in any language can be translated, more or less adequately according to the circumstances and to the languages dealt with, into a message of any other language.

2. Functional Statements and Implicational Statements

This situation leads the linguist to hypothesize general tendencies, which are essentially of two kinds: 1) *functional statements*, relating functions and structures, e.g. “there is a correlation between word order and, on the one hand, focusing of certain elements, on the other hand, natural limitations on the memorial and perceptual capacities of human beings”; 2) *implicational statements*, relating two features, e.g. “if a language has p^h and t^h , then it has h ”, or “if a language has both *SOV* in a full sentence and *determining N + determined N* in a Noun Phrase, then it has at least some postpositions, which historically come either from verbs (ex. Bengali), or from nouns (ex. Mande languages (W. Africa))”. These statements apply to any level of language, phonology, morphosyntax or semantics. Furthermore, they come from the

linguist's own experience, but they must be involved in a *permanent dialectic movement*. Working on language universals, that is, consists of testing, on the most numerous and most various possible languages, hypotheses suggested by what the linguist knows of certain other languages (whose limits he consciously decides to go beyond, in order to avoid reductionism); he then modifies, slightly or deeply according to the qualitative and quantitative importance of the counter-examples, the initial hypotheses. Among the latter, those which apply to less than 50% of the examined sample will of course not, though, be simply discarded. Just because they do not hold as general tendencies, they will quite naturally be utilized in the field which is tightly related to the search for universals, of which it is a counterpart, i.e. language typology.

3. The Three Viewpoints

Now, in order to insert our search for general tendencies, i.e. both functional and implicational statements, within a fruitful framework, we need to have a theory of the organization of minimal linguistic utterances 1) in general, 2) in the particular case of the *semantic* relationship between the parts of the utterances. Fig. 1 and 2 hereafter meet these requirements: fig. 1 gives the organization of minimal linguistic utterances, which are conceived as being analyzable from three viewpoints: morpho-syntactic, semantico-referential and information-hierarchical; and fig. 2 gives the semantic types of minimal linguistic utterances, i.e. 6 types: 5 non-active and 1 active.

Figure 1: *The three viewpoints*

	Morphosyntactic viewpoint		Semantico-referential viewpoint	Information-hierarchical viewpoint	
	SUBJECT OF	PREDICATE OF	PARTICIPANT (agent or patient)	EVENT	TOPIC COMMENT
EXPANDED UTTERANCE	- functions : - categories : 1) In languages without (verbal) predicate inflected for person, the subject is represented by any of the following categories or associations of categories (=phrases): Noun, Pronoun, Noun Phrase, Relator Phrase, etc ¹ . 2) In languages with (verbal) predicate inflected for person, a) if the person is 1st or 2d, the subject is the personal pronoun or index, discontinuous or not b) if the person is 3rd, the subject is the personal pronoun or index (often a zero morpheme, according to languages), if and only if the latter can be expanded by something else in an enlarged (=non-minimal) utterance				
	- function : COMPLEMENT OF direct(sequential) indirect (with relator) - categories : Noun, Pronoun, Adjective ² , Noun Phrase, Adverb, Relator Phrase, etc ¹ .		- CIRCUMSTANCES spatio-temporal non-spatio-temporal oral temporal and/or NEW PARTICIPANT (patient or agent)		

(the arrows mean "corresponding mainly to")

1 The grammar categories mentioned here are the ones shared by most known languages, past and present. By "etc.", I mean any other category, rarer in the known languages and likely to function as subject.
 2 In languages which possess such a category, of course, since it is not universal (see Hagège 1974).

Figure 2: *The six semantic types of minimal utterances*

It is proposed here to tentatively set up a universal typology from viewpoint n°2, the semantico-referential viewpoint. Given the differences between the three types of organization of linguistic utterances, and given that these differences increase in proportion with the length of the utterances, it is methodologically safer to limit oneself here to minimal utterances. More precisely, there are at least two reasons to do so:

1) firstly, if we include cases with more than one participant and one process, then we reduce the chance of getting at a typology likely to apply to all human languages. In other words, it is hypothesized that the simpler the semantic structure, the more predictable its diffusion

2) secondly, since there is a more or less important, but, at any rate, inevitable distance between the abstract conceptual formula which represents meaning and its realization, in concrete languages, as an utterance, we reduce this distance if we reduce the number of things talked about. In formerly fashionable terms, the shorter the linguistic utterance, the smaller the difference between its form ("surface structure") and its "deep structure". And since our scope is to get at a typology likely to have a cross-linguistic validity, it is important to reduce the abstract conceptual schemes to their simplest manifestations.

Having set up the method, we can now go on to propose the following six types of semantic relations between a participant and a process:

Semantic types		Participant
non-active	1 equative	defined by the process
	2 attributive	qualified by the process
	3 situative	identified by its location
	4 existential	posited as existing
	5 descriptive	conceived as experiencer
active	6	having control over the process

4. General Tendencies in the Relationship between Form and Meaning of Utterances

a) *Dissymmetry*: i) Type 3 (fig. 2 above) is not the only one in which we find adverbs and relator-groups, since we also find them in, e.g., Russian *ja k vam* (I towards you), "I will go to your place", which belongs to type 6 (same structure in many other languages, including, e.g., Malay), and likewise in Old-Egyptian $X (=Noun) + \text{relator } m (= "as") + Y (=Noun)$, which means "X is Y", and therefore belongs to type 1; *hr*, "on" and *r*, "towards", in this same language, are used to express, respectively, the present and future of verbs, and therefore belong to types 5 or 6.

ii) Type 1 is not the only one in which we find nominal predicates: such predicates are also to be found in types 5 and 6, with our more participant, when "I want this" is expressed by "this is my wanting": such a structure occurs in Comox (a Salishan language of British Columbia), Palauan (an Indonesian language of Micronesia), and in various Melanesian languages. Nominal

predicates also correspond to type 6 in languages which use the same structure for the agent of the two-participant utterance with a definite patient and for the possessor: ex. Eskimo *arn-ara* (mother-my), "my mother" as compared to *tusarp-ara*, "I hear him" (= (he is) hearing-my).

iii) The notions corresponding to Engl. *can*, *may*, *must* are expressed by ordinary (non-defective) verbs in French, and by affixes in Turkish, as well as in Kečua (Peru), where "I must carry it" is expressed as "my having to carry it exists".

Given this situation, should we think that the forms of utterances are contingent, that they are what they are by mere chance?

b) *Symmetry*: if we examine expanded, as well as minimal, linguistic utterances, the relations between structures and meanings appear clearly, and we can set up general tendencies:

i) *Possessive structures*

Sentences expressing possession can present one (or several) of the following structures, X being the possessor and Y the possessed:

— X as defined by the fact of possession: type 1: "X (is) Y-possessor" (ex. Kečua, Aymara (Peru))

— X as qualified by this possession: type 2: "X (is) Y-ed/Y-having" (ex. North-eastern Australian languages)

— Y as being "in", "for", "with" X or at X's place, or, conversely, X as being "with" Y: type 3: "at X's place is Y" (ex. Russian, Hebrew, Aymara), or "X is with Y" (ex. many African languages)

— Y as existing: type 4, logically related to type 3: "Y of X exists" (ex. Palau (Micronesia), Kečua, Aymara)

— Y as a more or less voluntary agent: type 5, logically related to types 3 and 4 (cf. *avoir/ya avoir* in French and its equivalents in other Romance languages: "X has Y" ("have"-languages, like English, French, or Moore (Upper-Volta); in the latter, subject and complement get permuted, according as a human being possesses (good) or is possessed (evil).

As is easy to see in these illustrations, it often happens that one and the same language has more than one process to express possession, according to the element which is to be topicalized, and to other criteria.

ii) *Semantico-structural pairings*

There is a general tendency towards an association of the six types in pairs whose members contrast, both formally and semantically, in most languages, where the existence of one implies that of the other:

— type 1 corresponds to the essence and type 2 to the qualities, hence the opposition, in Russian, between nominal predicate in the nominative and in the instrumental, in Finnish between the essive and partitive, in Spanish, Irish, etc. between two verbs "to be"

— types 3 and 4 are in implicational relationship as corresponding to the

opposition between definite and indefinite cf. Finnish *auto on kadulla* (car is street-in), "the car is in the street" / *kadulla on auto*, "there is a car in the street"

—the boundary between types 5 and 6 is fluctuating, because there are various degrees of *control* of a participant over a process, starting from the purely descriptive type, in which the participant is a mere experiencer of things happening to him (sometimes with specific markers, like the *na/ma* prefix of Tagalog) until the most active type, in which he deliberately acts in a certain way: Russian makes a wide use of the contrast illustrated by a pair like *mne xočetsja* (to-me wants-itself), "I feel like it" / *ja xoču*, "I want"; in most languages of India, there is a grammatical contrast (lexical in French or English) between two structures, as illustrated by the following Kanarese sentences: *avanige ha:du ke:lisitu* (to-him music was-to-be-heard), "he heard music" / *avanu ha:danu ke:lidanu* (he music (accusative) heard), "he listened to music".

For lack of space, I cannot mention other cases of relationships between form and meaning of utterances, but there are many more than the ones provided here. They can all serve as illustrations of general tendencies, both functional and implicational, in the structure of linguistic utterances. For more details on this matter, see Hagège 1982, chapter 2.

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Object Initial Languages and Their Implications for Word Order Universals

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1. Introduction

Research on word order universals and typology since Greenberg's seminal study (1966) has taken little account of object initial languages and it is only within the last five years that their unequivocal attestation has been achieved. Owing to the work of Greenberg, Lehmann and Vennemann among others, a number of implicational statements have become possible which allow for the characterization of languages as consistently VO or OV languages. SOV languages such as Japanese possess a cross-categorial ordering principle which serializes its constituents according to an operator-operand order. This principle yields objects before nouns, genitives before possessed nouns, adjectives before nouns and nouns plus postpositions. The predictive value of such a statement resides in the fact that should we discover a language which manifests an OV order, we expect also to find the ordering correlations in the other categories. The opposite ordering principle, operand-operator, treats VSO languages (Classical Hebrew) and SVO languages (English) as special cases of VO structure.

Until the existence of object initial languages came to light, we had only SOV languages as examples of OV structure. This is what led Greenberg to formulate universal no. 1.

In declarative sentences with nominal subject and object, the dominant order is almost always one in which the subject precedes the object.

In light of the research on object initial languages, this universal can only be maintained as a tendency. Derbyshire and Pullum (1981) surveyed twelve languages in the Amazon Basin which they argued are either OVS or OSV in their basic order. Their study concentrated on the relation of objects to verbs in transitive sentences and subjects to verbs in intransitive sentences. They did not investigate other word order parameters. This paper brings to the attention of linguists yet another object initial language and studies it from a typological viewpoint.

Wajana is a Carib language spoken in the interior of Surinam along the Tapanahony and Lawa rivers. Broadly speaking general linguists have neglected the Carib languages which is unfortunate because of their bearing on issues

of word order. This paper argues that Wajana patterns in its surface syntax like other OV languages and that its basic order is OVS. Thus object initial languages are taken to be specific realizations of OV structure in a way similar to the classification of VSO, VOS and SVO as specific cases of VO structure. I shall employ basic order in both a statistical and stylistic sense as well as claiming that it can be used across syntactic categories. I shall now adduce evidence from genitive-noun order, main verb-auxiliary order, and verb morphology to argue that Wajana is an OV language. Then I cite some examples which tend to indicate that OVS is the basic order in this language.

2. Genitive-Noun Order

A brief look at some Wajana sentences indicate that it too uses a GN order to express the possessor-possession relation. Since this paper argues that OVS and SOV orders are specific realizations of OV structure, a GN order in an OVS language would support that claim. Data from Wajana appear to confirm this. In all the examples I have seen, Wajana rigidly maintains a GN order. In the grammatical variations of the following sentence, a number of the constituents can be moved around and yet the GN order always remains fixed.

1. *Kan Pampilan w-ekale-jai tale pileowime-po iwetitomo -ja*
 God's paper I-give-PRES here pileowime-at my tribes people-to
 'I am giving God's Word to my tribe's people here (on/at) pileowime'

I draw the tentative conclusion that with respect to the two features of adposition and genitive-noun order, Wajana patterns in its surface order are like Greenberg's type III languages. In fact, I have not encountered one example to the contrary. The crucial nature of order is also emphasized by the fact that there are no morphological markers on either of the nouns to indicate which is possessor and which is possessed.

3. Main Verb-Auxiliary

The Aux in Wajana is actually the verb 'to be' and may be used as the MV such as in the following sentences:

- | | |
|--|------------------|
| 2. <i>ipok wai</i>
good I am | 'I am good' |
| 3. <i>ipoke-la wehaken</i>
good-not I was | 'I was not good' |
| 4. <i>enin man</i>
who you are | 'Who are you?' |
| 5. <i>min man</i>
where it is | 'Where is it?' |

When the verb 'to be' = *eise* is used as an AUX, it may be used in conjunction with a number of forms of the MV + suffixes. In negative sentences (i.e. the

MV is negated) the AUX is used frequently and may even be obligatory. In such cases, the AUX carries the categories of person and tense. If the subject is expressed, number is not indicated on the AUX. If the subject is included in the AUX, the number is also expressed:

- | | | |
|--------|------------------------------|-----------------------|
| 6. | <i>ipok man eluwa</i> | 'The man is good' |
| | good is man | |
| 7. | <i>ipok man kalipono-tom</i> | 'The people are good' |
| | good is people -PL | |
| but 8. | <i>ipok naitot</i> | 'They are good' |
| | good they are | |

These data tend to corroborate the conclusion stated above that Wajana patterns like other OV languages in its treatment of the main verb and auxiliary. The predominant order is main verb followed by the auxiliary.

4. Verb Morphology

Winfred Lehmann (1973) brought to light an important structural principle in regard to the ordering of elements. The principle is that verbal qualifiers (modifiers) "are placed on the opposite side of a basic syntactic element from its primary concomitant" (48). If Wajana is indeed an OV language we will expect that it will suffix its verbal qualifiers. This is in fact the case. The general subject and object markers may precede the verb root while all other markers must be suffixed. The following data give us a clear idea of how negation operates.

- | | | |
|-----|------------------------------|-------------------------------|
| 9. | <i>en-ili-la man</i> | 'he is not fixing it' |
| | it-fix-not he | |
| 10. | <i>k-upanangma-la-he man</i> | 'he does not want to hear us' |
| | us-hear -not-want he is | |
| 11. | <i>ew-eŋa-la wai</i> | 'I am not teaching you' |
| | you-teach-not I am | |
| 12. | <i>en-epa-la in</i> | 'I am not teaching it' |
| | it-teach-not me | |
| 13. | <i>en-ale-la eme</i> | 'You are not carrying it' |
| | it-carry-not you | |

Another qualifier is the interrogative particle /-ka/ which also follows the main verb. If the sentence has an auxiliary it follows /-ka/.

- | | | |
|-----|---|-----------------|
| 14. | <i>m-umekja ka</i> | 'did you come?' |
| | you-come Q | |
| 15. | <i>uwame ka man</i> | 'are you well?' |
| | well Q you | |
| 16. | <i>emna tamutpe ka man, Jakopi uhpolo?</i> | |
| | very much greater our father Q you Jacob our father | |

These examples show that while the question particle is not a bound morpheme, it does follow the pattern which we would expect for an OV language.

5. Basic Order: OVS

Of the three possible orders of OV languages, I shall present evidence here that Wajana is OVS in its basic order. Wajana allows considerable variation of its constituents; in transitive sentences with nominal subjects and objects, SOV, OVS and SVO are the most common orders with OVS being the preferred order.

- | | | | |
|-----|-------------------------|-----|------------------------|
| 17. | <i>wewe n-ene eluwa</i> | OVS | 'The man saw the tree' |
| | tree 3s-saw man | | |
| 18. | <i>eluwa wewe n-ene</i> | SOV | |
| 19. | <i>eluwa n-ene wewe</i> | SVO | |

An OV order also appears to be preferred in transitive sentences with locative phrases.

- | | | |
|------|--|--|
| 20. | <i>Kan omi w-ipanankma Kan pakolon-tao</i> | |
| | God's word 1s heard God's house in | |
| | 'I heard God's word in God's house' | |
| 21. | <i>Kan pakolontao Kan omi wipanankma</i> | |
| 22.* | <i>wipanankma Kan omi Kan pakolontao</i> | |

These data suggest that Wajana prefers a postverbal position for the Subject as well as a pre-verbal position for the Object.

A final argument for the post-verbal position of Subjects comes from the causative construction. The following reveal a crucial pattern:

- | | | |
|-----|--|-----------------------------------|
| 23. | <i>Nuwahe lemepja</i> | 'Nuwahe dies' |
| | Nuwahe dies | |
| 24. | <i>Nuwahe lemep-ka-po Tawalijana</i> | 'Tawalijana causes Nuwahe to die' |
| | Nuwahe die -caus Tawalijana | |
| 25. | <i>pampila titip-ka-poi eluwa-ja e-patenu-ja</i> | |
| | paper read-CAUS man -AGT teacher-AGT | |
| | 'The man caused the paper to be read by the teacher' | |
| 26. | <i>pampila titip-ka-poi e-patenu-ja eluwa-ja</i> | |
| | paper read CAUS teacher -AGT man -AGT | |
| | 'The teacher caused the paper to be read by the man' | |

Sentence 24 can only mean 'Tawalijana causes Nuwahe to die' because the causer of the action must immediately follow the verb (there are no formal markings to indicate Subject and Object). This is confirmed by the order of sentences 25 and 26. This evidence suggests that Wajana does indeed possess OVS as its basic order.

6. Conclusion

The preceding discussion leads, in its treatment of Wajana syntax, to the conclusion that Wajana has a basic order of OVS. This addition to the list of object-initial languages in the world supplements the work of Derbyshire and others who are engaged in research on this group of languages. Yet this paper has primarily examined Wajana in the context of typology and this yields one of the most important conclusions. Wajana patterns like other prominent SOV languages like Japanese and Turkish and therefore belongs to essentially the same classification.

With the addition, then, of OVS languages to the list of basic orders, we find that the division of the world's languages into two major types, VO and OV, is corroborated. This argues that syntactic typology is to be understood in terms of verbs and their objects. In other words, subjects are not good predictors of word order whereas the relation of objects to verbs is.

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A Possible Universal: A Dichotomy of Actance Constructions According to Categories of the Object

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1150
1. In this paper actance constructions are defined distributionally as pure grammatical relations between the predicate, i.e. the verb in verbal clauses (we will not here consider nominal predicates) and the main nominal members of the clause (actants). If we designate the translation equivalents, in any language, of the subject and the object in basic, unmarked sentences of standard West European languages, as X and Y respectively, any combination X Y V with its morphological and syntactic markers (such as case suffixes or prefixes, prepositions or postpositions, etc.), verbal markers of person, number etc. concord with X and/or Y, and word order, is an actance construction (cf. Lazard, BSLP 1978, fasc. 1, p. 49 sqq.).

If two different actance constructions are in use in the same language, depending on tense, aspect, person, definiteness, animacy, or whatever, we call this actance variation. This variation may consist in as little as the addition or suppression of a case marker, or it may involve a complete rearrangement of the whole construction: most types of what is usually called split ergativity are such rearrangements.

A cross-linguistic survey shows that actance variations may appear in correlation with three kinds of factors: verbal categories, either grammatical (tense, aspect) or lexical, nominal categories (definiteness, humanness, animacy) manifested either in actant X or actant Y, and communicative-intent structure (theme-rheme relations). Here we are concerned only with actance variations correlating with categories pertaining to actant Y, the object of familiar West European languages, primarily the categories of definiteness and humanness. These notions are taken in a general sense. We regard definiteness as a range of values, from properly definite to non-specific indefinite and further on to generic. The notion of humanness is scalar too: it is understood as including animacy and extending from human persons to inanimate amorphous substances.

2. Actance variations correlating with these categories of actant Y may obtain as different types of alternation:

— nominal markers vs. Ø: Turkish (accusative ending -i), Persian (post-position *râ*), Ossetic (genitive ending -i), Hindi (postposition *ko*), Aymara (post-position *ru*), Hebrew (preposition *et*), Spanish (preposition *a*);

— different nominal markers: Finnish (accusative vs. partitive);

- different conjugations: Hungarian ('objective' vs. 'subjective' conjugation);
- different diatheses: Tagalog (passive vs. active), Eskimo (ergative vs. 'antipassive');
- autonomous vs. incorporated Y: classical Nahuatl, Oceanic languages.

3. Definiteness and humanness, as extralinguistic scales of values, may give rise, in different individual languages, to different oppositions of grammatical relevance. The following may be mentioned:

— definite vs. indefinite (in the usual sense): Turkish, Persian, Hebrew, Hungarian. Ex.: Persian *ketâb-râ xândam* 'I read the book', *ketâb xândam* 'I read a book/books'.

— non-generic vs. generic: incorporating languages. Ex.: class. Nahuatl *ni-naca-cua* 'I eat flesh (do flesh-eating)', *ni-c-cua nacatl* 'I eat (some) flesh'.

— human vs. non-human: Hindi. Ex.: *mai us-ko dekh rahā hū* 'I see him', *mai vah dekh rahā hū* 'I see it'.

— pronouns and proper names vs. common nouns: in Oceanic languages pronouns and proper names are incorporated (differently from generic nouns), while (non-generic) common nouns are not.

— individual vs. mass: Finnish. Ex.: *näen talo-n* (acc.) 'I see a/the house', *juon maito-n* (acc.) 'I drink the milk', *juon maito-a* (partit.) 'I drink (some) milk'.

— 1st and 2nd persons vs. 3rd person: Dyirbal, Pashto (in this language a 1st or 2nd person Y is in the oblique case, but if it is a noun or a 3rd person pronoun, it is in the direct case).

The two oppositions first mentioned fall under the category of definiteness, and the other ones rather under the notion of humanness. But in fact these categories often interact with each other (and with other factors) in a subtle interplay. Since there is a fair amount of natural agreement between the scales of definiteness and humanness, it is possible, with some simplification, to unite them in the following overall scale.

pron. 1-2	pron. 3 prop. nam.	def. hum.	indef. non-hum.	mass	generic
A	B	C	D	E	

This scale is neither aprioristic nor intuitive, but is the result of empirical observations: its transitions (A, B, C, D, E) correspond to points critical for actance variation in existing languages, and its scalar progression conform to what is observed in those languages.

Point C appears as especially important: it is critical for many languages (Turkish, Persian, Ossetic, Hindi, Aymara, Spanish, Tagalog, Eskimo, Hebrew), and for rather different types of actance variation. But incorporating languages

have their crucial point in E, Finnish in D, Dyrbal and Pashto in A. Some languages have a double split (Oceanic languages in B and E).

4. An interesting conclusion arises from the comparison, in each individual language, of the actance constructions correlating respectively with the right part and the left part of the scale. We observe that 'rightside' constructions either are identical to, or have affinities with, uniactant (intransitive) constructions, while the 'leftside' ones have characteristic markers or syntactic features which keep them far removed from uniactant constructions. For instance, in Hungarian, the 'subjective' conjugation used with indefinite Y is nothing else but the conjugation of intransitive verbs. In Eskimo an indefinite Y enters the same construction as words expressing manner with intransitive verbs (unipersonal conjugation and use of the 'modal' case); it is remarkable that the same relationships obtain in Oceanic, in spite of the languages being of widely different types: e.g. in Fijian an indefinite (generic) Y is incorporated like adverbial terms. In Persian a verb with an indefinite Y (which has no marker) forms a syntactic unit (and often a lexical one as well) which behaves like a simple verb.

Thus what we observe in languages of widely different structures leads us to posit two types of biactant constructions as a general tendency. In one, the 'leftside' construction, actant Y is an autonomous, first rank member of the clause; its position is more or less free; it is more or less on a par with actant X, the other main nominal member of the clause. In the other type, the 'rightside' one, Y is in a somehow subordinate position; it is more or less tightly linked to the verb; it may be incorporated in it, or it is obligatorily or preferably placed in its near vicinity, tending to form a kind of compound with it. In other words, one type of clause is actually composed of three members; it has three poles: X, Y and V. The other one has, or tends to have, only two poles, X and YV, and in this respect resembles uniactant constructions:

'Leftside'
biact. constr.
X — Y — V

'Rightside'
biact. constr.
X — YV

Uniactant
construction
Z — V

The second type meets the traditional description of the clause as composed of NP + VP. Our analysis suggests that this traditional view is not wrong, but is not the whole truth either.

5. What is the scope of this conclusion? It is based on the empirical observation of a necessarily limited number of languages. Since these languages belong to rather different types, one may incline to assume that the tendency to distinguish two types of biactant construction, one with three poles (Y being more definite and/or human) and one with only two poles (Y being less definite and/or human) is a universal one. This hypothesis does not imply an unescapable

necessity for all languages to manifest this distinction overtly, but only a tendency which may yield different results. In particular the trend to coalesce Y and V might be counteracted by the morphological structure of the language. It would be worth investigating whether in such languages the trend can nevertheless be perceived in one form or other.

Another possibility is that that tendency is not universal, but characteristic of certain languages only. In that case it would define a linguistic type: the languages manifesting the tendency would have to be ranged under that type, in spite of their large differences in other respects. The observations made and the conclusions drawn therefrom would be none the less interesting; only, instead of pointing to a universal, they would be relevant to linguistic typology.

(See a more detailed version of this study in a volume edited by F. Plank, *Objects. Towards a theory of grammatical relations*, to be published in 1983 by Academic Press, London.)

The Present State of Linguistic Typology

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1152
1. *Basic concepts.* In order to forestall any misunderstandings, it may be useful to begin with some terminology. Linguistic typology might be taken in a very broad sense to include the typology of anything that falls within the field of linguistics, e.g. the typology of the word-classes of a language or the typology of possible phonological rules. I take linguistic typology in a narrower sense as typology of the shapes that a given domain of the language system takes in the languages of the world. The comprehension of such a domain can vary from a single category or construction to the whole language system.

Various authors, especially outside Europe, do not make a clear distinction between linguistic typology and language universals research (Greenberg 1974, Comrie 1981, Mallinson/Blake 1981). These two main subdisciplines of general comparative linguistics are complementary to each other, but clearly distinct. Universals research is the search for universals, typology is the search for types. Language universals are properties which all human languages have in common because they are constitutive of human language as such (Lehmann 1982, §5.). Linguistic types are alternative underlying patterns according to which a (given domain of the) language system may be structured. I will reduce the scope of the present paper by excluding from it universals research and concentrating on typology in the sense just defined.

Again mainly outside Europe, we frequently encounter the equation of typology with classification, of type with class (Moravcsik 1979, Anderson 1980, Mallinson/Blake 1981). Classification of linguistic phenomena and of languages is doubtless a useful activity; but it is not typology (Coseriu 1980:157-160; Ramat 1980:329). A type is not a class of entities, but a pattern underlying the form of such entities, a model or a plan in accordance with which they are constructed (Seiler 1979). If we relate linguistic phenomena or languages to the various types, they will fall into—overlapping!—classes; but this is secondary to typology.

The ultimate goal of linguistic typology is the establishment of holistic language types. When linguistic typology is explicitly aimed at this goal, it deserves the name 'language typology'. Attainment of the goal presupposes the discovery of connections among various structural properties, because it is the principles governing these connections which constitute the language type. Most typologists nowadays are pessimistic about the possibility of a finite set of holistic language types (see Lehmann (ed.) 1982). However, most of them do

carry out research which contributes to this end when they look for relationships among structural properties, for clusterings among diverse features. From this point of view, much recent work that has been done under such titles as 'typology of nominal modification' (Foley 1980), 'typology of the relative clause' (Lehmann 1979) or 'typology of passive constructions' (Givón 1981) is merely preparatory if it typologizes only a single subsystem and does not demonstrate correlations with other subsystems (cf. Yartseva 1979:279 f). By an inductive procedure which integrates ever more features into the cluster of correlations, we gradually approach the goal of the language type.

2. *Problems of present-day typology.* The central concern of present-day typologists is morphosyntactic typology. As for semantic typology, there is little, if any, activity; and there is not even a consensus among scholars as to what it should deal with (s. Lehmann (ed.) 1982). The fate of phonological typology is not much different, although here the task, viz. that of discovering types in the sound patterns of different languages, is clear in principle (cf. e.g. Hagege/Haudricourt 1978, ch. III). However, while there is currently much serious research in phonological universals, nobody appears to be interested in phonological types (s. Dressler 1979). The assumption prevails that phonological types are interesting only to the extent that they correlate with morphosyntactic types; and the majority of typologists are clearly pessimistic about this possibility. Perhaps some advance can be made if Trubetzkoy's (1931:163) claim that morphophonology is the best basis for language typology is examined afresh.

There is some theoretical justification for the central position of morphosyntactic typology. If linguistic typology is to stand up to its claim of reducing linguistic diversity to a manageable number of underlying patterns, it necessarily must approach linguistic structure at the level which displays the greatest differences cross-linguistically. While both semantics and phonology are closer to universal substance, the combination of sound and meaning constitutes the area most intrinsic to human languages, in which they differ most. This combination, insofar as it is subject to rules, is brought about in morphosyntax.

This reasoning provides us with an important principle of morphosyntactic typology. It is the patterns of association of sound with meaning which must constitute the central concern of morphosyntactic typology. To give an example: One may ask, on the one hand, whether a language possesses the category of number, or on the other hand, whether it possesses suffixes on the verb. Neither of these questions is of immediate typological relevance, since they are concerned exclusively either with the content or the expression of grammatical categories. What potentially contributes to the language type is the association of this content with this expression, viz. whether a language expresses number (differentiated according to certain subcategories) by verbal suffixes (of a certain kind).

Much current typology falls short of this requirement. For instance, the

typology of the fundamental relations, i.e. of the accusative, ergative and active types (proposed by G. Klimov (1979) and others), is content-oriented, i.e. it more or less explicitly disregards the ways in which these relations are expressed. On the other hand, traditional morphological typology, establishing types such as the isolating, agglutinative, flexional etc., is exclusively form-oriented; and so are modern syntactic versions of it which ask, for example, whether a language has 'heavy morphology' and 'free word order' or, on the contrary, 'little morphology' and 'fixed word order' (e.g. Vennemann 1974). Such models fail to ask the essential question of what is expressed by such structural means. A basic tenet of structural linguistics is that form covaries with meaning, that structural differences reflect semantic or functional differences. This covariation of meaning and expression has the consequence that one cannot find a strict equivalent to a grammatical category of a given language in another language. Some typologists have been reluctant to acknowledge this truth, because it seemed to render cross-linguistic comparison and, thus, typology impossible (cf. Yartseva 1979:279). Quite the opposite is true. Typology—and indeed structural linguistics—would be impossible if meaning and expression could vary independently of each other. The fact that they covary in regular ways constitutes the basis of morphosyntactic typology.

All partial typologies center around a specific domain of the language system. The domain currently most favored is constituted by the verb-actant, or predicate-argument, relations. Basic order typology deals with the sequential order to be observed in this realm, especially among subject, verb and object (Antinucci 1977, Lehmann, W. 1978). The typology of fundamental relations instead focusses on the syncretisms of case roles in the fundamental syntactic relations of the accusative vs. nominative, the ergative vs. absolutive or the active vs. inactive (Lazard 1978, Klimov 1979, Plank (ed.) 1979). T. Milewski's (1950) all too neglected typology of the concentric vs. excentric constructions is based upon the expression of verb-actant relations either by verbal agreement affixes or by nominal case markers. It is astonishing and disappointing to see that these three approaches, which actually deal with the same subject matter under different aspects, have remained almost totally disconnected, although the most detached observer immediately perceives that they can only do justice to it if they are combined (first attempts in Pullum 1977, Bossong 1980 and Mallinson/Blake 1981, §3.4.).

All existing typologies are partial typologies, and therefore there cannot conceivably be any competition among them. For many of them, however, the claim has been raised that they are valid as language typologies. For instance, the typology of topic-prominent vs. subject-prominent languages (Li/Thompson 1976), or of role-dominated vs. reference-dominated languages (Van Valin/Foley 1980), have been offered as alternatives to each other and to basic order typology. Such claims illustrate the greatest danger to which linguistic typology is exposed, namely reductionism. Building a whole language typology upon a single feature such as main constituent order, or topic-prominence vs.

subject-prominence, or whatever, presupposes that everything else in the language system depends on, or correlates with, this one feature. This has not been, and will not be, demonstrated for any of the typologies proposed so far. Beside the methodological defect of reductionism, such proposals involve two essential theoretical errors. The first is the assumption that there is a single structural property, or grammatical domain, which is basic to the language system and, therefore, to typology. In reality, language is a balanced combination of various subsystems (cf. W. Dressler's polycentristic model); and the higher their level of complexity, the looser the connections among them become. While there are clear-cut implicative relations among the members of a morphological paradigm, there is no more than an equilibrium, a relation of mutual preference (Skalička 1966), among the major parts of the whole grammar. The second false assumption is that types are constituted by structural properties. If this were so, types would indeed be equivalent to classes, since a class may be defined by a property common to its members. In reality, a type is a set of interrelated principles which underly the structuring of language systems. Only if we accept this can we avoid admitting that every typology is as good as any other.

Reductionism goes hand in hand with oversimplification. We have now seen almost twenty years of basic order typology, and still some fundamental problems are constantly glossed over. What is the basic order if there are variant orders or different orders under different contextual conditions? How is basic order typology relevant to a language with free word order? (Mallinson & Blake (1981, ch. 3) address themselves to such problems.) What is the basis for equating subject, verb, object, genitive attribute, adposition etc. of English with similar categories in any other language? Does the difference between accusative, ergative and active construction, between alienable and inalienable possession, between adpositions of nominal and of adverbial nature really have no relevance here? On the other hand, typologies of case systems are being offered which, on closer inspection, boil down to typologies of the fundamental relations (Dixon 1979:71; Comrie 1981, ch. 6). That is, only the expression of the agent and patient relations is analyzed, as if these were not embedded, in every language, in the whole network of semant syntactic relations which comprise, in addition, the indirect object, the locative complement and several more. Moreover, several authors (Moravcsik 1978, Comrie 1981 Shibatani 1982) are still dealing with a dichotomy of accusative vs. ergative construction, as if the existence of at least one more construction type, the active one, had not been demonstrated by G. Klimov (1972) ten years ago. Linguistic typology is only possible if we are constantly aware of the connections which a given property has with other parts of the language system.

3. *Suggestions for future research.* Efforts in typological work have been stepped up in the last few years, and some promising results and interesting suggestions have been put forward (summarized in Comrie 1981 and Mallinson/

Blake 1981). Despite such achievements, the present state of linguistic typology gives no occasion for contentment. The field is split up, there is not even agreement on what linguistic typology is, let alone a common theoretical framework or methodology. The very first thing we need is more cooperation among typologists. The present situation in which linguists from the USA, of Western and of Eastern Europe, of Japan, Australia or whatever place remain largely ignorant of what is being done in other parts of the world is detrimental to the discipline. Linguistic typology is an exacting task which demands empirical analyses and inductive generalizations on a vast scale. Therefore typologists must unite their efforts and try to find a common denominator (Dezső 1979). (A first attempt has been made by the East-West Group on Typology, which has assembled typologists from the USA, from Western and Eastern Europe in annual meetings since 1979; see *ALHung* 29 (3/4), 1979).

Furthermore, linguistic typology is possible only if comprehensive and uniform grammatical descriptions are available. In this respect, the discontinuation of the *Lingua Descriptive Studies* project (1979–1982) is regrettable, because despite any weaknesses which one might blame on it, it was a first and valuable attempt to provide such grammars. Finally, typology must co-operate with universals research in the following way (cf. Seiler 1979): Universals research must establish the functional domains or dimensions of the language system and collate the various structures by which languages represent each dimension. For example, the dimension of possession (Seiler 1981) comprises the various possessive affixes, genitive attributes and 'have'- and 'be'-constructions of alienable and inalienable possession, and a variety of other structural means. Then the laws of covariation of function and expression as well as the limits of structural variation within the domains must be formulated (cf. Keenan 1978). Here linguistic typology takes over. It observes how each language makes its selection among the structural means available within each dimension, and how this choice correlates with the choices made on other dimensions, for instance those of determination or local orientation. None of the dimensions has claims to more basicness than any other. The principles which govern these choices and their correlation are typological principles, they constitute language types. As more dimensions are gradually integrated into the picture, it becomes more complex, and from a collection of linguistic types we arrive at holistic language types. This is what we want, in the long run: not a variety of competing typologies, but one comprehensive typology.

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Les tournures passives en bourouchaski*

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1. Introduction

1.1 Dans son ouvrage sur l'ergatif Cl. Tchekhoff (1978:40) élève des doutes sur l'existence d'une voix passive dans les langues à construction ergative. "Lorsqu'il y a voix dans un schéma ergatif, c'est dans ce sens que doit se faire l'opposition du patient à l'auteur, et non dans le sens contraire comme dans une construction accusative." Nous pensons que le bourouchaski offre un exemple intéressant allant à l'encontre de cette théorie. Notre enquête menée à Yasin au cours de trois séjours, respectivement en 1975, 1978 et 1979, porte essentiellement sur le bourouchaski parlé dans cette vallée. Nous avons pu vérifier la pertinence de nos conclusions avec la langue du Hounza en 1982, grâce à l'aide de M. Nasir Oudin, qui a travaillé à Montréal avec nous sur ce sujet. Toutefois la plupart de nos exemples sera empruntée au premier dialecte.

1.2 Nous avons cru pouvoir distinguer de prime abord deux types de passif en bourouchaski. Le premier se caractérisait par un agent marqué en *-um/-cum* et le deuxième par un agent marqué en *-e*. A l'analyse, il est apparu que ces deux procédés relevaient d'une même organisation syntaxique et étaient en distribution complémentaire.

2. La tournure passive avec agent en *-um/-cum*

2.1 En bourouchaski, il est aisé de constater qu'un verbe transitif peut devenir intransitif; le verbe alors s'accorde avec l'ancien objet, qui conserve le cas absolu. Cette réduction entraîne, le plus souvent, l'élimination de l'agent:

- | | | | |
|-----|----------------------------|-----------|-----------------------|
| (1) | mo gúse | mená | gurgúnu |
| | la femme-Erg. | mena-Abs. | a moulu |
| | 'La femme a moulu le ména' | | |
| (2) | mená | dugúrguni | 'Le ména a été moulu' |
| | mena-Abs. | a-moulu | |

Cependant, il est des cas où l'on peut exprimer l'agent, qui devient alors un complément oblique à l'ablatif:

* Nous tenons à remercier le Conseil de recherches en sciences humaines du Canada, qui a subventionné, pour une grande part, nos recherches sur le bourouchaski.

- (3) ne híre biánc (u)kháči
l' homme-Erg. vaches-Abs. enferma
' L'homme a enrermé les vaches '
- (4) biánc dukháčičum bién
vaches-Abs. enrermées sont ' Les vaches sont enrermées '
- (5) jácum biánc dukháčičum bién
moi-Abl. vaches-Abs. enrermées sont
' Les vaches sont enrermées per moi '

2.2 De telles tournures sont soumises à une contrainte: l'agent doit être animé. Elles sont, d'autre part, moins usitées que la tournure à l'ergatif. On peut faire un parallèle avec le rapport qui, en français, lie le passif à l'actif. La phrase *la rue a été traversée par le piéton* est grammaticalement correcte, mais reste artificielle. On préférera la tournure active *le piéton traverse la rue*. En revanche, une phrase comme *j'ai été dévoré par les moustiques* ne soulève par les mêmes difficultés.

2.3 L'importance de la tournure à l'ablatif est capitale. Elle permet de lever un doute important sur l'existence d'un passif en bourouchaski. La présence d'un cas distinct de celui du sujet normal d'un verbe intransitif pour marquer l'agent prouve clairement que nous n'avons pas affaire à des constructions absolues ou à sujet non exprimé, comme on peut argumenter pour le dyirbal ou pour l'avar. Dans cette langue, d'ailleurs, une phrase comme (6), littéralement "l'âne vient du fait de l'homme", ne s'oppose à aucune autre tournure simple signifiant spécifiquement "l'homme amène l'âne". Tel n'est pas le cas en bourouchaski où l'on peut opposer à la tournure passive (7) la tournure ergative-transitive (8).

- (6) či-yas hama bač'ula ' L'homme amène l'âne '
- homme-Erg. âne-Abs. vient
- (7) hírcum bal duxáurti
homme-Abl. mur-Abs. est éraflé
' Le mur est éraflé par l'homme '
- (8) ne híre bal xáurti ' L'homme a éraflé le mur '
- l' homme-Erg. mur-Abs. érafla

Le bourouchaski du Hounza ne procède pas différemment:

- (9) úwe báalt súcáan ' Ils mangent des pommes '
- ils-Erg. pommes-Abs. mangent
- (10) báalt úwacum dusúciyen
pommes-Abs. eux-Abl. sont mangées
' Des pommes sont mangées par eux '

3. La tournure passive avec agent en *-e*.

3.1 Le bourouchaski connaît également une tournure passive où l'agent est à un cas oblique en *-e*, et non en *-um/-cum*:

- (11) garúm céle jā párcimu dóskurušum duá
chaude eau-Erg. moi-Gén. chapeaux-Abs. rétréci est
'L'eau chaude rétrécit mes chapeaux'
- (12) garúm céle jā párcimu dóskurušum bién
chaude eau-Erg. moi-Gén. chapeaux-Abs. rétréci sont
'Mes chapeaux rétrécissent sous l'effet de l'eau chaude'

Cf. Hounza:

- (13) tíše ciír ósqorčila
vent-Erg. chèvres-Abs. fait-sombrer
'Le vent fait sombrer les chèvres'
- (14) tíše ciír ósqorčiyen
vent-Erg. chèvres-Abs. sombrent
'Les chèvres sombrent sous l'effet du vent'

3.2 Cette construction pose un problème délicat. Que le verbe s'accorde avec l'absolutif, cela ne soulève aucune difficulté, puisque, dans les langues à construction ergative, ce cas peut marquer l'objet dans les tournures transitives et le sujet dans les tournures intransitives. En revanche, en bourouchaski, le cas ergatif n'est clairement attesté que comme marque du sujet. Faut-il, dans ces conditions, considérer la forme en *-e* dans les exemples passifs, comme attestant un autre usage de ce cas? On sait qu'en bourouchaski, la marque de l'ergatif est homonymique de celle du génitif, sauf pour les féminins. Ex.: Erg.-Gén m, x, y *híre* 'homme', *cigíre* 'chèvre', *cíle* 'eau'; Erg. f *gúse* 'femme', Gén. *gúsmo* 'femme'. Malheureusement, il a été impossible de recueillir un passif en *-e* avec un agent féminin. Cela est dû à certaines contraintes que nous envisagerons plus loin (3.4). L'existence d'une telle tournure aurait permis de lever l'ambiguïté qui pèse sur la forme en *-e*.

3.3 Cette tournure passive en *-e* est soumise à certaines contraintes. Alors que nous avons pu relever en hounza pour un même verbe trois tournures possibles, il semble que pour certains informateurs de Yasin, la tournure dite passive ne puisse alterner avec la tournure active. L'une s'impose à l'exclusion de l'autre. Ainsi, un informateur de Darkot, accepte seulement la phrase (12) et refuse (11). D'autres informateurs, cependant, considèrent celle-ci comme correcte. Si l'on s'appuie sur l'enseignement du hounza, on peut penser que l'informateur a été frappé par l'évidence de la tournure passive au point de refuser la tournure active. D'ailleurs, un de nos informateurs les plus rigoureux a accepté à plusieurs reprises les deux tournures.

3.4 En revanche, cette tournure passive est indubitablement soumise à des contraintes sémantiques. Le bourouchaski distingue quatre classes de noms: les humains masculins (m), les humains féminins (f), les animés non humains et les éléments dénombrables (x), les éléments indénumérables et les abstraits (y). Il y a lieu de réorganiser ces classes pour tenir compte des restrictions à la tournure passive de la façon suivante: animés humains (h), animés non humains (x[+animé]), inanimés et abstraits (x[-animé] et y). Lorsque le sujet ou l'objet est h, cette tournure passive est souvent rejetée (cf. 15 et 16). Il en va de même lorsque le sujet est x animé (cf. 17). En revanche, lorsque x animé est objet et que x inanimé et y sont sujet ou objet, la tournure passive est admise, voire requise.

- | | | | | |
|------|---|--------------|------------|------------|
| (15) | *hurí | malín | déγoljačum | bicá |
| | hommes-Erg. | champs-Abs. | brûlés | sont |
| | 'Les champs sont brûlés par les hommes' | | | |
| (16) | *harálte | daḱtár | déγoskinum | bái |
| | pluie-Erg. | docteur-Abs. | retardé | est |
| | 'Le docteur a été retardé par la pluie' | | | |
| (17) | *cigíra | béliše | cel | ménium duá |
| | chèvre-Erg. | mouton-Gén | eau-Abs. | bue est |
| | 'L'eau du mouton a été bue par les chèvres' | | | |

Cependant d'assez nombreux exemples relevés contreviennent à cette règle; il est plus juste de parler de tendance.

4. Conclusion

4.1 Il apparaît clairement, au terme de cette étude, que la distinction nettement établie au cours des enquêtes entre le passif en *-um/cum* et le passif en *-e* ne se justifie pas. Nous n'avons pas affaire à deux tournures différentes, mais à deux tournures complémentaires, qui relèvent d'un même système. Le marqueur est en *-um/-cum* lorsque l'agent est animé, et en *-e* lorsqu'il est inanimé. Le problème délicat à résoudre porte sur la nature du patient. Le bourouchaski recourt difficilement à la tournure passive quand le patient est h. Dans ce cas, les informateurs hésitent longuement et tendent à se prononcer négativement. Une phrase comme (18) est difficile:

- | | | | | |
|------|--|-------------|------------|------|
| (18) | *sā | hurí | dóγoljačum | ban |
| | soleil-Erg. | hommes-Abs. | brûlés | sont |
| | 'Les hommes sont brûlés par le soleil' | | | |

Ce malaise noté chez nos informateurs permet de noter un trou dans le système passif du bourouchaski.

4.2 Le bourouchaski, qui possède une construction ergative connaît donc une tournure passive bien identifiable et systématisable. Certes, la passivation en

bourouchaski est peut-être moins souple que dans d'autres langues, car elle est liée à des contraintes sémantiques assez strictes. Elle est toutefois suffisamment productive pour qu'on puisse en tirer certains enseignements intéressants à la typologie des langues. Parmi ceux-ci, nous espérons que l'analyse proposée permet de mettre en cause, si besoin est, une fois pour toutes la théorie de la valeur passive du verbe dans les langues à construction ergative.

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A Re-Definition of 'Ergative' and 'Accusative'

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1. INTRODUCTION In accordance with the usual practice in case-marking typology, I will use the three labels A, O, and S for referring to certain NPs:

- | | | |
|---|---|--------------|
| A: agent, perceiver, experiencer, possessor, etc. | } | of two-place |
| O: patient, perceived, experienced, possessed, etc. | | predication |
| S: sole participant of one-place predication. | | |

'Ergative (-absolutive) pattern', 'ergative (-absolutive) construction', and 'ergative and absolutive cases' are often defined as follows:

ERGATIVE-ABSOLUTIVE	{	pattern : A \neq S = O
	{	case : <u>ERG</u> <u>ABS</u> ¹
	{	construction: A-ERG O-ABS Verb.

Similarly, '(nominative-) accusative pattern', '(nominative-) accusative construction', and 'nominative and accusative cases' are often defined as follows:

NOMINATIVE-ACCUSATIVE	{	pattern : A = S \neq O
	{	case : <u>NOM</u> <u>ACC</u>
	{	construction: A-NOM O-ACC Verb.

As will be demonstrated below, these definitions lack a degree of precision, and consequently, have sometimes been used misleadingly. Therefore, more precise definitions are necessary.

2. MISLEADING USE OF THE TERM 'ERGATIVE' In many languages—in both so-called 'ergative languages' and 'accusative languages'—expressions of perception, knowledge, feeling, possession, ability, etc. involve case-frames such as DAT-ABS, GEN-ABS, LOC-ABS, or the like; or DAT-NOM, GEN-NOM, LOC-NOM, and so on. (For examples, see Table 1.) They have sometimes been regarded as instances of the ergative pattern, and consequently these so-called 'accusative languages', e.g. Russian and Japanese, have been suggested to be ergative/accusative-mixed, containing both the accusative and the ergative patterns. Thus, involving the DAT-NOM case-frame, for instance:

accusative pattern

$$\underbrace{A = S}_{\text{NOM}} \neq \underbrace{O}_{\text{ACC}}$$

ergative pattern (?)

$$\underbrace{A \neq S}_{\text{DAT}} = \underbrace{O}_{\text{NOM}}$$

1) The abbreviations used are as follows: ABS absolutive; ACC accusative; APU apudessive; DAT dative; ERG ergative; GEN genitive; LOC locative; NOM nominative; POSS possessive.

Table 1-1. Case-frames in eleven selected languages.

type	1		2	3
meaning	direct effect on the O		perception	pursuit
examples	kill, break, bend, hit, shoot, eat		see, look, hear, listen, smell, find	search, wait
subtype	1A	2B		
meaning	causing change	causing no change		
examples	kill, break	hit, shoot	NOM see ACC NOM look at O NOM listen to O	NOM await ACC NOM wait for O
English	NOM kill ACC	NOM hit ACC NOM hit at O NOM tread on O		
Japanese	NOM-ACC	NOM-ACC NOM-DAT	NOM-ACC NOM-DAT DAT-NOM	NOM-ACC
Tibetan	ERG-ABS	ERG-DAT/LOC	ERG-ABS ERG-DAT/LOC	ERG-ABS ERG-DAT/LOC
Avar	ERG-ABS		LOC-ABS	ERG-ABS ABS-APU
Tongan	ERG-ABS		ERG-ABS ABS-DAT ABS-LOC	ERG-ABS ABS-DAT
Samoa	ERG-ABS		ERG-ABS ABS-DAT ABS-LOC	ERS-ABS
Djaru	ERG-ABS		ERG-ABS	ERG-DAT ABS-DAT
Warrungu	ERG-ABS		ERG-ABS	ERG-ABS ABS-DAT
Guugu Yimidhirr	ERG-ABS		ERG-ABS	ERG-ABS
Basque	ERG-ABS		ERG-ABS	ERG-ABS
Eskimo	ERG-ABS		ERG-ABS	ERG-ABS

Table 1-2. Case-frames in eleven selected languages.

type	4	5	6	7
meaning	knowledge	feeling	possession	ability
examples	know, understand, remember, forget	love, like, want, need, fear	posses, own	capable
subtype				
meaning				
examples				
English	NOM know ACC NOM aware of O	NOM like ACC NOM fond of O	NOM own ACC	NOM capable of O
Japanese	NOM-ACC DAT-NOM	NOM-ACC NOM-DAT NOM-NOM DAT-NOM	NOM-ACC DAT-NOM	DAT-NOM
Tibetan	ERG-ABS	ERG-ABS ABS-DAT/LOC DAT/LOC-ABS	DAT/LOC-ABS	
Avar	LOC-ABS	DAT-ABS	GEN-ABS	
Tongan	ERG-ABS ABS-DAT DAT-ABS LOC-ABS	ERG-ABS ABS-DAT ABS-LOC	ABS-ABS	
Samoan	ERG-ABS ABS-DAT LOC-ABS	ERG-ABS ABS-DAT ABS-LOC	DAT-ABS	
Djaru	ERG-ABS ABS-DAT	ERG-ABS ABS-DAT ABS-LOC	ERG-ABS	
Warrungu	ABS-DAT	ABS-DAT	ERG-ABS	
Guugu Yimidhirr	ABS-DAT	ABS-DAT	DAT-ABS POSS-ABS	
Basque	ERG-ABS	ERG-ABS	ERG-ABS	
Eskimo	ERG-ABS	ERG-ABS	ERG-ABS	

However, this view is misleading. For example, according to this view, we would have as many as four distinct ergative patterns in Avar, N.E. Caucasus (see Table 1):

involving action verbs	involving perception verbs	involving feeling verbs	involving possession verb
$A \neq S = O$	$A \neq S = O$	$A \neq S = O$	$A \neq S = O$
ERG ABS	LOC ABS	DAT ABS	GEN ABS

But, according to the traditionally accepted view of Avar grammar, only its action verbs constitute the ergative pattern/constructions, and other three types of verbs do not.

The reason for the misleading usage of the term 'ergative' is that the type of the verbs employed in its application was not held constant. Thus, in order for this term to be crosslinguistically valid, we need a re-definition of the term whereby the type of the verbs employed is fixed.

3. RE-DEFINITION OF 'ERGATIVE' As necessary (if not sufficient) conditions, I propose that **ERGATIVITY** involve (i) not only the $A \neq S = O$ pattern, (ii) but also true transitive verbs.

It is first necessary, in terms of case-marking, to define true transitive verbs. Table 1 shows a 'transitivity scale' of two-place predicates/verbs. The transitive case-frame—ERG-ABS or NOM-ACC—is more likely to occur as we move towards the left end, and in Subtype-1A only the transitive case-frame occurs (in every language investigated). This suggests that (among various two-place verbs) the true transitive verbs are Subtype-1A verbs; they describe activities that directly impinge on the O and cause a change in it. (For further details of this 'transitivity scale', see Tsunoda 1981).

Under this proposal, in Avar, only the action verbs—which include Subtype-1A verbs—constitute the ergative pattern/constructions, and other types of verbs do not. (This complies with the traditional view.) Furthermore, those above-discussed 'accusative languages' which are alleged to be ergative/accusative-mixed, are not in fact mixed; they lack the ergative pattern.

4. MISLEADING USE OF THE TERM 'ACCUSATIVE' Tongan and Samoan have the following two types of two-place verbs:

two-place	canonical transitive:	Verb	<i>e</i>	A	Ø	O	(ERG-ABS)
	middle	:	{	Verb	Ø	A	<i>i</i> O (ABS-LOC)
		:	{	Verb	Ø	A	<i>ki</i> O (ABS-DAT)
one-place	:	Verb	Ø	S			(ABS)

Canonical transitive verbs include Type-1, while middle verbs correspond to other types, but not Type-1; see Table 1. It has sometimes been suggested that

(i) middle verbs constitute the accusative pattern and constructions, and (ii) consequently Tongan and Samoan are ergative/accusative-mixed. Thus:

involving canonical transitive verbs

$$A \neq S = O$$

$\underbrace{e}_{\text{ERG}} \quad \underbrace{\emptyset \quad \emptyset}_{\text{ABS}}$

ERG ABS

involving middle verbs

$$A = S \neq O$$

$\underbrace{\emptyset \quad \emptyset}_{\text{NOM} (?)}$ $\underbrace{i \text{ 'at' or } ki \text{ 'to'}}_{\text{ACC} (?)}$

NOM (?) ACC (?)

However, this view is misleading. For example, according to this view, English would have numerous distinct accusative patterns. To give just four examples from English:

involving *kill*

$$A = S \neq O$$

$\underbrace{\text{NOM}} \quad \underbrace{\text{ACC}}$

involving *look*

$$A = S \neq O$$

$\underbrace{\text{NOM}} \quad \underbrace{at}$

involving *listen*

$$A = S \neq O$$

$\underbrace{\text{NOM}} \quad \underbrace{to}$

involving *fond*

$$A = S \neq O$$

$\underbrace{\text{NOM}} \quad \underbrace{of}$

But, according to the traditionally accepted view of English grammar, among the four verbs/predicates given above, only *kill* constitutes the accusative pattern/construction, and others do not.

5. RE-DEFINITION OF 'ACCUSATIVE' As necessary (if not sufficient) conditions, I propose that **ACCUSATIVITY** involve (i) not only the $A=S \neq O$ pattern, (ii) but also true transitive verbs, i.e. Subtype-1A verbs.

Under this proposal, English has only one accusative pattern, involving Subtype-1A verbs (among others), e.g. *kill*. (This complies with the traditional view.) Furthermore, Tongan and Samoan are not ergative/accusative-mixed; they lack the accusative pattern.

6. CONCLUSION Employment of true transitive verbs (i.e. Subtype-1A verbs) in the definition of 'ergative' and 'accusative' enables a more controlled crosslinguistic comparison of case-marking patterns. In addition, according to this proposal, some languages which are allegedly ergative/accusative-mixed, are not in fact mixed.

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Toward Universal Principles of Word Formation: A Look at Antonyms

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The question of whether the vocabulary of a language is structured on universal principles has perhaps found its most dramatic affirmative answer in the work of Berlin and his colleagues studying the distribution of basic color terms in the world. In the present research addressing this question,¹⁾ the domain of investigation is not a semantic field such as color terms but rather a set of antonymous expressions, or 'opposites', which range over many different semantic fields. The two studies reported here are based on data from 31 dialects representing 29 languages.²⁾ One is an exploration of a suggestive comment in Greenberg (1966) about formal markedness and its correlates; the other is an investigation of polysemy. Each study yields a universal principle of word-formation. We first consider markedness.

It is argued in Greenberg (1966) that "zero expression" correlates with "contextual neutralization" in a number of linguistic structures. "Zero expression" refers to the fact that one member of an opposed pair of expressions may be formally unmarked with respect to one pole of that opposition. For example, *author* (vs. *authoress*) is formally unmarked for maleness, and thus exhibits zero expression. "Contextual neutralization" refers to the fact that a given opposition in meaning may be neutralized in certain contexts; for example, the male-female opposition is neutralized in the sentence *I know the author, not the editor*. The claim that zero expression is correlated with contextual neutralization is exemplified by the fact that in the neutralizing context given above the unmarked form *author*, not the marked form *authoress*, is the form that appears. Greenberg suggests in a brief discussion (p. 52) of adjectives that this claim is also instantiated by at least a few pairs of antonymous adjectives. The proposition that, in general, it is the unmarked member of a pair of adjectives which appears in a neutralizing context, which has apparently been generally accepted, has not been systematically documented through crosslinguistic research. The findings of the present studies provide some evidence for such a general claim about adjective pairs.

1) This paper is a highly condensed version of the paper presented at the Congress. The research reported here was supported by a grant from The University of Wisconsin-Milwaukee Graduate School in 1979 and by the UWM ESL Program in 1981. I am grateful to Edith Moravcsik and Fred Eckman for helpful discussions.

2) About ten distinct language families are represented; 18 of the languages are non-Indo-European, the remainder Indo-European.

Examination of data about 34 pairs of antonymous expressions in each language revealed that, with two exceptions, across languages, the formally unmarked member of a pair of antonymous expressions always expresses the same pole of the opposition, if the two expressions are morphologically related such that one is derived from the other by the addition of a single negative. Thus, for example, for the opposition RIPE-UNRIPE the formally unmarked member is the one for the meaning RIPE. (Cf. *Gegbè sɪ* RIPE, *mù sɪ* UNRIPE: Yoruba *pɔn* RIPE, *ko pɔn* or *aipɔn* UNRIPE.) This holds regardless of whether the derived expression is a single word or a phrase (Cf. the Yoruba example above). The set of attested bases for derivation (i.e., the formally unmarked member of a pair) is listed in the first two columns of the table in (1).

By Greenberg's hypothesis, the forms corresponding to the meanings listed in the Bases Attested columns in (1) should be the ones that appear in a neutralizing context. A partial test of this was conducted in a survey of 56 native English speakers which sought their judgments of the neutrality of members of pairs of antonyms in English. Subjects were asked to determine if either, both, or neither of the members of each pair of antonyms could be used in a neutral sense in the neutralizing context *How ... is X?* By "used in a neutral sense" is meant that the word can refer to the whole dimension encoded by the antonym pair without implying that X is at one pole; for example, in *How tall is X?*, *tall* does not imply that X is tall, but in *How short is X?*, *short* does imply that X is short. Thus *tall* is neutral and *short* is not. The meanings chosen as neutral by this test are listed in the last two columns in the table in (1):

1. BASES ATTESTED	BASES ATTESTED	NEUTRAL MEANINGS	NEUTRAL MEANINGS
ABUNDANT	RICH	ABUNDANT	RICH
BRIGHT	RIPE	BRIGHT	RIPE
CLEAR	SHARP	CLEAR	SHARP
COOKED	SMOOTH	COOKED	SMOOTH
DEEP	STRONG	DEEP	STRONG
EXPENSIVE	SWEET	EXPENSIVE	SWEET
FAST	TALL	FAST	TALL
FRESH	TRUE	FRESH	TRUE
GOOD	WIDE	GOOD	WIDE
HAPPY	*DIRTY	HAPPY	CLEAN
HARD	*CROOKED	HARD	STRAIGHT
HEAVY	DIFFICULT	HEAVY	DIFFICULT
LONG	*EASY	LONG	—
LOUD	LIGHT	LOUD	LIGHT
OLD (vs. YOUNG)	*DARK	OLD (vs. YOUNG)	—
PRETTY~HANDSOME		PRETTY~HANDSOME	MANY
		••CLEAN	••BIG
		••INDUSTRIOUS	••HIGH
		••WHITE?	••OLD (vs. NEW)

• Meanings were not judged to be neutral

•• No instances found in the crosslinguistic study of these or their antonyms as bases for derivation.

In all but two cases (LIGHT, WHITE) there was more than 50% agreement on the neutrality of a given form, and in most instances there was more than 90% agreement. Since the list of Neutral Meanings correlates closely with the list of Bases Attested, the results of the test confirm the hypothesis.

Based on these results, a universal set of neutral or unmarked meanings is proposed here, namely the set of meanings listed under Neutral Meanings in (1), and a universal principle of word formation is formulated:

Principle of Antonym Formation

Of the antonymous meaning pair A/B, if A (but not B) is neutral, and if a language forms antonyms through the addition of a single negative element to the base, then either

- 1) A is the base; OR
- 2) If B is the base, the language must also have another non-derived form which means A.

Case 1) is exemplified by the RIPE/UNRIPE data from Gègbě and Yoruba given earlier. Case 2) accounts for the few cases where the non-neutral meaning is the basis for derivation, as in Gègbě *mú ñána*, DIFFICULT, which is derived from non-neutral *ñána*, EASY. In these cases, the language has another non-derived word for the neutral meaning; thus Gègbě also has non-derived *sêú* for DIFFICULT. This principle implies that for neutral meanings an implicational relation holds, namely, that for a given neutral meaning, if a language has a formally marked expression for it, the language will also have a formally unmarked expression for that meaning.

We turn now to the study of polysemy. The question in this case is whether the frequent polysemy of words in languages is systematic across languages or random. For example, in English, the word *hard* can mean FIRM (*hard* as opposed to *soft*) or it can mean DIFFICULT (*hard* as opposed to *easy*). Cross-linguistically, do the same pairs of meanings become encoded in a single form? That is, for example, do other languages also use a single form for the pair of meanings FIRM and DIFFICULT just as English uses *hard*?

The answer seems to be yes. Though the numbers are small, the *kinds* of meanings which are encoded by a single form are significant. Listed in (2) are the instances of polysemy found in the data examined.

2. *Attestations of polysemy*

(NEUTRAL)	(NON-NEUTRAL)	(NEUTRAL)	(NON-NEUTRAL)
BIG/OLD	SMALL/YOUNG	HEAVY DIFFICULT	LIGHTWEIGHT EASY
BIG/WIDE	SMALL/NARROW	LONG DEEP	SHORT SHALLOW
BRIGHT/CLEAR	DIM/HAZY	LONG/WIDE	SHORT NARROW
CLEAN/CLEAR	LOUD BIG	SOFT SMALL
CLEAN/WHITE	DIRTY/BLACK	LOUD HIGH	SOFT LOW
LIGHT/WHITE	DARK/BLACK	LOUD HALL	SOFT SHORT
GOOD/PRETTY	BAD/UGLY	RICH ABUNDANT
HARD/DIFFICULT	SOFT EASY	RIPE COOKED	UNRIPE UNCOOKED

.....	SOFT/SLOW	SWEET/PRETTY~
.....	SOFT/LIGHTWEIGHT	HANDSOME	
HARD/LOUD	SOFT/SOFT	TALL/BIG	SHORT/SMALL
HARD/STRONG	SOFT/WEAK	TALL/HIGH	SHORT/LOW
		LONG/HALL	SHORT/SHORT

Counterexamples?: HARD/ROUGH; SOFT (vs. HARD)/SMOOTH; SOFT (vs. LOUD)/SMOOTH

Each pair of words separated by a slash represents a pair of meanings which are expressed by a single form in some language. In Gègbè for example, the word for BIG in some contexts is the same as the word for OLD.

What is significant about the data is that in all but three instances the meanings encoded by the same forms are either *both* neutral or *both* non-neutral (in the sense of the neutrality test reported on earlier). Based on this finding we formulate another universal principle of word formation:

Principle of polysemy

Of two pairs of antonym relations A/B and C/D where only one member of each pair is neutral, if a language encodes a member of A/B by the same form as a member of C/D, then the two meanings so encoded must *both* be neutral with respect to their antonymic mates, or *both* be *non-neutral* with respect to their antonymic mates.

Thus, for example, of the pairs RIPE/UNRIPE and COOKED/UNCOOKED, if a language chooses to use the same form to represent more than one meaning, this principle would allow the language to use the form for RIPE to mean COOKED as well, since both RIPE and COOKED are neutral with respect to their antonymic mates UNRIPE and UNCOOKED. But it would exclude the possibility of a language using the form of RIPE to mean UNCOOKED, since the meanings encoded do not have the same neutrality value.

It is not clear at present whether the three cases which do not fit the principle (see bottom of table in (2)) constitute genuine counterexamples to it. Interestingly, they all involve the same dimension, SMOOTH/ROUGH. Moreover, agreement among English speakers that SMOOTH is neutral was not overwhelming (57%); thus it may be that neither SMOOTH nor ROUGH is neutral, and that the principle is not applicable to these cases of polysemy. Further tests for neutrality should be devised to validate the proposed set of neutral meanings here, and to decide whether these cases are counterexamples or not.

To conclude, these findings support the contention of a correlation between zero expression and contextual neutralization, and yield identification of a universal set of neutral meanings which play a significant role in word formation. Two principles have been formulated which partially determine the form and range of meaning of antonymous expressions. Two interesting questions arise for future research: Why is polysemy of antonyms restricted to sameness of neutrality value? and Why is the neutral meaning (rather than the non-neutral meaning) the basis for derivation?

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Typological Restrictions of Syntactic Ambiguity

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The treatment of syntactically indefinite structures is usually based on their semantics. But nevertheless similar attention should be given to the morphological properties and peculiarities of the elements which constitute "syntactically indefinite" sentences, because the frequency of their occurrence is directly and immediately affected by morphological structure of the language concerned. If we hear sentence "The shooting of the hunters was terrible" we may be inclined to interpret "of the hunters" as Genetivus Subjectivus rather than as Genetivus Objectivus because for "hunters" to shoot seems more appropriate than "to be shot". However from the grammatical point of view "of the hunters" is ambiguous. The Russian sentence "Mat' ljubit doch'" is ambiguous because in this type of declension the forms of the Nominative and the Accusative coincide and it is not clear whether the word "mat'" is an object or a subject. In English with its fixed order of words the position before the verb usually denotes the subject notwithstanding the possibility that instead of the definite article "the mother loves the daughter", we can say "the mother loves her daughter". However in analysing constructions similar to the above quoted grammarians do not pay enough attention to the morphological properties of the members of syntactically ambiguous sentences.

The interdependence between the facts of morphology and those of syntax has often been noted in linguistic science. The specific structural organization of a given language affects the forms of the interdependence between morphology and syntax as well as the way grammatical meanings are expressed in morphological paradigms or in syntactical patterns. The concept of paradigm is essential for the morphological level of the language. The contextual meanings of the grammatical forms and other semantic shifts they are capable of should be distinguished from the paradigmatic meaning of the form in the grammatical series it is part of in the morphological system of the language. This question has a methodological significance for the typological comparison between languages, because the researcher should decide what he is going to compare, the meaning of the forms or their use. If the content of individual items of morphological paradigms is revealed when they are opposed within a confined morphological system the same can be seen in such structures where individual forms of a given morphological set are used in the syntactical patterns of the same order.

It is relevant to cite an interesting observation of Fries on the use of the article in English, which can to some extent apply to other languages which

have inter-paradigm homonyms. In analysing the collocation *ship sails today*, which might have been used in a telegram, Fries writes that it is ambiguous because the indicator referring the word to a certain part of speech is absent here. If this indicator in the shape of the element *the* is placed before the first word, as in *The ship sails today*, the ambiguity is removed, just as it is removed if the indicator is placed before the second word, as in *Ship the sails today*. Fries notes that other elements can also serve as indicators of the part of speech, for example, the indicator *-ed*: *Shipped sail today* or *ship sailed today*. From this example it is clear that the article is a formal indicator of the noun but cannot itself be formally included among accident morphemes, although functionally, as in the above case, it can refer this or that word to a certain lexicogrammatical class just like inflexional indicators characteristic of this class. (See: Ch. Fries. *The Structure of English*. N.Y., 1952, p. 62).

Linguistic study should take due account of the features of the structure of a language system. The question therefore arises: to what extent should the specific features of the various aspects of language (i.e. the nature of its level structure) be taken into account. The systemic approach to language phenomena enriched typological studies with new ideas. Due to the integrity of a language system, features of one level usually correlate with the structure of other levels. Accordingly such a reflection presupposes distinct intersystem links for each level. The latter is particularly tangible in a comparison of strata of the same level in the structure of different languages.

However no language can be considered as a homogeneous combination of individual forms. It is especially in the languages whose morphological paradigm has a great number of homonyms that syntactically indefinite structures ("syntactic indefiniteness") can be observed rather frequently. It does not follow, however that such is always the case exclusively with the languages that have morphologically deficient word-forms, i.e. in nonflexive languages. The distribution of syntactically indefinite structures ("syntactic ambiguity") is closely related with the general typological characteristics of the language involved and consequently both with its morphological and its syntactical features.

We accept the following definition of the language type. A "language type" has to be distinguished by operating with distinctive features capable of expression in purely single terms. Each of these features must be strictly monosemantic. In determining distinctive features it is advisable to adopt the principle of binary oppositions but it is also possible to use a gradual scale of distinction which results the distinctive features becoming more fractionized. A "language type" should be established according to its fundamental and not secondary typological indicators which is why its semantic and not only purely formal parameters must be taken into consideration.

Sometimes it is difficult to distinguish between typological markers and universal law-governed patterns possible in all languages. But if we are concerned with languages having different morphological structure it becomes

obvious that the interlink between different levels of language has a direct bearing on the methods used to distinguish language types.

Is there possibility to assume that none of the devices for the expression of grammatical categories are used indiscriminately in language but are, in every instance, indicative of some new modulation of language meaning?

In morphology, it is primarily the structure of paradigm of nouns and verbs (and their classes as a whole); in syntax this involves the structure of word combination and form of expression of syntactic and semantic relations between the constituents of word-combination; the realization of lexico-semantic sphere of the language is brought about through lexical and grammatical classes, i.e. the distribution of words which is expressed in the parts of speech through grammar structure of the language. Structural organisation of the sentence, specific for the language concerned, influences the relations between syntax and morphology and therefore the typological approach in linguistics is of paramount importance for study and the solution of the problem of syntactically indefinite structures ("syntactic ambiguity") in various languages.

New Dialect and Linguistic Change

—An Age-Area Survey near Tokyo—

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1. Introduction

The main purposes of this paper are to show that new dialectal forms are increasingly being used even in modern Japan, and that glottograms and their multi-variate analyses are effective in studying linguistic changes in progress.

The Japanese language is undergoing rapid change for standardization. It is generally believed that Japanese dialects will soon die out. But changes for non-standardization are also observable in many parts of Japan. These phenomena can be called 'new dialect forms'. New dialect forms are linguistic phenomena which satisfy the following three qualifications:

1. more users are found among younger people than among older people,
2. users themselves know that the forms are informal or non-standard,
3. forms are different from those of the standard (or common) language.

The new dialect forms show linguistic changes in progress. The mechanism of the change is different from that of standardization, so, the new dialect forms serve as an observatory for linguists who want to know more about linguistic change.

Linguistic geography has been concerned with linguistic change in progress in the form of spatial diffusion. If a geographical axis is taken into consideration besides an axis of age, more fruitful results can be obtained. The glottogram (age-area graph) is a new method developed to satisfy these demands. Recent studies of new dialect forms show that the same kind of change is observed even in Tokyo, which is regarded to be the center of the standard language. Thus a glottogram survey was conducted between Tokyo and Fukushima which lies about 270 km. north of Tokyo.

2. Multi-Variate Analysis of Age-Area Distribution

An overall consideration of all the data will become possible if we adopt a multi-variate analysis called 'Hayasi's Quantificational Theory Type III' (in short 'Hayasi III'.) This is a statistical method something like Factor Analysis, the difference being that Hayasi III is developed for nominal (or non-numerical) variables, such as answers in public opinion surveys and in linguistic research. By applying Hayasi III, glottograms showing similar distribution are given similar numeric scores, and informants who gave similar answers are also given

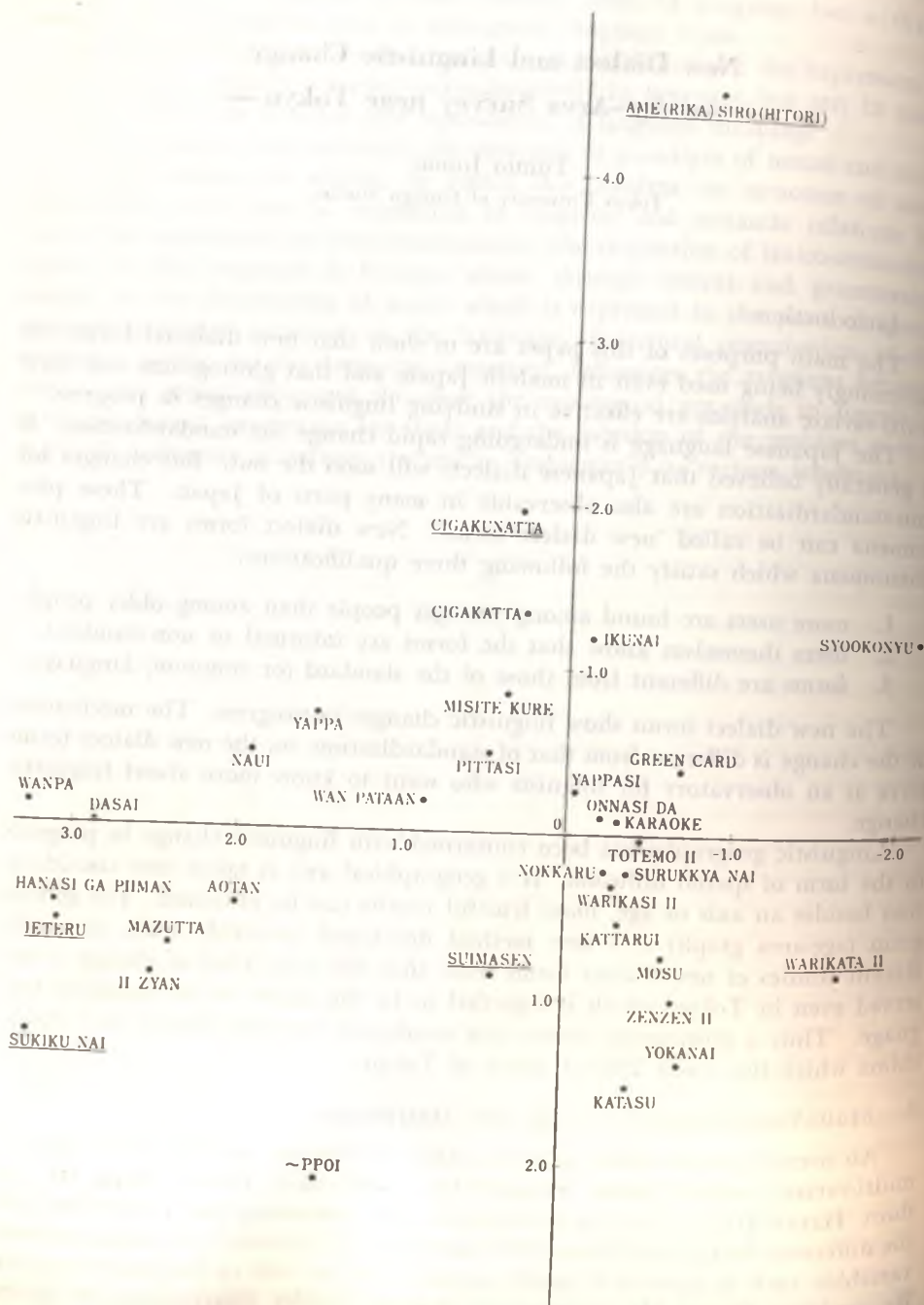


Fig. 1

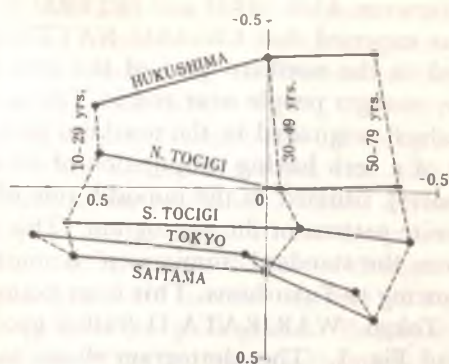


Fig. 2

similar numeric scores. Thirty-four items including neologisms and slang were treated here. Calculation has been done in a short time by using a large computer.

Fig. 1 shows inter-relationships among the words investigated. All the words here are answers to yes-no questions, in which we asked if informants used the forms suggested. Fig. 2 shows average scores for the informants divided into fifteen groups (three age groups by five areas.) (Figs. 1 and 2 are drawn upside-down so that comparison with glottograms will be easier.)

Fig. 2 shows the general tendency clearly. Horizontal axis is related to age. Vertical axis is related to area. Areas are arranged in order from north to south except that Tokyo is situated between Saitama Prefecture and south Tochigi Prefecture. Thus it is clear that by applying Hayasi III the data have been successfully classified according to age and area. So we can duly expect that Fig. 1 reflects the age and area distribution pattern of each word investigated. Comparison of Fig. 1 and the raw data in the form of glottograms will suffice to show the close relationships.

3. Comparison with Glottograms

Six representative glottograms are shown in Fig. 3. The arrangement here reflects relative locations in Fig. 1 (the six items in question are underlined.) IETERU, 'exactly said', literally 'can be said' is located in the leftmost side of Fig. 1, and by comparing it with Fig. 2, we can guess that IETERU is used mainly by young people. Fig. 3 shows that this is exactly so. IETERU is a new dialect form in the whole area. SUKIKU NAI, 'don't like', which is located near IETERU in Fig. 1, is also a new dialect form. AME SIRO, an abbreviation for 'amerika sirohitori' (a kind of harmful caterpillar) is located in the upper right-hand side of Fig. 1. By comparing this with Fig. 2 it is possible to infer that AME SIRO is used mostly in Fukushima Prefecture. Fig. 3 confirms this. This is an example of a neologism (an abbreviation) having restricted geographical distribution. CIGAKU NATTA, '(something) became different', is

located approximately between AME SIRO and IETERU in Fig. 1. The glottogram in Fig. 3 shows as expected that CIGAKU NATTA has a twofold character: it is mainly used in the northern part of the area investigated, and is beginning to be used by younger people near and in Tokyo. CIGAKU NATTA is a new dialect form which originated in the northern part of the area investigated. (This is a form of a verb having conjugation of an adjective.) SUIMASEN (Excuse me, I'm sorry), situated in the opposite side of the horizontal line of Fig. 1 shows an opposite pattern in the glottogram. This so-called 'corrupted' expression, changed from the standard 'sumimasen' is much used in and near Tokyo and is now advancing to Fukushima. This is an example of a new dialect form originating from Tokyo. WARIKATA II (rather good) is situated in the lower right-hand side of Fig. 1. The glottogram shows as expected that this expression is now old-fashioned, and is used mainly in the southern area including Tokyo.

In this way, all the thirty-four glottograms were arranged as to their distri-

IETERU										SUKIYU NAI										AME (RI) A SIPO (HITORI)										AGE									
1	0	/	-	-	-	-	-	-	-	127	/	-	-	-	-	-	-	-	-	127	/	0	0	0	0	0	0	0	0	127	MIN HUYUSIMA-								
1	0	/	-	-	-	-	-	-	-	128	/	-	-	-	-	-	-	-	-	128	/	0	0	0	0	0	0	0	0	128	PANATAGAWA								
1	0	/	-	-	-	-	-	-	-	130	/	-	-	-	-	-	-	-	-	130	/	0	0	0	0	0	0	0	0	130	ADACI								
1	0	/	-	-	-	-	-	-	-	131	/	0	-	-	-	-	-	-	-	131	/	0	0	0	0	0	0	0	0	131	NIMONMACU								
1	0	/	-	-	-	-	-	-	-	132	/	-	-	-	-	-	-	-	-	132	/	0	0	0	0	0	0	0	0	132	SUGITA								
1	0	/	-	-	-	-	-	-	-	133	/	-	-	-	-	-	-	-	-	133	/	0	0	0	0	0	0	0	0	133	MOTOMIYA								
1	0	/	-	-	-	-	-	-	-	136	/	-	-	-	-	-	-	-	-	136	/	0	0	0	0	0	0	0	0	136	KOORIYAMA								
1	0	/	-	-	-	-	-	-	-	138	/	-	-	-	-	-	-	-	-	138	/	0	0	0	0	0	0	0	0	138	SUKAGAWA								
1	0	/	-	-	-	-	-	-	-	139	/	-	-	-	-	-	-	-	-	139	/	0	0	0	0	0	0	0	0	139	YAGAMITA								
1	0	/	-	-	-	-	-	-	-	140	/	-	-	-	-	-	-	-	-	140	/	0	0	0	0	0	0	0	0	140	YABUKI								
1	0	/	-	-	-	-	-	-	-	141	/	-	-	-	-	-	-	-	-	141	/	0	0	0	0	0	0	0	0	141	IZUMIZAKI								
1	0	/	-	-	-	-	-	-	-	142	/	-	-	-	-	-	-	-	-	142	/	0	0	0	0	0	0	0	0	142	KUDANO								
1	0	/	-	-	-	-	-	-	-	143	/	-	-	-	-	-	-	-	-	143	/	0	0	0	0	0	0	0	0	143	SIRAKAWA								
1	0	/	-	-	-	-	-	-	-	144	/	-	-	-	-	-	-	-	-	144	/	0	0	0	0	0	0	0	0	144	NISIGOD								
1	0	/	-	-	-	-	-	-	-	145	/	-	-	-	-	-	-	-	-	145	/	0	0	0	0	0	0	0	0	145	SIRASAKA								
1	0	/	-	-	-	-	-	-	-	146	/	-	-	-	-	-	-	-	-	146	/	0	0	0	0	0	0	0	0	146	TOYOHARA								
1	0	/	-	-	-	-	-	-	-	147	/	-	-	-	-	-	-	-	-	147	/	0	0	0	0	0	0	0	0	147	KURODAHARA								
1	0	/	-	-	-	-	-	-	-	148	/	-	-	-	-	-	-	-	-	148	/	0	0	0	0	0	0	0	0	148	TAKASU								
1	0	/	-	-	-	-	-	-	-	149	/	-	-	-	-	-	-	-	-	149	/	0	0	0	0	0	0	0	0	149	KUROISO								
1	0	/	-	-	-	-	-	-	-	150	/	-	-	-	-	-	-	-	-	150	/	0	0	0	0	0	0	0	0	150	HIG NASUNO								
1	0	/	-	-	-	-	-	-	-	151	/	-	-	-	-	-	-	-	-	151	/	0	0	0	0	0	0	0	0	151	NIS NASUNO								
1	0	/	-	-	-	-	-	-	-	152	/	-	-	-	-	-	-	-	-	152	/	0	0	0	0	0	0	0	0	152	NOZAKI								
1	0	/	-	-	-	-	-	-	-	153	/	-	-	-	-	-	-	-	-	153	/	0	0	0	0	0	0	0	0	153	YALTA								
1	0	/	-	-	-	-	-	-	-	154	/	-	-	-	-	-	-	-	-	154	/	0	0	0	0	0	0	0	0	154	KATOKA								
1	0	/	-	-	-	-	-	-	-	155	/	-	-	-	-	-	-	-	-	155	/	0	0	0	0	0	0	0	0	155	KANASIZAKA								
1	0	/	-	-	-	-	-	-	-	156	/	-	-	-	-	-	-	-	-	156	/	0	0	0	0	0	0	0	0	156	WATAYAMA								
1	0	/	-	-	-	-	-	-	-	157	/	-	-	-	-	-	-	-	-	157	/	0	0	0	0	0	0	0	0	157	OHASHI								
1	0	/	-	-	-	-	-	-	-	158	/	-	-	-	-	-	-	-	-	158	/	0	0	0	0	0	0	0	0	158	UCHINOIYA								
1	0	/	-	-	-	-	-	-	-	159	/	-	-	-	-	-	-	-	-	159	/	0	0	0	0	0	0	0	0	159	UCHINOIYA								
1	0	/	-	-	-	-	-	-	-	160	/	-	-	-	-	-	-	-	-	160	/	0	0	0	0	0	0	0	0	160	UCHINOIYA								
1	0	/	-	-	-	-	-	-	-	161	/	-	-	-	-	-	-	-	-	161	/	0	0	0	0	0	0	0	0	161	UCHINOIYA								
1	0	/	-	-	-	-	-	-	-	162	/	-	-	-	-	-	-	-	-	162	/	0	0	0	0	0	0	0	0	162	KOGANEI								
1	0	/	-	-	-	-	-	-	-	163	/	-	-	-	-	-	-	-	-	163	/	0	0	0	0	0	0	0	0	163	OYAMA								
1	0	/	-	-	-	-	-	-	-	164	/	-	-	-	-	-	-	-	-	164	/	0	0	0	0	0	0	0	0	164	MAKADA								
1	0	/	-	-	-	-	-	-	-	165	/	-	-	-	-	-	-	-	-	165	/	0	0	0	0	0	0	0	0	165	WATAYAMA								
1	0	/	-	-	-	-	-	-	-	166	/	-	-	-	-	-	-	-	-	166	/	0	0	0	0	0	0	0	0	166	WATAYAMA								
1	0	/	-	-	-	-	-	-	-	167	/	-	-	-	-	-	-	-	-	167	/	0	0	0	0	0	0	0	0	167	WATAYAMA								
1	0	/	-	-	-	-	-	-	-	168	/	-	-	-	-	-	-	-	-	168	/	0	0	0	0	0	0	0	0	168	WATAYAMA								
1	0	/	-	-	-	-	-	-	-	169	/	-	-	-	-	-	-	-	-	169	/	0	0	0	0	0	0	0	0	169	WATAYAMA								
1	0	/	-	-	-	-	-	-	-	170	/	-	-	-	-	-	-	-	-	170	/	0	0	0	0	0	0	0	0	170	WATAYAMA								
1	0	/	-	-	-	-	-	-	-	171	/	-	-	-	-	-	-	-	-	171	/	0	0	0	0	0	0	0	0	171	WATAYAMA								
1	0	/	-	-	-	-	-	-	-	172	/	-	-	-	-	-	-	-	-	172	/	0	0	0	0	0	0	0	0	172	WATAYAMA								
1	0	/	-	-	-	-	-	-	-	173	/	-	-	-	-	-	-	-	-	173	/	0	0	0	0	0	0	0	0	173	WATAYAMA								
1	0	/	-	-	-	-	-	-	-	174	/	-	-	-	-	-	-	-	-	174	/	0	0	0	0	0	0	0	0	174	WATAYAMA								
1	0	/	-	-	-	-	-	-	-	175	/	-	-	-	-	-	-	-	-	175	/	0	0	0	0	0	0	0	0	175	WATAYAMA								
1	0	/	-	-	-	-	-	-	-	176	/	-	-	-	-	-	-	-	-	176	/	0	0	0	0	0	0	0	0	176	WATAYAMA								
1	0	/	-	-	-	-	-	-	-	177	/	-	-	-	-	-	-	-	-	177	/	0	0	0	0	0	0	0	0	177	WATAYAMA								
1	0	/	-	-	-	-	-	-	-	178	/	-	-	-	-	-	-	-	-	178	/	0	0	0	0	0	0	0	0	178	WATAYAMA								
1	0	/	-	-	-	-	-	-	-	179	/	-	-	-	-	-	-	-	-	179	/	0	0	0	0	0	0	0	0	179	WATAYAMA								
1	0	/	-	-	-	-	-	-	-	180	/	-	-	-	-	-	-	-	-	180	/	0	0	0	0	0	0	0	0	180	WATAYAMA								
1	0	/	-	-	-	-	-	-	-	181	/	-	-	-	-	-	-	-	-	181	/	0	0	0	0	0	0	0	0	181	WATAYAMA								
1	0	/	-	-	-	-	-	-	-	182	/	-	-	-	-	-	-	-	-	182	/	0	0	0	0	0	0	0	0	182	WATAYAMA								
1	0	/	-	-	-	-	-	-	-	183	/	-	-	-	-	-	-	-	-	183	/	0	0	0	0	0	0	0	0	183	WATAYAMA								
1	0	/	-	-	-	-	-	-	-	184	/	-	-	-	-	-	-	-	-	184	/	0	0	0	0	0	0	0	0	184	WATAYAMA								
NO OF CASES = 40-50-60-70-307										NO OF CASES = 40-50-60-70-307										NO OF CASES = 40-50-60-70-307										AGE									
0	(56)	USE-SAY <IU>							0	(23)	USE-SAY <IU>							0	(47)	USE-SAY <IU>							0	(20)	USE-SAY <IU>						
0	(65)	HEAR <KIKU>							0	(65)	HEAR <KIKU>							0	(20)	HEAR <KIKU>							0	(20)	HEAR <KIKU>						
0	(185)	DO NOT HEAR							0	(217)	DO NOT HEAR							0	(237)	DO NOT HEAR							0	(237)	DO NOT HEAR						
0	(1)	OTHERS <SONOTA>							0	(2)	OTHERS <SONOTA>							0	(2)	OTHERS <SONOTA>							0	(2)	OTHERS <SONOTA>						

Fig. 3-1

bution pattern by age and area. There is no space to show all the glottograms, but it would seem almost unnecessary since we can safely infer the distribution pattern from Fig. 1. This shows the effectiveness of the multi-variate analysis called Hayashi III for linguistic data.

By the definition above, new dialect forms are being increasingly used among younger generations. Thus we could surmise that the forms in the left-hand side of Fig. 1 are candidates for new dialect forms. Examination of each glottogram shows that the guess is right. But all the forms are not new dialect forms in the narrowest sense, that is, natural linguistic changes in progress. Slang and neologisms among youngsters are located together in Fig. 1, and also shows a similar distribution pattern in glottograms. Examples are NAUI, DASAI, HANASI GA PIIMAN etc. They are hard to distinguish from new dialect forms: for example, WAN PATAAN (<one pattern) was first used as slang among the youth, but it is now used also by older people, and seems to be becoming an ordinary word.

CIGAKUNATIA										SUIMASEN										VARIKATA II																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																											
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4. Conclusion

The result of our survey shows that the age and area distribution of new words spreading near Tokyo can be classified into three categories:

1. spread of the standard language from Tokyo,
2. spread of non-standard forms from Tokyo,
3. spread of non-standard forms from outside into Tokyo.

The phenomenon of the new dialect forms themselves and also their diffusion towards Tokyo are important because they show that there is a change from below. A simplistic model of language change consisting of change from above and by prestige (socially from upper class, stylistically from formal style, and geographically from big cities) does not work here. The model of language change should be altered and expanded, to include change from below.

Computer-aided Analysis of Field Survey Data. —GLAPS and its Application—

Tsunao Ogino
University of Tokyo

1. Introduction

Linguistic geography and sociolinguistics have been widely employed among dialectologists in postwar Japan. Over the last ten years, computer-processing of field survey data has become more and more common.

The author originally developed GLAPS (Generalized Linguistic Atlas Printing System) to produce linguistic atlases by computer. GLAPS has since been modified to produce glottograms and crosstables and to handle socio-linguistic data in general.

This paper presents an outline of GLAPS and an example of its application.

2. Characteristics of GLAPS

2.1 *Easy Understandability*

GLAPS is a FORTRAN program of about 14,000 lines. It is a package program whose strongest point is that even people ignorant of computer programming can obtain output results using it. About forty students of the Department of Linguistics, University of Tokyo, have used or are using GLAPS for their analysis of field survey data. Most of the students had never used a computer system before, but just a few hours of instruction were sufficient for them to understand how to use GLAPS and obtain their desired line-printer output.

2.2 *Applicability to Various Data*

GLAPS is applicable to various data, or various field surveys. Actually, the author has applied GLAPS to data in different formats from seven field surveys. Moreover, other researchers have used GLAPS to process their own dialect data.

2.3 *Compatibility with Various Computers*

GLAPS is compatible with virtually all computer systems. Because, the whole of GLAPS is written in FORTRAN. In fact, GLAPS has been run on eleven different computers in Japan without modification.

2.4 *Flexibility with regard to Data Processing*

To run GLAPS, users simply prepare their dialect data and compose a short program written in 'GLAPS language'. In this program, the user must specify all of the functions and operations to be performed. Most programs run only 20 to 30 lines. GLAPS can perform a variety of functions needed for dialect data processing, such as the re-categorization of data, the pairing and combining of investigated word-forms, the deletion of unnecessary data, and the division of

informants into subgroups by specified variables. Thus, GLAPS provides a versatile and flexible system for the user.

2.5 Processability of Multiple Answers

GLAPS is capable of processing multiple answers. This function is necessary for dialect data analysis. Because, an informant often gives more than one answer to a question about a word-form.

3. An Example of the Application of GLAPS

In 1974, a team from the Department of Linguistics, University of Tokyo,

**** intensive investigation at Nishiyama ****
(Shizukuishi, Iwate Pref.)

COWLICK

(the whirl of hair on the head)

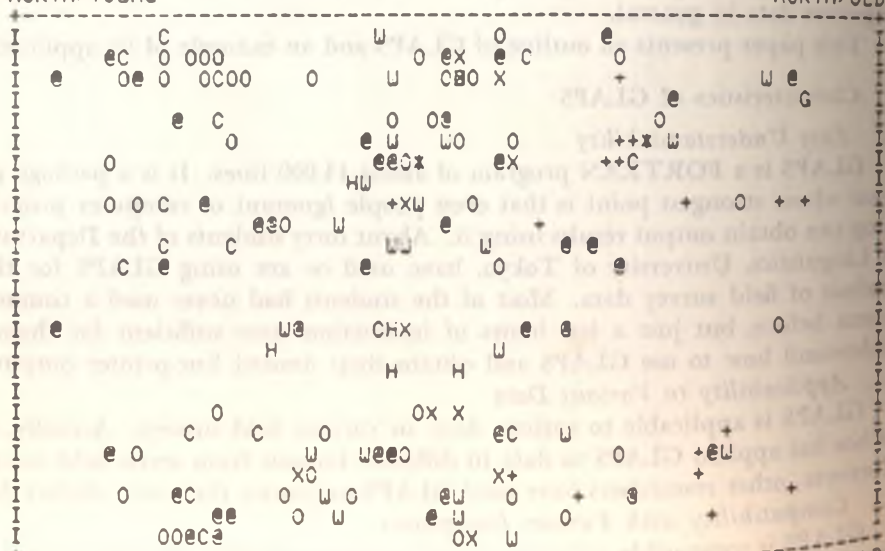
// rough classification //

CONTROL - COMMUNITY

VALUE - east-side

NORTH/YOUNG

NORTH/OLD



SOUTH/YOUNG

NO OF CASES - 203

e	(40)	uzumaki
+	(17)	makizyumonzi
G	(1)	makiguri
x	(2)	makibosi
X	(11)	makure
W	(24)	makurebosi
O	(48)	maruhosi
H	(5)	makurezyumonzi
C	(15)	tsumuzi

Fig. 1 Glottogram of East Side

conducted an intensive investigation to interview all the residents of the Nishiyama area of Shizukuishi township, Iwate Prefecture. The team interviewed 348 of about 500 residents above age 15, to examine distribution patterns of word-forms and the process of language change within a small area.

In this paper, the author will analyze only one item—"cowlick", the whirl of hair on the head. The first step of the analysis was the production of cross-tables, though the outputs of GLAPS are not included here. Those crosstables

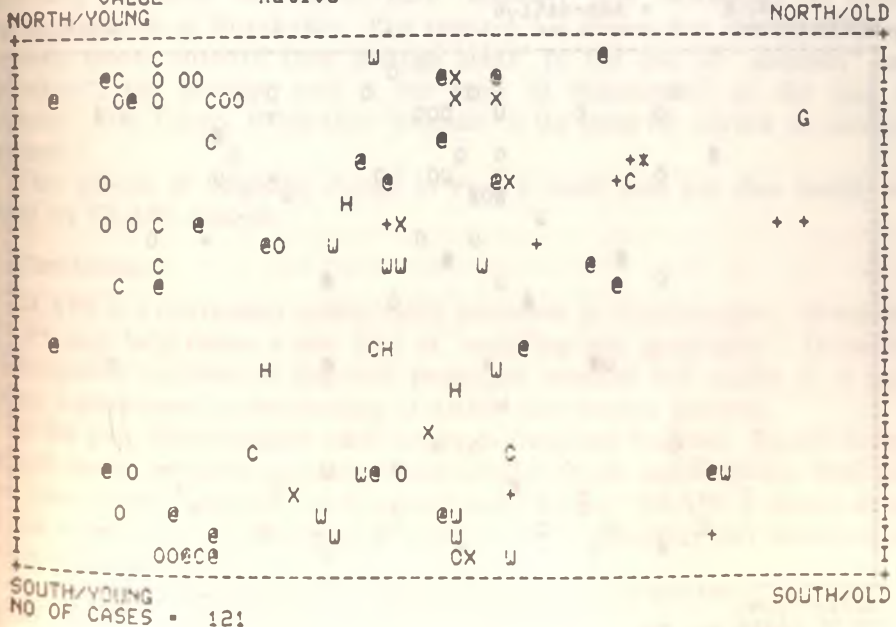
**** intensive investigation at Nishiyama ****
(Shizukuishi, Iwate Pref.)

COWLICK

(the whirl of hair on the head)

// rough classification //

CONTROL 1 = COMMUNITY
VALUE = east-side
CONTROL 2 = NATIVE-OR-NOT
VALUE = native



e	(25)	uzumaki
+g	(8)	makizyumonzi
x	(1)	makiguri
x	(1)	makibosi
u	(8)	makure
o	(13)	makurebosi
h	(17)	maruhosi
c	(4)	makurezyumonzi
	(12)	tsumuzi

Fig. 2 Glottogram of East Side Natives

revealed the combined influence of age and geography on the word *cowlick*. There are two ways to examine the combined influence: by linguistic maps of every age-group and by glottograms.

Fig. 1 is a glottogram of the east half of the investigated area. The right side means older people, and the left side younger people. The upper side is north, and the lower side south. Fig. 1 shows that "makizyumonzi" is used by older people, that "makurebosi" is by middle-age, and that "tsumuzi" is by

*** intensive investigation at Nishiyama ***
(Shizukuishi, Iwate Pref)

COWLICK

(the whirl of hair on the head)

// rough classification //

CONTROL 1 = COMMUNITY

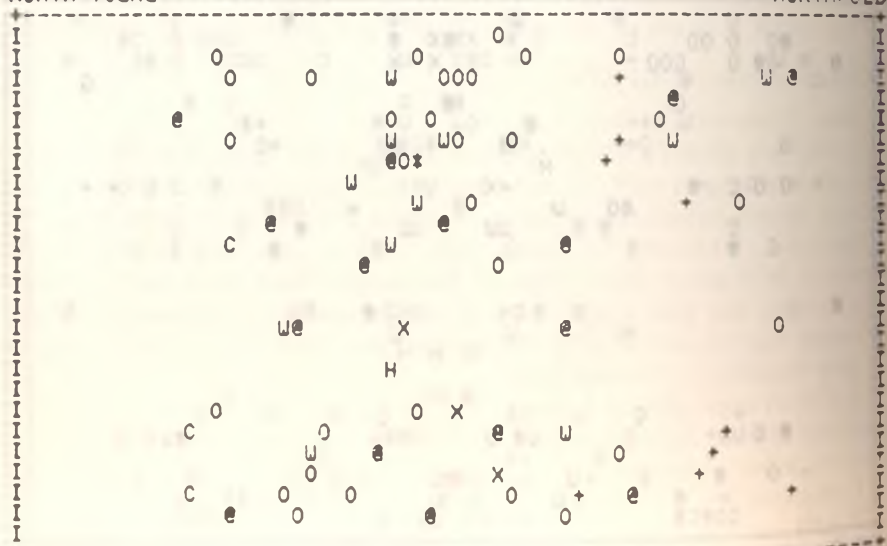
VALUE = east-side

CONTROL 2 = NATIVE-OR-NOT

VALUE = non-native

NORTH/YOUNG

NORTH/OLD



SOUTH/YOUNG

SOUTH/OLD

NO OF CASES = 87

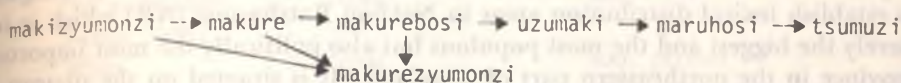
e	(15)	uzumaki
+	(9)	makizyumonzi
x	(1)	makibosi
X	(3)	makure
w	(11)	makurebosi
o	(31)	maruhosi
H	(1)	makurezyumonzi
C	(3)	tsumuzi

Fig. 3 Glottogram of East Side Non-natives

younger people. But, "maruhosi" appears here and there. How can we explain this phenomenon?

Figs. 2 and 3 plot the data of Fig. 1 according to whether informants are native or non-native, respectively. In Fig. 2, "maruhosi" (symbol 0) is used by only younger informants. In Fig. 3, however, it is widely used by all generations. This difference means "maruhosi" was brought into this area by non-native speakers and that young native speakers only recently began to use it. "Maruhosi" is thus a fairly new word-form in this area.

Though a detailed discussion must be omitted here, Figs. 2 and 3 suggest the changes in terminology used for "cowlick" in this area as follows:



According to another field survey, both "uzumaki" and "maruhosi" are widely used in the town of Shizukuishi. Past research has shown that new terminology generally moves outward from prestige areas. In the case of "uzumaki" and "maruhosi", the prestige area is the town of Shizukuishi; in the case of "tsumuzi", it is Tokyo. (Note that "tsumuzi" is the word for cowlick in standard Japanese.)

This process of language change in even a small area was thus readily revealed by GLAPS analysis.

4. Conclusion

GLAPS is a convenient system easily accessible to dialectologists. Moreover, GLAPS may help create a new field of "sociolinguistic geography". Including sociolinguistic variables in linguistic geography research will enable us to gain a more sophisticated understanding of dialect distribution patterns.

In the past, dialectologists made no use of computer facilities. Recent dialect research teams, however, especially those involved with sociolinguistic field surveys, have found computers to be useful and efficient. GLAPS is meant as an aid for researchers who are professionals in field linguistics but amateurs in computer programming.

In the humanities, generally, a package program like GLAPS could play an important role. Japan, at least, is backward in training persons in the humanities in computer programming. As far as the author knows, Japan is also backward in the development of convenient program packages for humanists. GLAPS might help promote the spread of computer-assisted dialectology. Needless to say, equipping students of the humanities with computer facilities is most necessary.

Word Geography in Nakhon Ratchasima: A Pilot Project

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This paper is a report of a pilot project undertaken by 4 members of the Department of Linguistics, Faculty of Arts, Chulalongkorn University, Bangkok, to establish lexical distribution areas in Nakhon Ratchasima (NR) which is not merely the biggest and the most populous but also politically the most important province in the northeastern part of Thailand. It is situated on the plateau in this part of the country and is the gateway to the rest of the region.

This research is the first of its kind, i.e., a Thai dialect study of the whole geographic area of a province to delineate lexically defined isoglosses.

The work is based on data gathered during 3 trips made to the region in October and December 1978 and April 1979 from 25 informants at 25 localities situated in 16 districts. The informants were selected in accordance with specified criteria: women between 40-60 years of age, born and having lived in the village all their lives, whose parents also are (or were) natives of the village.

From a study of lexical distribution, we were able to draw isoglosses defining two major linguistic areas. Approximately 3/4 of the total area of the province speaks the Thai Korat (TK) dialect with 17 locations in 16 districts while the remainder (located in scattered pockets to the north, east and southwest) speak what we have called the Thai Isan (TI) dialect with 8 locations in 8 districts.

Of the 250 basic words selected for this study 150 or 3/5 of the total, support the isoglosses drawn.

These 150 words may be termed multi-code items which exhibit differing codes, i.e. lexical variants.

The 150 multi-code items include:

- 99 2-code items of which 39 are diagnostic
- 37 3-code items of which 9 are diagnostic
- 14 4-code items of which 4 are diagnostic

Thus 52 multi-code items altogether support the isogloss dividing TI and TK.

The remaining 98 multi-code items exhibit great support with a high percentage in the majority of cases. We find that the range of support is as follows.

Multi-code Items	The range of support in %			
	TI Dialects	(Median)	TK Dialects	(Median)
60 2-code items	60-92%	(78%)	93-100%	(98%)
28 3-code items	46-64%	(57%)	67- 89%	(82%)
10 4-code items	20-80%	(40%)	50-100%	(80%)

Pak Chong exhibits deviant behaviour with the support of only 28%, 39% and 10% for the above 3 kinds of multi-code items respectively, because it is a district which is situated on the main road into NR and the rest of the north-eastern region. The Pak Chong dialect has been heavily influenced by the large numbers of people who have passed through this location.

<i>examples:</i>	<i>Thai Isan</i>	<i>Thai Korat</i>
<i>2-code items</i>		
"to look"	bəŋ	duu
"to do"	het	tham
"long (time)"	doon	naan
"to complain"	com	bon
<i>3-code items</i>		
"thin"	cəj	soo, phəəm
"to tug"	ŋaat	chiŋ, jəŋ
"hard-working"	man, duu	khajan
"to talk"	waw, paak	phuut
<i>4-code items</i>		
"lie"	tua?	pot, koohok, tɔ̀ɔ̀ɛ
"gossip"	waw khuan waw phuun	phuut dɛəm ninthaa

Tone markings have been omitted to simplify the presentation of lexical data.

While this research was conceived as a word geography project, data gathering included information on the tone system for each location. It may be noted in passing that the tonal evidence gives further confirmation of TI/TK isoglosses which we posit on lexical grounds. We find a 4-tone system in all of the TK dialects and a 6-tone system in all of the TI dialects. The pocket in the south-western region (Pak Chong dialect) is again an exception in having a 5-tone system.

Subdivision of Thai Korat dialect group

Within the TK dialect group, there is a further subdivision into two sub-areas: Eastern and Western, on the basis of segmental phonological evidence only. This subdivision retains all the lexical features of the TK dialect in general. The TK dialect is quite similar in lexicon to the central Thai dialect while the TI dialect is more characteristic of the entire northeastern region.

Evidence for this subdivision into E and W (Area I and II on map) is provided by 27 of the items used for determining the TI/TK classification.

1. In the Eastern area /r/ corresponds to Western /l/. Fifteen items are found for this.

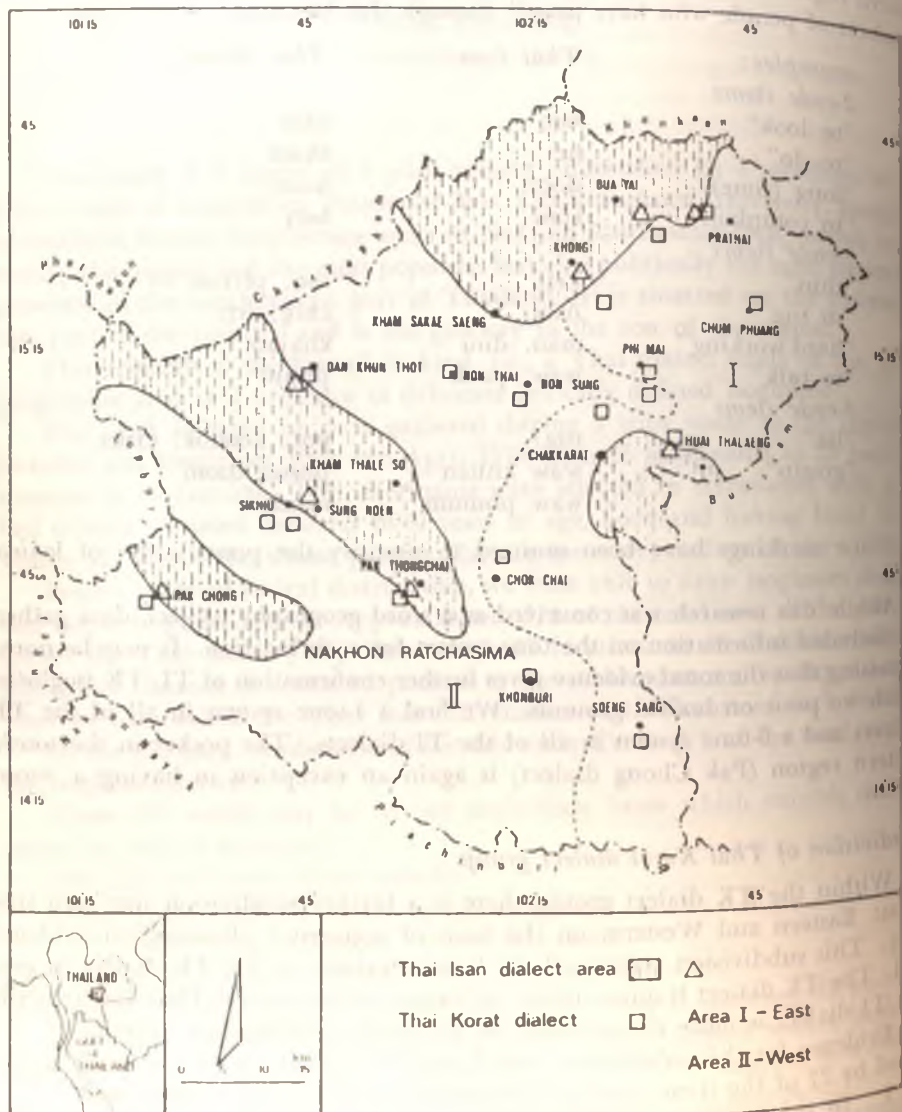
2. In E, consonant clusters containing stop and liquid are found, while in W the liquid is not found. Twelve items support this.

Note that the E/W split is further evidenced by tonal behaviour. Both E

Word Geography in Nakhon Ratchasima: A Pilot Project

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and W have the 4-tone system but they differ in the pattern of historical development of one Proto-Tai (PT) tone, i.e. in E PT tone *A reflects the \pm voice split while in W tone *A reflects the secondary \pm aspiration split.

examples

	TK-E	TK-W
"to call"	riak	liak
"ugly"	khii ree	khii lee
"to cover"	khlum	khum
"to roll"	klin	kin

Early work on this project was carried on by Dr. Pranee Kullavanijaya, Assist. Prof. Amara Prasitratthasindh, Chalida Rojanawathanavuthi and Vichin Panupong. I acknowledge with thanks the assistance of my colleagues, but am solely responsible for any shortcomings which this paper may have.

The following chart showing the proportional distribution of the total sample of 150 lexical items supports the distributional findings given in the paper.

Location	White Area TK item : TI item	Location	Shaded Area TK item : It item
1	144 : 6	14	139 : 11
2	143 : 7	15	141 : 9
3	138 : 12	16	140 : 10
4	142 : 8	17	142 : 8
5	140 : 10	18	29 : 121
6	137 : 13	19	26 : 124
7	142 : 8	20	34 : 116
8	141 : 9	21	37 : 113
9	140 : 10	22	38 : 112
10	137 : 13	23	19 : 131
11	137 : 13	24	47 : 103
12	142 : 8	25	68 : 82
13	143 : 7		

On Areas of Meaning

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Phonetic, morphological and syntactical peculiarities of a language are known to develop areas of certain extent. A good example of these is the so-called regional association of languages. Thus, the Balkan language-union is characterized by the following peculiarities: it has the reduced back vowel, the postpositional article, the future tense pattern conforming to one and the same rule, no infinitive ect. The peculiar features of the Lettisch and Estonian are stress on the first syllable, a prepositional genitive, fact-mood ect. Many languages of the Volga-Kama region have the following in common, the universal pattern for past tense formation, similarity in models used to form compound verbs ect. The Yakut and Tungus-Manchurian languages share some common features as well.

Peculiarities common to various languages may or may not give rise to a bundle of isoglosses which are sometimes of considerable length. Cf. formation of perfect tenses without any auxiliary verb in Turkish and Permian Finno-Ugric languages, use of the subjunctive mood in adverbial clauses of purpose (common in many languages), no accusative with an indefinite object, which is typical of the Turkic and some Finno-Ugric languages ect.

Specific meanings are also capable of developing certain areas. Thus the meaning of partitive in the Finnish and Estonian has much in common with that of the genitive—partitive in the Slavic and Baltic languages. There is a striking coincidence in the meaning of the verbs denoting some shades of actions in Selkup and Syryene.

Specific meanings most frequently give rise to isolated isoglosses of considerable length. Cf. use of two different meanings for "day" and "sun" in one word which is observed in some Turkic and Finno-Ugric languages; cooccurrence in one word of such meanings as "the world" and "light" in the Slavic languages, Rumanian and Hungarian ect, use of two adjectives meaning "old" for the inanimate and animate objects in the Turkic and some Finno-Ugric languages ect.

We have chosen as an object of our investigation the group of languages spoken in the North-east of the Soviet Union. This group comprises six languages: Zyryene, Votyak, Cheremis, Tatar, Chuvash and Bashkir. These languages have the following common semantic features:

1. The verb "pour" has the parallel meaning "strew", cf. zyr. kištyny, vol. kiškany, cher. sawas, chuv. sap-, tat. sip-, bashk. hip-.
2. The verb "shoot" has the parallel meaning "throw", cf. tat. at-, bashk.

at-, chuv. per-, zyr. lyjny.

3. The verb "cat-" has the parallel meaning "to saw", cf. tat. kis-, bashk. cher. püčkaš, syr. vundavny, vot. vandyny.

4. The meaning "to go downstream" in some languages of this zone is expressed by a special verb, cf. chuv. an-, zyr. kyvtny.

5. The verb "to seat down" has the parallel meaning "set" (about the sun, moon ect), cf. zyr. šondy pukšis the sun has set, vot. šundy pukšiz, chuv. xěvel larnă, cher. keče šinčyn.

6. The verb "to seat" has the parallel meaning "to be situated", cf. cher. pört enjer woktene šinza The house is situated near the river, chuv. Pirën jal Adäl šyv xërrinže larat' Our Village is situated on the bank of the Volga, tat. Bezneñ awyl Idel jarynda utyra Our village is situated on the bank of the Volga.

7. There are two verbs with the meaning "to be" in all languages. One of them has the meaning "to become", cf. cher. lijas, zyr. lony, vot. luyny, tat. bul-, chuv. pul-.

8. The verb "to thave" has the parallel meaning "to melt", cf. zyr. syvny, vot. šunany, cher. lewaš, chuv. irël-, tat. ěr-.

9. The verb "to catch" has the parallel meaning "to hold", cf. tat. and bashk. tăt-, chuv. tyt-, zyr. kutny, vot. kutyny, cher. kučaš.

10. The meaning "old" for the animate beings has a special word, cf. zyr. pōrys, vot. pereš, tat. iske, bashk. iŋke, cher. šoŋo.

11. The word "sound" has the parallel meaning "the voice", cf. zyr. šy, vot. kuara, cher. jük, tat. tawyš, bashk. tawyš.

12. Squirrel has the parallel meaning copeek (pense), cf. zyr. ur, cher. ur. vot. koňy, tat. tien.

13. The word "nose" has the parallel meaning "beak", cf. cher. ner, zyr. nyr, tat. bəryn.

14. The word "grass" has the parallel meaning "hay", cf. zyr. turun, vot. turyn, cher. šudo, chuv. uda.

15. There is a special verb with the meaning "to sing" (about the birds), cf. tat. sajra-, bashk. hajra-, vot. čirdyny, zyr. žolžyny.

16. The verb "to wait" has the parallel meaning "to pasture", cf. vot. vožmatyny, tat. köt-, bashk. köt-, chuv. kět-.

17. The verb "to draw" has in some languages of this zone the meaning "to smoke", cf. tat. tart-, bashk. tart-, vot. kyskyny, cher. šupšaš.

Besides these common specific meanings in the languages of this zone there are common semantic models of the word-building.

1. The verb "to put" is built according to semantic model "to make to seat", cf. cher. šyndaš, chuv. lart-, zyr. puktiny.

2. The denomination "The Milky Way" is built according to the model "The way of the wild geese" or "The way of the wild ducks", cf. vot. Lud šures, cher. Kajëkkombo korno, that. Kiek kaz july, zyr. Utkatuj.

3. The similarity of the semantic models is observed in the denominations

of some parts of the world: The North means literally night-side, cf. zyr. vojvyv, cher. jüdwel, vot. ujpāl, tat. tönjak, bashk. tönjak.

The South has the model "day-side", cf. tat. könjak, bashk. könjak, zyr. lunvyv, vot. nunazepal, cher. kečywalwel. The same model of wordbuilding exists for the word "East" in some languages of this zone. The model is "Morning-side". Cf. zyr. asyvyv, cher. erwel, vot. čukpal.

4. The phrase "The leaves come out" is built according to the model "The leaves burst", cf. tat. jafrak jara, zyr. kor potlašō.

5. The phrase "He wants to eat" is built according to the model "His eating comes", cf. tat. ašysy kilā, chuv. anān šies kilet, cher. tudyn kočmyze šues. This model exists in Zyryen to, cf. zyr. unnys loktō He wants to sleep (literally "His sleeping comes").

In the languages of this zone there are also specific grammatical meanings, ex. the reflexive has the parallel meaning of the old greek medium, cf. zyr. korsyšny seak for myself, vot. gožjaškyny write for myself, tat. tēgēn sew for myself, bashk. idem. The perfect can denote the meaning of an action, unobserved by the speaker. The so-called emphatic imperfect exists in all languages of this zone.

Study of specific meaning areas is of general linguistic interest. Particularly significant is the study of the reasons of these phenomena and peculiarities of their isoglosses.

Some Aspects of Tonal Development in Chinese Dialects

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Academia Sinica, R. O. C.

1. Introduction

Although different theories have been proposed in recent years about the origin of the tones of Archaic Chinese, it is obvious that there were four tone categories in Archaic as well as in Ancient Chinese to the best of my knowledge. These tone categories might have derived from final consonants in Proto-Chinese, but eventually changed into various tonal systems in all modern dialects, such as Northern Mandarin, which has four or three tones and Cantonese, which has nine or more tones. Tonal splits and mergers were mainly conditioned by the contrast of voiceless and voiced initial consonants, and sometimes by sonority or aspiration. In the process of tonal developments, it is worthwhile to observe some facts which may lead to new methods in reconstructing the value of proto-tones. Three aspects of tonal developments will be discussed in this paper.

2. "Sandhi tones" as proto-tones—the case of Southern Min

In the Lin-kao dialect, also called the Be language, of Hainan Island, I recorded two sub-dialects having the following tonal systems:

	1	2	3	4	5	6
L1	35	55	33	11	55	33
L2	11	55	33	22	55	33

There is only one case of tone sandhi that occurs in L1. For instance:

ba 35 "fish"	ba 11 kim 35 "gold-fish"
	ba 11 diam 55 "fish shop"
	ba 11 laŋ 33 "salted fish"
bui 35 "to cut"	bui 11 ŋau 11 "to cut rice plant"
teŋ 35 "to be born"	teŋ 11 ŋit 55 "birthday"
tsui 35 "hammer"	tsui 11 het 33 "iron hammer"

The rising tone 35 preceding any tone is changed into a low level tone 11. Comparing to the tonal system of L2, which has no sandhi forms, it is quite natural to conclude that the sandhi form of tone 1 in L1 is very likely the tone value of proto-tone 1. In other words, we may consider the sandhi tone 11 as the original or underlined form of the isolated tone 35. This differs completely from the traditional view of linguists in Chinese phonology. They usually use "basic tone" and "sandhi tone" to indicate the tones of a syllable in isolation and in connection respectively. Now, we suppose that the sandhi tones in connection are more conservative in keeping the value of proto-tones.

This simple method of internal reconstruction to a certain degree may be applied to much more complicated data such as that of the Southern Min dialects. But because tones change so easily and drastically in Chinese dialects, reconstructing the proto-tones of any dialect involves too many factors which are difficult to handle. The method of internal reconstruction and comparative studies have to be combined in order to solve this problem. Four tonal systems of the main sub-dialects of Southern Min are cited as follows:

	1 (Yin- p'ing)	2 (Yang- p'ing)	3 (Yin- shang)	4 (Yang- shang)	5 (Yin- ch'ü)	6 (Yang- ch'ü)	7 (Yin- ju)	8 (Yang- ju)
Lung-ch'i, Chang-chou (Tung 1960)								
"Basic tones"	24	313	53		31	33	32	13
		↓	↓	=6	↓	↓	44, 53	↓
"Shadhi tone"	33	33	35		51	11		11
Chin-chiang, Ch'üan-chou (Tung 1960)								
"Basic tones"	44	24	55	33	31	31	53	35
	↓	↓	↓	↓	↓	↓	55, 53	↓
"Sandhi tones"	44	11	35	11	55	11		11
Ch'ao-chou (Chan 1959)								
"Basic tones"	33	55	53	35	213	11	21	44
	↓	↓	↓	↓	42, 53	↓	33, 44	↓
"Sandhi tones"	23	213	24	21		12		21
Ch'eng-mai, Hainan (Ho 1981)								
"Basic tones"	22	41	21	33	24	=1	55, 55	33, 33

The value of nearly every single "basic tone" is divergent. Depending on these data alone, the proto-forms of these tones can hardly be reconstructed. But, except for Tone 5, all the "sandhi tones" clearly show resemblance. It seems plausible to take these sandhi tones directly as proto-tones as mentioned above. But special attention must be paid to the Ch'eng-mai dialect in Hainan that has no sandhi forms, and may in fact preserve the tones of an early stage, one which had not undergone the usual tonal change, and which may therefore play a decisive role in the Southern Min dialects. We must make detailed comparisons between the Hainan tones and the sandhi tones of the other sub-dialects. But before going any further, more data of Hainan sub-dialects should be cited:

Lo-hui (Ho 1977)	44	22	21	=6	13	42	55	22
					↓		↓	↓
					55		53	22
Hai-k'ou (Chang 1976)	13	22	21	=6	35	33	55	33
Ting-an (Norman 1969)	24	22	21	33	35	24	55	33
Wan-ning (Chan 1958)	33	11	21	53	13	=4, 5	44	12
Wen-ch'ang (Yüan 1960)	33	11	21	=6	24	53	44	21

Roughly speaking, the tone values of Hainan sub-dialects correspond to the sandhi tones of the other sub-dialects. We may posit the tonal system of Proto-Southern Min as follows:

1	2	3	4	5	6	7	8
*33	*11	*35	?	(falling?)	?	*55	*11

3. Tonal Change from short to long

3.1 Scholars of Chinese linguistics probably all agree on the theory that Ancient Chinese endings -p, -t, -k of checked syllables gradually merged into -k or a glottal stop, and eventually changed to an open syllable by dropping the ending. At the same time, the short tone that a checked syllable bears would usually change to a long tone. The most possible direction of this kind of change would be merging with the long tone having a similar or identical contour. This can be proven by South-western Mandarin spoken in Yün-nan province.

		1 (Yin-p'ing)	2 (Yang-p'ing)	3 (Shang)	4 (chü)	5 (ju)
Group I						
P1.	Lu-liang	44	53	42	24	313 (312)
P2.	Ch'ü-ching	33 (23)	53	42	35 (25)	31 (312)
P3.	Hsün-tien	44	53	31	13	42
P4.	Chan-yi	44	42	53 (54)	35 (25)	31 (312)
Group II						
P5.	Hsüan-wei	44 (33)	31	53	24 (214)	=2
P6.	P'ing-yi	33	31	53	24 (214)	=2
	⋮	⋮	⋮	⋮	⋮	⋮
P14.	Hui-che	55	31	53 (54)	24 (14)	=2
P15.	Ma-lung	44 (33)	42	53	13 (213)	=2

It is obvious that Group I which has five tones represents a dialect older than Group II which has only four. Starting from Group II, we can see that Tone 2 and Tone 3 are both falling tones, but relatively speaking, the tone value of the former is lower than the latter. Tone 5 syllables all merged into the low falling tone. Group I indicates two kinds of tonal systems. The first three have the same high falling contour for Tone 2, and Tone 3 is lower. But the fourth one is quite different. Close to Group II, Tone 2 is lower than Tone 3 in this dialect, and therefore paves the way for the merger of Tone 5 from short to long.

3.2 Different from the above, checked syllable also exhibit another direction of change. In the Ju-kao dialect of Chiang-su, some of the Tone 6 syllables have the following sandhi forms:

p'i? 35 "other": p'i 11 rəŋ 11 "others"

p'o? 35 "white": p'o 11 y 35 "white fish"

Out of ninety-nine Tone 6 morphemes, I found only nine which have this

kind of sandhi form. The tonal system of Ju-kao is as follows:

(Yin-p'ing)	(Yang-p'ing)	(shang)	(ch'ü)	(Yin-ju)	(Yang-ju)
11	35	424	44	44	35

We can see that there is a long tone 35 having the same contour of Tone 6. Yet, Tone 6 changed to Tone 1 instead of Tone 2. This is contrary to the process described above. If we call the first kind of change a rule of "assimilation," the second one would be a rule of "dissimilation."

4. Probable origin of the falling-rising tone

In addition to the Yün-nan data listed above in Group I and Group II, let us examine a wider ranges of materials:

	1	2	3	4	5
Group III					
P16. Feng-yi	44	31	42 (41)	55 (45)	24 (14)
P17. Yün-lun	33	53	31	55	13 (12)
P18. Erh-vüan	44	53	42	24	31 (21)
P19. Chien-ch'uan	44	42 (32)	31 (41)	55 (45)	13 (213)
P20. Teng-ch'uan	44 (33)	53 (54)	31 (41)	35 (24)	11 (21)
P21. Pin-ch'uan	33	42	53	13 (14)	31
P22. Yen-hsing	55	53 (52)	42 (41)	313 (213)	31

We notice the following varieties of Tone 4 in this group: 55, 45, 35, 24, 14, 13, 313, and 213. It is valid to describe the change from high (35) to low (13), or the other way around. But, the falling-rising 313 or 213 always occurs in the process of change. In Group II, Tone 4 syllables are also pronounced in falling-rising tones, such as 213 and 214. It seems that the proto-tone *13 shifted upwards on the one hand, and created a falling-rising tone on the other. The latter change is made possible by the addition of a little falling contour to the beginning of a rising tone. At any rate, the falling-rising tone must derive from a rising tone, no matter how we reconstruct it.

Another possible origin of the falling-rising tone is a low falling tone. But more study will be required before we can give a detailed description.

"The Presentation and Interpretation of English Dialects: Computer-assisted Projects"

Wolfgang Viereck
Universität Bamberg

Whereas we have large computer corpora of connected *educated* British English speech available based both on written and spoken sources, i.e. the Lancaster-Oslo/Bergen Corpus—as a counterpart to the American Brown Corpus—and the London-Lund Corpus, which are used more and more widely with great profit by scholars all over the world, there is, unfortunately, nothing comparable as far as *dialectal* British English is concerned.

In addition to a computerized corpus on Scottish English (Edinburgh project), consisting of conversations with 15-17 year-old schoolchildren from a working class comprehensive school and a middle class private school, there are a few modest beginnings that may develop into data banks of connected British English speech including non-standard varieties. Thus there are extensive orthographic transcripts of the speech of pre-school children (Bristol project), of 6-12 year old children (project of the Polytechnic of Wales) and of adolescents aged 11-17 on Tyneside. Yet the tape recordings of the non-standard speech of *old(er)* English people interviewed in the 1950s and 1960s in connection with the *Survey of English Dialects* (SED) and housed at the University of Leeds are still not available for scholarly investigations. These conversations, once they are orthographically transcribed and computerized, will constitute an important source for writing a much-needed grammar of dialectal English. It is to be hoped that the great usefulness of the corpora on educated British English, as evidenced in many studies, will help persuade the scholars in charge of the Leeds materials to make them available before too long.

Since this is likely to be a really long-term undertaking, especially when the grammatical tagging of such a corpus is considered as well, I wish to comment on a less extended project of computerizing, presenting and interpreting the data of the SED, which was brought out in narrow phonetic transcription in twelve geographically arranged volumes of so-called *Basic Materials*, published between 1962 and 1971. The responses are given in list form item by item, following the ordering of the *Questionnaire*. Although the way the data are presented is the most neutral one possible, the arrangement of the publications according to regions is such that *four* SED volumes must be consulted to get the complete information on a single item. For the comparatively few items selected and mapped in *A Word Geography of England* (1975) and in *The Linguistic Atlas of England* (1978) such a tedious procedure is, in most cases, no longer necessary. I say in most cases because there are quite a number of

instances where one needs to go back to the *Basic Materials*. Moreover, and this is more serious, neither publication provides any insight into the dialectal speech areas. A knowledge about these areas is important for a number of reasons, not least of which is their indispensability for a sensible discussion of British English and American English interrelationships. It is for these and other reasons to be mentioned below that a computerization of the SED data has become necessary and important.

For the coding of the lexical and grammatical, i.e. above all morphological, material, to which I restrict myself, I have devised a special key and drawn up a number of conventions to be followed. The phonetically transcribed responses require a standardization of the orthography. This also makes the material more suitable for quantification. The standardization of spelling among the responses conforms to the spellings given at the head of a set of entries for an item in the respective SED volumes, provided that these spellings in turn conform to those of main entries in Joseph Wright's *English Dialect Dictionary* (1898-1905). For our purposes main entry is defined as separate lemmatization together with a definition and information on geographical distribution. Variant spellings found subsumed under a main entry in Wright's dictionary are normalized to the spelling of the main entry, as are forms that are separately lemmatized there but lack any further information except a reference to the main entry or some sub-part of it. In view of the fact that the SED material has not yet been phonemicized, should this be at all completely possible, the procedure adopted is the only feasible one in a lexical investigation. It is a compromise dictated by our present state of knowledge. Any other approach would have caused problems especially within the vowel range with regard to the sameness of lexemes. Certain standardizations followed in coding the lexical data are, of course, not followed in morphology. Inflectional endings of verbs, nouns with dissyllabic plurals, pronominalizations and modals, to mention some of the most important features, are spelt as they appear in the transcriptions and thus show greater variation than the other data. For the lexical material the coding is completed for all areas of England, two thirds of the data have been punched on cards and put on magnetic tape. The coding of morphology has begun and is progressing satisfactorily. The computerization of the whole data is thus at an advanced stage.

Once completed, the data bank will be used in various ways. Firstly, the lexical and the relevant morphological data will be presented in the form of a dictionary. Secondly, a computer-produced linguistic atlas will be published that will present the whole SED data in map form. The maps thus obtained lend themselves to several interpretations. There will be, first of all, a comprehensive analysis of the dialectal speech areas of England with their focal and relic areas and their equally obvious transition zones. I have already completed such a study, working, however, with a rather restricted number of heteroglosses, as is common practice in traditional linguistic geography and also taking, as is equally customary, extralinguistic factors into account. Also, it will then be possible

to make nationwide studies of certain selected problems, such as the system of independent personal pronouns or the "causative use" of originally intransitive verbs (e.g. *learn* in the sense of 'teach'). Connected with the examples mentioned is the nationwide study of St(andard E(nglish) influence on the dialects. Are there specific domains and/or geographic regions that are especially sensitive to dialectal erosion? When were StE words taken over? What are the consequences of such transfer? Here multiple responses and informants' statements about the status of words, which are duly coded, are of particular importance. The approach followed is both synchronic and diachronic since the data collected by Guy S. Lowman, Jr., in southern England and most of the English Midlands in the late 1930s, which I edited and published in two volumes in 1975 and which I also interpreted in two article-length studies of 1980, both according to traditional selective and to modern quantificational methods, will be drawn upon for purposes of comparison. The project will thus allow insights into the developments of dialectal English during a decisive period of time (from pre-World War II to post-World War II years). Thirdly, the data will be quantified and interpreted both in *dialectometrical terms* and in terms of *cluster analysis*. Here, as well as in the afore-mentioned more traditionally-oriented interpretation, the lexical and the morphological data will be interpreted separately in order to determine whether the findings pertaining to different levels of a language—to the more stable morphological and the more fluctuating lexical level and resulting from completely different approaches—agree with each other and, if so, to what extent. Lowman's data serve, again, as a basis to draw comparisons.

The quantitative approach I am mainly concerned with, termed *dialectométrie* by Jean Séguy and developed further by Hans Goebel, allows *all* linguistic atlas data to be taken into account. It is based on the question of identity or non-identity of two linguistic forms. Linguistic atlases are representable as two-dimensional matrices: the variables are locality points and maps. In a paper to be published in England I have applied two dialectometrical tests—the coherency test and the identity test—to SED data of southern England, as a kind of pilot study, without computer assistance. For the coherency test the responses given in any one locality are compared with the responses given in every other locality of the area investigated, while with the identity test one locality is chosen in several different regions of the investigated area and the responses given in the whole area are in each case compared with those of the particular locality chosen as regards identity or non-identity. The results obtained can then be put on maps and interpreted. All this sounds easier than it actually is, but the problems involved cannot be discussed here. It has now been demonstrated that high coherency values point to a close proximity to the standard language only when the linguistic centre forms part of the area investigated, otherwise this fact simply shows a considerable linguistic homogeneity of the area(s) thus marked. An identity test will also be carried out for the forms of StE. Here the title of the map will be taken as the form to be checked

in all SED localities. It is a kind of fictitious point representing the StE norm. Every response in every locality will be compared with the map title to see whether it corresponds with it or not. It is in this way that the influence of StE on the dialects will be quantified.

The projected dictionary will be in two parts: Part 1 will be restricted to dialectal English in the narrow sense of the term, taking the fact into account that SED informants are bi-dialectal, understanding StE and sometimes using, e.g., StE lexemes—if not with the proper pronunciation—in addition to their respective dialect. The distribution of forms (regional vs. national) and the labeling/non-labeling of these in standard dictionaries of English will be of great importance in determining what to include and what to exclude. StE words will be included only when their meaning is different from standard usage. Part 2 will consist of an index, listing the complete SED material; all the responses given to a particular question will be listed under the respective StE headwords. Whereas the material of Part 1 will be arranged alphabetically, that of Part 2 will be grouped around certain domains, thus largely following the *Questionnaire* order. The numerical lexical diversity of different domains will thus stand out clearly. Since the dictionary aims at a broad readership, its entries will not contain phonetic transcriptions. With our present knowledge, the notation could not be phonemic. Furthermore, only in very few localities did the SED fieldworkers interview one informant, mostly they interviewed more—even seven in one locality! This fact alone has phonetic consequences. In an introductory chapter the distribution of important sound types will be shown on a number of maps, as far as is possible, to which the interested reader can relate some of the more widely used dialectal pronunciations, also those of otherwise standard forms, listed in Part 2. Etymologies will be provided whenever possible and all available grammatical information will be given. Drawings will be used in cases where semantic definitions tend to become too complex. In view of the rather broad SED network of localities, the geographical distribution of forms will be given as precisely as possible. Since computer-produced symbol maps of every item will be on hand, they will be included, especially when forms occur in several completely different areas and will thus imprint the distribution on the reader much better than a mere verbal listing. These are only the broad outlines. Further problems will arise and must be solved as the work progresses.

"Strategies in English and Japanese Dyadic Discourse"

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An analysis of the functional semantics and their tactical combinations in dyadic discourse in family settings both of Japanese and English speaking groups is based upon the multifunctional use of utterances in psycho-social interaction. The purpose of the study is to examine some of the tactics of rule-governed behavior in dyadic discourse, particularly in the medial phase of such discourse and compare the resulting tactical typologies or strategies in English and Japanese.

The data which has been utilized consisted of two hours of taped conversations in both an English setting and a Japanese one. The data was judged to be sociolinguistically comparable in that each group centered in a family group and their close acquaintances. Each group consisted of adults and teenage members only. The English data consisted of 27 episodes involving a total of fifteen persons; the Japanese consisted of fourteen episodes involving thirteen persons. The data was analyzed first for the functional uses of the utterances including cognitive, affective and emotive semantic functions. Primary meaning (the dominant intention) and secondary meaning (two degrees) only were recorded on the basis of the intention of the speaker and the response of the listener's choice. Most utterances were clear from context, but those few which remained highly ambiguous were not included in the analysis. Of the semantic functions only seven percent of the English and six percent of the Japanese had a primarily cognitive function. The remainder were related to such affective and emotive functions as suggestions, apologies, criticism, surprise, appreciation, etc.

Utterances in the grammatical form of questions 51% in English and 44% in Japanese were used to make inquiries. (Inquiries constituted 6.3% and 6.8% of the total data in English and Japanese respectively.) Other semantic functions of questions were as follows: surprise (Eng. 10%; Jp. 26%), challenge (E. 7%; J. 2.6%), suggestion (E. 6%; J. 2.6%), requests (E. 6%; J. 1%), agreement (E. 3.6%; J. nil), reluctance (E. 3.6%; J. nil), greetings (E. 3.6%; J. 1%) teasing (E. 2%; J. 3%), lead-in (E. nil; J. 6%), etc.

An examination of "tag questions" reveals some potentially important differences between English and Japanese dyadic discourse. Only 5.4% of the English questions were of this form, but in Japanese 26.5%. Of the latter 29% were used for inquiry, 23% for lead-ins, 11.5% for criticism 8% for appreciation, 6% for explanation-clarification, etc.

The predominant tactical paradigm or strategy of the semantic functions

can be summarized in the following paradigm:

Speaker A: I + P.F. F. + At. Ex. + (L.I.) + C.

Speaker B: (L.I.) + A/C + (L.I.) + At. Ex. + C.

In which the terms are I=initiators and/or lead-ins; P.F.F.=primary focused semantic function; At. Ex.=attitudinal (self or other directed) expression; L.I.=lead-ins (preparatory statements); C=explanatory-clarification statements; A/C=agreement or contradiction. The diagonal slashes refer to an obligatory choice of one, and the parentheses to optional elements, but which occur frequently in the data.

Example I

- | | |
|--------------------------------------|------------------|
| A: You won't dance? | Lead-in/surprise |
| Come on. | request/coaxing |
| B: No, really, | contradicting |
| I'd like to, but I can't. | |
| I mean, | lead-in |
| if old Mr. Simpson came here and saw | explanation |
| me dancing...he'd have my rear end. | |

Example II

- | | |
|--------------------------------------|-------------------------|
| A: Oi | Attention |
| Iya, | Lead-in |
| chotto yose yo. | Request |
| Kimi. | Rel. bonding |
| Iya, | Lead-in |
| boku wa ne, koko no fuufu no nakoodo | Explanation (criticism) |
| nan da yo. | |
| B: Nee, | Lead-in |
| reezooko ni, nee, juusu hietaru | Request |
| kara, katte ni dashite nonde te. | |
| A: Hai hai. | Agreement |

The above strategy typology accounts for 47% of the English data and 42.4% of the Japanese data semantic functions. (1)

Within the data the semantic function of "lead-ins" ranked highest. These preparatory expressions prior to the primary focused semantic function in the paradigm were of three types in both English and Japanese. Type A consisted usually of one or two words such as "uh" "hey" "I mean" "you know" "now" "here" "come on" "uh, listen" etc; "iya" "a" "un" "hora" "jaa" "nee" "soreja" "anoo" "anoo ne" "tsumari". Type B was a preparatory "because" type of statement, and type C was a "non-because" type of preparatory statement. A number of differences in combinatorial patterns between English and Japanese did occur. Japanese frequently had multiples of lead-ins whereas in English usually only one utterance. This may be suggestive of the sense of "indirection"

in Japanese discourse. In Japanese such lead-ins occur most frequently with cognitive information, criticism, explanations and then apologies and suggestions. Whereas in English the frequency order is requests, suggestions, promises, disagreement and then apology and inquiry.

Explanatory or clarification functions following the primary focused semantic functions also showed some differences between English and Japanese.

Clarification Types	% ENGLISH	JAPANESE
A: Explanations of previous functions	71.5	59
B: Explanations of lexical meanings	4	nil
C: Explanations in response to inquiry	1.6	18.2
D: Explanations of behavior	8	8
E: Defensive explanations	14	13.8

The frequency of type C is perhaps indicative of a cultural imperative in which in English explanatory comments should be anticipated, but in Japanese they are given often only on inquiry. Explanatory utterances ranked third in English (9.4%) and second in Japanese (11.9%).

Among the primary focused semantic functions in the paradigm, apology had a relatively low frequency of occurrence, in English 0.8% and in Japanese 1.1%. However since the vocabulary of apology and gratitude in Japanese are essentially the same, the "effect" of such vocabulary (1.1% + 2.5%) would give a stronger impression of the apology and deference usually associated with the Japanese society.

This study is a part of a continued examination of English and Japanese dyadic discourse in which the realization of the semantic functions in terms of their lexical and syntactic representations as well as the typology of their tactics in paradigms has been made. As a study of the medial phase of dyadic discourse, attention has been paid to the typologies of the contexts in which functions occur and the patterns of the responses made within dyadic discourse.

(1) In an earlier study (Berendt 1979) 65.5% of all semantic functions occur within this paradigm.

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The Function of Intonation Contours in Biomedical Speeches

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Since 1976, I have been engaged in a study of spoken scientific rhetoric, specifically, in how biomedical scientists make their first public communication of research results to their peers by *reading papers* at professional meetings, a study including, but not limited to, traditional linguistics. Biomedical sciences constitute a basic research field devoted to the eventual betterment of the human physical condition, as contrasted with medicine, which is a technology for the delivery of care.

In April, 1979, I attended the 63rd Annual Meeting of the Federation of American Societies for Experimental Biology (FASEB), the umbrella organization for specialized biomedical professional groups. I observed and audiotaped five sessions, from which I transcribed 52 papers, 12-15 minutes in length, all those presented by an apparently native speaker of English. Here, I report on 12 papers from Session 414.

I have already established that the biomedical speech has a decidedly more *narrative character* than the journal article (Dubois forthcoming); For example, one of the speeches of 414 has essentially all verbs in past modification. As to delivery, the speeches are not uniformly written out and then read, although that practice can occasionally be found. There is thus inter-speaker delivery variation, and marked intra-speech variation as well. The speeches begin with an *introduction* (variable length), delivered from a lectern placed at left-front of the platform. Observable preparation varies from complete script through notes/outlines to no written materials at all. The *body* of the speech is always—no exceptions—organized around slides. Overwhelmingly, speakers leave the lectern to face the screen at center back of the platform and direct their remarks to slide content. They can be heard by the audience via a lavalier microphone; none takes papers from the lectern, since one hand is occupied with the flashlight pointer. Speakers return to the lectern and face forward for the *termination* and following short question and answer period. Some, but not all, speakers use slides with introduction and termination, as well as with the body of their speeches.

The Pear Stories (Chafe 1980) describes cross-cultural experiments in the spontaneous production of *narratives*. Groups of subjects view a short film, wordless but not soundless, which projects a series of events, and afterwards recount the story to the experimenter. The pear story narrators which the FASEB narrators will be compared to are a group of undergraduate women from the University of California-Berkeley. Although Chafe is interested chiefly

in cognition, what he calls the "deployment of consciousness," and I am interested in the rhetoric of professional biomedical spoken communication, there are nevertheless interesting points of comparison between the *two sets of narratives*: intonation contours, words in *tone groups* (Chafe's idea units), *centers of interest* (a chunk of information too large for one tone group; typical signal: syntactic closure + falling intonation), and extended sentences (cognitive units which cross sentence-final (falling) intonation). For the biomedical speeches, function of intonation, tone group length, and the like, is emphasized.

METHOD

To my original transcriptions, I added repetition of words and slips of the tongue, and most important, I separated the text into numbered perceptual tone groups (nuclear tone + following pitches, if any, set off by pauses) and coded the contours. I did not measure pauses, nor did I mark them.

RESULTS

Contour Types. Chafe characterizes his tone group (idea unit) as follows:

1. (most) ending with intonation contour identifiable as clause final, "usually either a rise in pitch, such as we are marking with a comma, or a fall, such as we are marking with a period." (p. 14) There is no discussion of unusual cases.
2. (typical) separated by pauses. Pausing is quite consistent in the biomedical speeches.
3. (tendency) one clause per tone group.
4. (many) beginning with *and*; (some) *but*, *so*.

The contour situation is much more complex in the biomedical speeches, there being six different contours in the biomedical speeches.

Chafe's finding of a preponderance of rising clause final contour may be due to the interactional nature of his experiment: subjects looking to the experimenter for confirmation of the correctness of their memory. At any rate, /, / *i.e.*, a fall from pitch 3 to pitch 2, is the most common contour of the biomedical speeches. Functions of /, /, a fall from pitch 3 to pitch 1, will be commented on in connection with the *center of interest*.

Words in Tone Groups. The pear story tone groups have a mean of 6.01 words, while the biomedical speech average is 5.47, which is also the median of the 12 papers, with a range of 1-13 words and a range of averages from 4.38 to 7.5. The word count is slightly problematic, since I have counted expressions such as "delta PCO₂" as one word. Furthermore, the pear stories apparently do not contain words of the length of "formal-methionylphenolalanine," nor do they ever speak of "eosinophilic granulocytes," that is to say, pear story words may be shorter, on the average. Perhaps a syllable count would be more meaningful than a word count.

In any case, an average of the entire group is slightly misleading, since, as

demonstrated by averages, some speakers habitually speak longer tone groups than others. Moreover, there is rhetorical variation, in that many times a speaker suddenly and obviously shortens his tone groups when he is making an important point. Typically, the shortening takes the form of separation of subject and predicate into two tone groups. In one case, the main verb itself was separated into two tone groups. There is an extended passage in which there is a close correspondence of tone group and clause (either dependent or independent), but the passage is quite uncharacteristic of the biomedical speeches, although not unique. To summarize: the length of tone group varies by rhetorical purpose in the biomedical speech; if there is no or little such variation in the pear stories, it may be that the narrators had no intrinsic rhetorical purposes.

Center of Interest. Effectively an intonational sentence, a *center of interest* contains more information than the speaker cares to put, or perhaps can put, in one tone group, typically signalling by syntactic closure and falling contour. The biomedical speeches do indeed have intonational sentences (some, in fact, are really intonational paragraphs of up to five independent clauses); some speeches have relatively many and some relatively fewer. The mean of tone groups per intonational sentence is 4.85, HALF AGAIN AS MANY as the pear stories have, and the average of each biomedical speech is higher than the average for the group of pear narrations. Once more, averages can mislead. There is a marked tendency for intonational sentences of the FASEB narratives to be longer in the body, i.e., the more extemporaneous part of the speech. Thus, I see no simple relation between preparation and length of intonational sentence, but it appears that great length is associated with familiarity with one's material and with less actual preparation of sentences. The cognitive function of intonational sentences may well be as Chafe says. But the sentence-final intonation has two rhetorical functions in the biomedical speeches. It can signal an important point, in the midst of a syntactic sentence; it also can signal an appositive.

Extended Sentence—The *extended sentence* is longer than center of interest and is not defined syntactically or intonationally. It is a higher order discourse constituent, one that crosses sentence-final contours. Such a unit is certainly found in the biomedical speeches in great complexity, but it is not a device for reparation of premature closure such as Chafe typically found. It can occur as an *oral topic sentence* with sentence-final intonation, followed by supporting details which are often presented as a single intonational paragraph. Other functions of the intonational paragraph in the biomedical speeches are to present whole cognitive units such as an experimental episode or to make a rhetorical point: the peak of an episode (the commentary on it) is often marked by the longest intonational paragraph in the episode.

CONCLUSION

I believe that the striking differences between the pear stories and the bio-

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medical speeches are due in part to the following factors: the nature of the material to be narrated; the extent of preparation, the education and experience of the speakers; the degree of personal involvement and motivation of the speakers. Some of the pear story characteristics may in fact be artifacts of the experimental situation.

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Sentence Delimitation and Sentence Order in Japanese

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It is obvious that the semantic content of a text¹⁾, conceived as a set of given clauses, can be distributed into sentences in a number of ways. I assume that the set of appropriate clause orders and the set of potential main clauses are defined by a set of rules. These rules ought perhaps to be understood as pertaining to the domain of pragmatics, but the problem of the domain is beyond the scope of this paper. Nevertheless, it is evident that application of such rules is a basis for defining certain important conditions of cohesion.

The space for my paper is limited. The main points of my argument will be made with reference to one example, namely to the first two paragraphs of Kawabata Yasunari's novel "Utukusisa to kanasimi to" (Beauty and Melancholy).

My assertions are as follows:

1) For any monologue discourse, existing in a written form, there is a definite number of *minimal text sentences* (further MTSs) to express its content without recovering any implicit or extra-textual information. The MTSs are not identical to clauses, because there are clauses which are not able to be isolated as independent sentences unless the meaning of the discourse changes. For instance the initial MTS of the above-mentioned text (further text T 1) has the form: "Revolvable chairs are in a row along the A-side of the saloon-carriage of the express train Hato".²⁾ Propositions like "The Hato is an express train", "The Hato has a saloon-carriage" etc. are not MTSs, because they are not assertions of the T 1, but only its presuppositions. Note that even in the so-called FSP theory the segments corresponding to these propositions would be carriers of the non-focal information. A similar case is the MTS 21 (part of the 8th sentence of the T 1) which has the following content: "The lonely chair, spontaneously turning in front of the lonely Ooki Tosio (old information)—called forth a feeling of loneliness in his heart (new information)" (abbrev.). Segmentation of the MTS 21, as presented here, into smaller units would result in repeating isolated sentences which occurred in the text before, and the function of interconnecting the meanings of the old elements would be lost.

Consequently, a MTS is either an unmodified one-clause sentence in which all eventual modifiers and modifying clauses are carriers of the naming or instaurating (identifying, restrictive) function, not of the descriptive (non-

1) The term "text" stands here for written discourse. Only monologue texts are discussed in this paper.

2) The MTSs are presented in an "underlying" version, with their syntactical frames completed.

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restrictive) function. The MTS is supposed to be the minimal unit such as cannot be split without a considerable shift in the meaning of the text. On the other hand, "amalgamation" of the MTSs into longer sentences is optional.

2) *MTS order* in the text is carried by MTS-ordering factors of two types, the directed factors, which will be called primary, and the undirected or secondary factors.

The major *directed MTS-ordering factors*, which are also relevant for text T 1, are the relation of temporal sequence, the relation of intersentential implication and the relation of intersentential possible causation. The temporal sequence analysis (abbr. T) for text T 1 specifies the temporal ordering of the extreme admissible initial time points of the time spans covered by the main verbs of the MTSs, or, in case of their identity, of the extreme admissible final time points. The order of the initial points is generally preferred to the order of the final points on the basis of empirical experiments. The set of orders based on the implication (abbr. Impl.) is the set of sequences for which it holds that if the propositional content of MTS x is incorporated in the propositional content of MTS y , then necessarily x precedes y . The relation of possible causation (Poss) is based on a weaker condition than Impl, namely that the propositional content of x is a possible and expectable (more or less probable) condition of the truth value of the propositional content of y . The above-mentioned semantic relations may connect not only individual MTSs, but also MTS clusters. Often there is a number of paths (e.g. in the Poss representation of the T 1 between MTS 4 and MTS 22). There are also other directed sentence-ordering factors, which I treat as "modal or quasi-modal structures" (abbr. MSs). So the MS 1 manifests the tendency to order the MTSs from what is presented as a fact to what is presented as an observation on this fact, the MTS 2 is the ordering tendency from the negative part of an amplifying anti-thetic statement to its positive part, the MS 3 tends to be directed from the descriptive segment to an evaluation or an expression of a participant's attitude, the MS 4 from the generic to the specific, the MS 5 from a factive statement to a remark or comment on it, signalized explicitly. In this connection I speak about a tendency, because the reverse order is also admissible. Besides that, I also pass over the MSs which are not involved in the T 1.

The order based on the directed MTS-ordering factors is the most current unmarked MTS order in monologue texts. It will be called primary MTS order. The primary order is for any pair of adjunct MTSs identical with the order of nodes in the graph of some of the directed MTS-ordering factors for the T 1.

Undirected MTS-ordering factors may be represented in the form of a cross-reference table G of recurrent sememes³⁾ and the sentences of the text. The secondary MTS order is the MTS order which for any pair of adjunct MTSs is identical either with the primary MTS order, or with the order of the cor-

3) Cooccurrence of sememes (semantic elements) is now being elaborated on the basis of a frame model.

responding nodes in the Hamiltonian walk of the graph connecting the black squares in the table G. (The Hamiltonian walk adopted here is a modification of Depth-first Search. The search selects first the top of an arbitrary column of the table G and proceeds towards its bottom. Before passing a black square s in the table G, the search switches to the highest unpassed black square, located in another column above the square s . The procedure is repeated until all possibilities are discharged.) The number of possible solutions is enhanced by the fact that a cross-reference table can be compiled separately for each alternative primary MTS order and that there are also more alternative search versions. The output secondary MTS order is marked—it has its specific stylistic functions. The acceptability of this order rises when the column with the highest number of blacks is selected at the beginning of the experiment, and during the following steps always a column with the lowest number of blacks is preferred.

The relation between two MTs, separated by a MTS sequence in a way that violates the primary MTS order, will be called a "split in the primary MTS order". A pair of sentences which would be adjunct if the MTS order were transformed into a primary order is a sequentially split pair. The number of these pairs is an indicator of the degree of cohesion.

3) *Optional amalgamation* of the MTs into longer sentences can be performed on an indeterminate number of levels, for which it holds that on each level a pair of sentences is amalgamated. The levels and the permissible amalgamations are shown in an amalgamation diagram, which is constructed on the

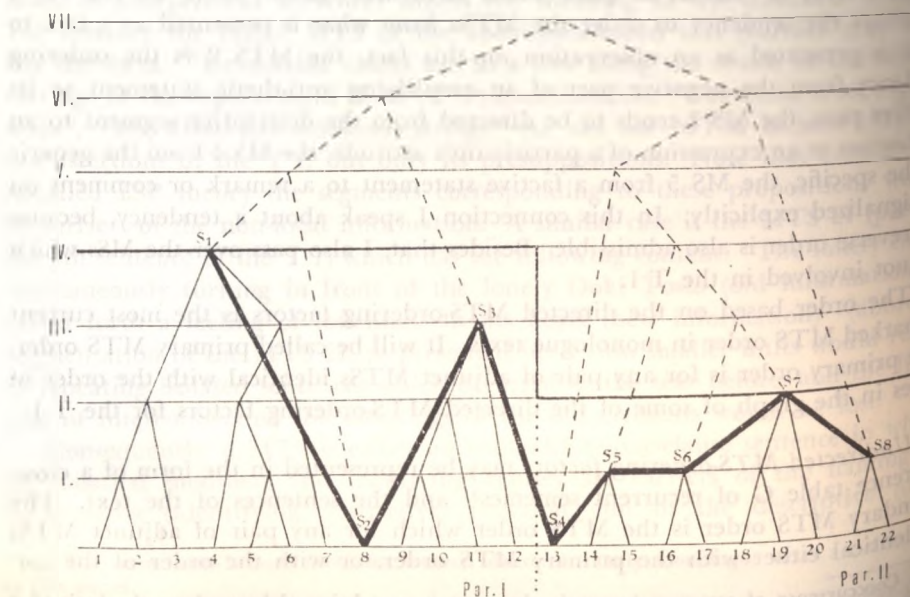


Figure 1. Amalgamation diagram for the text T I

basis of native informants' data. The optimal level of amalgamation depends, naturally, partially on a global factor, which is the type and the genre of the text. Besides, there are local factors which are to a certain extent subjective; for instance, short sentences are typical of emotional passages. Thus in the T 1 the MTSs 1~12, which correspond to the first paragraph of the text, are amalgamated up to the 4th level, and the MTSs 13~22, i.e. the second paragraph, up to the 2nd level, evidently due to the more emotional characteristics of the second paragraph (cf. the thick line delimiting the level of amalgamation). Another interesting point are the factors which determine the shape of the amalgamation diagram. The shape depends, of course, to a considerable degree on free choices made by the author of the text, because it is related to his or her interpretation of the text, mainly to the author's understanding of the major thematic oppositions participating in the semantic structure of the text.

Participation of major cooccurrent referents and major cooccurrent sememes in 'the underlying "frame-completed" versions of the MTSs enables us to segment the text into potential thematic blocks. An experiment on the T 1 shows that the six blocks isolated by this method roughly correspond to the actual sentences of Kawabata Yasunari's text⁴⁾. Only in two cases (Block I. and Block V.) does one block correspond to a sequence of two sentences. In both sequences the latter sentence is partially synonymous with the former, being however more specific. This type of a semantically more particular iteration is sometimes freed from redundancy and reduced into an elliptic subordinate-clause sentence (e.g. "Akamatu no yamayama ha semi no koe ni tutumarete ita. Musuu no mienai sou ga zyouzaizyu wo tonaete iru ka no you ni.—The red pine covered hills were full of cicadas' song. As if myriads of invisible monks had chanted a prayer against disaster."—Misima Yukio: *Kinkakuzi, Sintyou bunko*, Sy. 44, p. 46). The use of a specifying partially synonymous sentence in this position, eventually the use of a postponed elliptic subordinate-clause sentence⁵⁾, is associated with one or more of the following three presuppositions: a) the cognitive information carried by the sentence is of secondary importance, which is attested by the omissibility of the sentence, b) the sentence is a phenomenon of "secondary instance", i.e. its content is presented as supplemented later, c) the sentence is a functional carrier of a specific stylistic function as a consequence of its pretended use as an instance of b.

4) *Intersentential connectors* are optional carriers of the semantic relations between MTSs or MTS clusters. The set of all permissible scopes of a connector in the amalgamation diagram is the scope dominated by the node which is immediately above the lowest intersection point between the diagram and a vertical, dropped from that point of the horizontal axis of the diagram that corresponds to the location of the connector in the text.

4) Alternative segmentations presuppose selection of alternative oppositions of sememes as the basic oppositions, made by the author of the text.

5) Isolation of these sentences may be viewed as a case of clefting and contrasted with "pseudo-clefting" in the theory of syntax.

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Diachronic Textlinguistics

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Textlinguistics has been mainly a topic of synchronic linguistics, and practically only modern languages have been subjected to textlinguistic analyses. This becomes quite clear when we look both at the older and the more recent discussions and surveys of textlinguistics (e.g. Beaugrande 1980). Only very occasionally have particular aspects of older stages of a language been analysed. More often, examples taken from older texts have been used indiscriminately alongside examples from modern texts, thus reinforcing the idea that the same textlinguistic rules must equally apply to differing periods of a language. A typical example of this approach is Harweg (1980), who in his discussion of text-openings, includes texts from the Renaissance to the Modern English period—from Shakespeare's plays to 20th-century journals. But, as can easily be shown, the possibilities for text-openings have undergone changes within such a long period of time. It must not be taken for granted that textlinguistic rules for present-day English are also valid for the older periods of the language. What we need are textlinguistic analyses of, let's say, Old English (OE), Middle English (ME) and Early Modern English as a basis for diachronic textlinguistic studies.

One possible approach to a diachronic study of texts is suggested by the concept of intertextuality as it is developed by Beaugrande and Dressler (1981), who expound on the idea that every text stands in relation to certain other texts—which must of necessity have been produced before. In some texts such a relationship may even be explicitly stated, e.g. at the beginning of a letter reference is often made to the preceding correspondence. More important than these references, however, is the tradition in which texts are written: their cultural, social and situational context. It is the traditional form of a particular text-type that exerts the greatest influence on the production of new texts. Therefore, to return to our example, most business letters look pretty much alike; or to turn to an example in our own field of study, most textbooks of English grammar have a surprisingly similar structure. Textlinguistic changes may be due either to inner-systemic developments of a language or to a change of context in the widest sense of the word. The latter influences, some of which lie outside the scope of linguistics proper but which must be taken into account by linguists to arrive at a fuller understanding of texts, have only recently become prominent in linguistic and textlinguistic studies.

Let us begin with the influence of linguistics proper. Changes in the phonology and morphonology of a language may seem irrelevant to text-

linguistics but they influence at least the subset of poetic texts. New types of alliteration, assonance, and rhymes e.g. may become possible, which had hitherto been impossible. Consider the ME development of diphthongs or the Great Vowel Shift, always producing new sets of words that rhyme with one another, as e.g. Modern English (ModE) *hail, tail, nail, snail* (ME /ai/) with *tale, dale, ale, male* (ME /ā/). A loss of sounds may change the syllable structure, ME *marriage* (3 syllables) to ModE *marriage* (2 syllables), which has its effects on a metrical line; or, due to the loss of word-final <e>, we find a much larger proportion of masculine rhymes in ModE poetry than in ME. The whole character of Chaucer's poetry is changed if you don't pronounce the syllabic <e> at the end of a line, as some scholars have suggested. Changes in the morphology and syntax of a language can easily change the structure of texts. The change of the pronominal system (the set of deictic pronouns) from OE to ME and again to ModE involves a reordering of one of the main cohesive devices of English, which eventually leads to the ModE system as described by Halliday and Hasan (1976). What we lack is a historical description of the influences these changes, well-documented as they are, had on the structure and the inter-sentential relationship of OE and ME poetry and prose. The loss of the OE adverbial *pā* deprived speakers and writers of an important means of focusing important events and of structuring texts. Changes in the word-formation processes—derivational changes, new suffixes or prefixes—will give rise to new possibilities of reference and thus of sentence connection. The same holds true for additions to (or losses of) the vocabulary of a language. It is well-known that the development of the Expanded Tenses and the Passives in early and late ModE have opened up new cohesive possibilities, which again should be related to other developments in the language influencing the structural complexity of texts. Last but not least, standard topics of text-linguistic research, such as given and new information, focus, topic and comment, anaphora and cataphora, reference, repetition, ellipsis, and others, must be included in a diachronic textlinguistic study. I do not know, for example, of any exhaustive theme-rheme study of OE texts, which might yield very interesting results.

Let us turn to the influences of the context. A study of diachronic text-linguistics cannot omit an investigation of the relationship between texts and the evolution and further development of new text-types, which has led from a comparatively small number of text-types in OE or Old High German to the confusing number of text-types in ModE or German. Between languages contrastive studies will show how related text-types may differ considerably due to their different historical development and their cultural environment, cf. e.g. announcements of deaths and obituaries in English and German. The media in which texts occur will be one point to consider. With oral texts we have the well-known difficulties in historical studies that these texts are not recorded. For instance, we know very little about conversation in OE or ME. Even as simple a question as how people welcomed one another (a typical instance for

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text-openings) is difficult to answer with our present lack of evidence. The greeting *good morning*, for example, is first recorded around 1390. What did people use before? They hardly used *hail* and *welcome* all the time, as our written evidence would suggest (Fries 1982). Changes in the attitude towards memorizing texts (story-telling) certainly have an influence on text-types and text-structures. A history of writing systems and of printing could be profitably connected with their influence on text-production. One might even begin with the present-day computer-revolution and its impact on the formulation of texts. Even minor changes, such as the use of capital letters or of punctuation may bring about a change in the character of a text-type. A rule that one should not begin German letters with the pronoun *Ich* (*I*), which is increasingly violated or almost obsolescent today, may be due, apart from psychological changes in the attitude of the language users, to the custom of using a comma instead of an exclamation mark after the name(s) of the addressee, which in turn enables the writer to begin with a small (*i*) instead of the capital (*I*) of *Ich*.

The role of speakers and listeners in encoding or decoding texts must be described in diachronic textlinguistics just as in synchronic textlinguistics. Analyses of the global patterning of texts, the plans and scripts, the frames and schemas (Beaugrande 1980), which we can deduce from texts will reveal changes in the attitude towards texts, the linguistic behaviour of the producers and recipients of texts. An analysis, e.g. of the evolution, length and complexity of paragraphs, the ordering of individual elements in a text, the importance that is attached to experiential iconicism (Enkvist 1981), the development of new structuring devices (headlines, alphabetical ordering, the use of numbers), will show the changing ideas on the coherence of texts in different text-types. The audience of *Beowulf* had completely different ideas concerning the coherence of their text—consider the “highly sophisticated compositional technique” (Hart 1981) of the mathematical ordering of certain elements—than the audience of the XIIIth International Congress of Linguists has.

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Communicative Basis for Adequate Interpretation of Text Semantics

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Over the recent years there has been a considerable upsurge of interest not only in the formal structure of language but also in its semantic organization in the process of communication. It has been argued that text, or an integral number of utterances comprising both the referential and the illocutionary aspects of two-way communication, constitutes the basic unit of language-behaviour capable of conveying meaningful information. Coherent, or well-formed, text contains unambiguous information, and this is the criterion of its structural and semantic organization.

Looked at from the communicative point of view, text semantics can be defined as the sense accumulating all kinds of information involved in the process of speech communication.

It is true that the word and the sentence—two other kinds of linguistic entities—are also meaningful in that they relate to phenomena external to the language-system. However, they are units of nomination which face, as it were, the world of things, and their semantic information content is limited to the denotation of things and events in the material or the ideational sphere.

Text is a unit of communication; it is addressed to human beings, and it is this that determines text semantics, or the semantic information content of text. Therefore, text semantics can be defined as the sense integrating both the nominative and the communicative components of information. It cannot be limited to the analysis of factual situations, or the nomination of events. In other words, the interpretation of text semantics must reveal the sense which text acquires in the process of communication, i.e. the information not only about things but also about human activity (implicit and explicit objectives of the participants, their expected reactions, etc.).

Furthermore, the conditions for adequate interpretation of text semantics include the actual situation of communication (its spatio-temporal parameters, characteristics of the participants, their competence) as well as everything that can be described as "presupposition", or all the information that is prior to the generation of a given segment of text. All these conditions of communication which determine the sense and make it unambiguous for the participants represent a set of intra- and extralinguistic factors, or linguistic and paralinguistic context.

It should be emphasized that the sum total of meanings derived from individual grammatical sentence structures does not amount to what is the

sense of relatively complete text and cannot serve as the basis for its semantic interpretation. Indeed, the verification procedure for adequate interpretation of text semantics relies not on grammatical well-formedness but on the practical activity of people who use language as a means of social interaction in the course of which adequate interpretation leads to appropriate actions.

The semantic interpretation of individual utterances which is based on the meanings of grammatical types, or models, is essentially limited to isolated structures with a certain generalized sense. However, in text they can acquire a wide range of actual senses (for example, the grammatical structure of an interrogative sentence can contain a statement and vice versa—a declarative sentence can have the sense of a request, and so on).

Looked at from the nominative point of view, the elementary sentence "I am ill" describes the physical state of the speaker by virtue of its structure alone and not by virtue of its being a unit of communication, or a segment of text. In principle, such isolated structures are not inherent in language at all and represent only an extrapolation of actual utterances, i.e. the product of linguistic analysis. Used as an actual utterance in hypothetical text, the same structure can acquire a wide range of senses, as opposed to its structural meaning, e.g. "I can't work today", or "I am not going to the theatre today", or "I don't think I will be able to do anything", and so on. If this utterance is included in text as a unit of communication, it is only natural that its sense should be constituted not only by the nomination of a certain state-of-affairs but should also carry a certain message about it intended for some other person engaged in communication with the speaker. A simple test by synonymic substitution will show that in communication the same sense can adequately be rendered by numerous periphrases, cf. "No, I am ill", or "Very unlikely, I am ill", or "Absolutely, I am ill", and so on.

Looked at from this point of view, the sense is always contextually determined, because text generates its own context in the process of communication between at least two participants. This means that text may range in length from two utterances (either elliptical or non-elliptical) to infinity, at least in theory.

The sense of an utterance outside its textual framework is primarily centred on the denotatum, or a certain relation of objects which serve as subject-matter of its semantics. However, it is not linked with the situation of communication which can assign a value to it, i.e. a value for certain participants, apart from the denotatum.

The upshot of this discussion is that text constitutes a speechact, and its illocutionary force is to be found not in individual utterances but in entire text, or context—a fact which can be supported by frequently cited examples, such as "This room is stuffy", where an apparent statement is interpreted as a request or a command to open the window.

The procedure of semantic analysis on a communicative basis gives priority to the sense of text rather than the meaning of individual words, as has been

the case with descriptive linguistics.

The semantic analysis of text is much more complex than that of words and sentences. It is not limited to the denotatum of linguistic units, or nomination, but goes far beyond it to embrace the whole domain of communication as a complex information phenomenon. The analysis of text semantics consists, therefore, in the interpretation of the sense taking into account all the factors concomitant with real communication. Thus, in the example given above—"I am ill"—this interpretation should take into account the semantic information content of entire hypothetical text of which this utterance is only a segment (whether a reply to a question or part of a narrative), and thereby reveal its sense.

For all practical purposes, any problems arising in the interpretation of text semantics are resolved by specification, explanation, etc. For the purposes of linguistic theory, the construction of a set of rules governing the interpretation of text semantics is essential for making a correct assessment of the structure of language and laying down the guidelines for all kinds of language uses (in mass communication, translation, teaching, etc.).

Compared with the semantic analysis of words and sentences, text semantics is a far more complex task, because it embraces all aspects of communication and involves all kinds of extra- and intralinguistic factors, a wide range of presuppositions, the participants' communicative competence as well as takes into account the cultural and historical situation in which text is generated.

The Units of Dialogue

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As a text produced by more than one speaker, dialogue (D) is traditionally opposed to non-D textual forms presented by a single person uninterrupted by anyone else. One cannot appreciate full significance of this opposition unless priority of D to all the non-D types of text is taken into consideration. Life experience is essentially a D, and "the limits of the possibilities of D are the limits of awareness" (Buber 1929) D must be treated as a primary source of textuality, both from diachronic and synchronic points of view,—the most archaic type of written text was D (Ivanov 1976), and it remains, if hidden, in every type of text (Bakhtin 1963).

D is a juxtaposition of separate worlds that are trying to understand each other and each oneself at the background of another one. Besides the personal goals of the collocutors, and above them, there is "a common purpose, or set of purposes, or at least a mutually accepted direction" (Grice 1975) that brings a D to its objective destination, i.e., to a common stock of information created by mutual efforts of the collocutors, and rather neutral to each of them. In a way, this knowledge is depersonalized and alienated from each of the partners, so that neither of them is in full personal possession of the information pool attained in the course of D. Textual structure of D bears distinct marks of the tough process of mutualizing and objectivizing personal opinions of the collocutors.

A D is produced by at least two alternating speakers (α and β) that interchange their speeches, or *repliques*. A *replique* (R) is a continuous fragment of D located at a continuous space of time correlated with the time-spaces of other R's in such a way that $R(\alpha, t_i)$ uttered by α at t_i is immediately preceded by $R(\beta, t_{i-1})$ and immediately followed by $R(\beta, t_{i+1})$. As a D-unit, the R is not equal to and is even incommensurable with any of syntactic units. Though the border of R usually coincides with a sentential border, a sentence, or even a phrase may also be distributed among the collocutors. Actually, R is not a syntactic unit,—it is a communicative unit laying in the foundation of an independent hierarchy of D-units. From the point of view of D-interaction, a single R can hardly be assessed as D-meaningful, since the D-meaning of R has pragmatic components that are revealed on the background of other R's related to the R, especially of the response(s) to it. The basic nucleus of D is a pair of R's (R-pair) related to each other as a stimulus to a response: $[R_{stim}, R_{resp}]$. The complete D-meaning of R_{stim} is created partly by its speaker, and partly by its hearer,—since a speaker of R_{resp} retrospectively provides R_{stim}

with a conversational implicature (CI) that must be incorporated in the contextual meaning (CM) of the latter. For ex.:

- (1) R-1. TITANIA. What, wilt thou hear some music, my sweet love?
 R-2. BOTTOM. I have a reasonable good ear in music: let us have the tongs and the bones.
 R-3. TITANIA. Or say, sweet love, what thou desir'st to eat.
 R-4. BOTTOM. Truly a peck of provender: I could munch your good dry oats. Methinks I have a great desire to a bottle of hay: good hay, sweet hay, hath no fellow.

(W. Shakespeare. *A Midsummer-Night's Dream*, IV, 1)

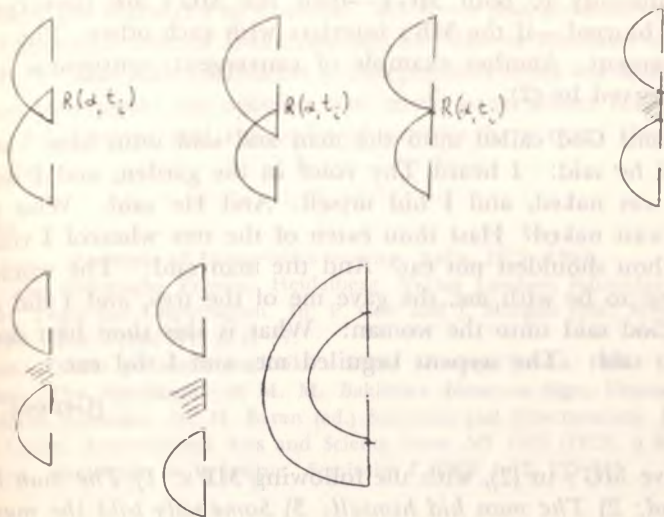
R-2 implies: *B. will be pleased by music*, which imparts to R-1: *T. suggests to please B. by music*. But then R-3 implies: *T. prefers to please B. by food*,—which imparts to R-2: *B. prefers to be pleased by food*. The two implicatures come to contradiction in R-2,—which fits well the ambiguity of R-2. The ambiguity is resolved by R-4. R-4 implies: *B. likes rough food*, which imparts to R-3: *T. is right in suggesting B. food.*, and endorses one of the two implicatures, at the expense of another.

The ultimate D-meaning of R is many-dimensional, since the meaning conveyed by the surface form is enriched by the outgoing CI (aimed at the R_{stim}) and in-coming CI (sent by R_{resp}). Therefore the D-meaning of "the last word" that is not affected by the response of the hearer, is relatively poor; the same, to less extent, can be said about the first R of a D, that has no R_{stim} to transmit any CI to the latter.

A D is a succession of concatenated R-pairs: each R appears first as R_{resp} at t_i , and then most of R's turn into R_{stim} of the subsequent R-pair, at t_{i+n} . Hence, the R-pairs are coupled:

$[R(\beta, t_{i-n}) - R(\alpha, t_i)] \circ [R(\alpha, t_i) - R(\beta, t_{i+n})]$. The three R's are mostly adjacent, but can also be detached. The couples of pRairs are tightly connected by the mediating $R(\alpha, t_i)$ that projects its influence in two directions: to the R_{stim} of the 1st pair and to the R_{resp} of the 2nd. The mediating R can be involved in each of the two pairs as a whole, or in parts; it can also be absent (then the couple is cleaved in two separate R-pairs). Accordingly, several types of coupling arise. (Scheme 1).

The succession of R-pairs makes a pre-thematic foundation level of the D-structure. The pairs are further arranged by *motif-groups* (MG) that form a higher, thematic level of D. A *motif* is a minimal element of content presented by, or, rather, equal to its MG. For the classification of motifs (or, which is the same, their MG's) two questions are important. First, whether the particular MG was worked over by one or by more than one person; second—whether the MG consists of R-pairs or of unpaired R's. There are three kinds of motifs: An *interaction motif* (I-motif) is equal to the MG that consists of R-pairs worked over by two or more persons. Here, each R by α gets a response by β . A *free*



Scheme 1. Types of coupling the R-pairs.

motif (F-motif) is equal to the MG that consists of unpaired R's uttered by one person only. Here, no R by α gets a response by β . An *auto-motif* (A-motif) is equal to the MG that consists of R-pairs worked over by one person only. Here, each R of α gets a response also by β . An F-motif arises when a person is not heard and not responded by his partners, though he addresses to them; an A-motif arises when a person is speaking to himself, expecting and providing auto-responses. A-motifs occur either as soliloquies, or as degenerative D of two or more persons each speaking to himself. Both A- and F-motifs are deviations from genuine D-interaction. Most of the MG's in a D are I-motifs.

Within a MG, the Maxim of Relation works at its full power, keeping a special internal microclimate, a kind of pragmatic greenhouse: For each R of the MG the same deep proposition, i.e., the same *motif-pattern* (MP) can be revealed, and the CM of lexical items are homogeneous. Relaxation of the Maxim means that the MP is changed, i.e., the current MG ends and the next MG starts. E.g., there are two MG's in (1) above. In the 1st of them, *the tongs and the bones* are interpreted as musical instruments; in the 2nd, they are assigned to the *desire to eat*, and then equalled to a *peck of provender*, *dry oats* and a *bottle of hay*. The 1st MP is: *B. will (wants to) hear music*; the 2nd is: *B. has a desire to eat*. Mediating between the two MP's is the 2nd sentence of R-2, where the switch of the meanings takes place.

The change of MP's may occur more or less smoothly,—this depends on the place where the border between the MG's is set. It may be set between two R's, then the adjacent MG's are *separate*: between the parts of the same R,—then the MG's are *contiguous*; if the change takes place within a R, so that it

belongs simultaneously to both MG's,—then the MG's are *convergent*. The borders may be blurred,—if the MB's *interfere* with each other. The two MG's of (1) are convergent. Another example of convergent, contiguous and interfering MG's is served by (2):

- (2) And the Lord God called unto the man and said unto him: Where art thou? And he said: I heard Thy voice in the garden, and I was afraid, because I was naked, and I hid myself. And He said: Who told thee that thou wast naked? Hast thou eaten of the tree whereof I commanded thee that thou shouldest not eat? And the man said: The woman whom Thou gavest to be with me, she gave me of the tree, and I did eat. And the Lord God said unto the woman: What is this thou hast done? And the woman said: The serpent beguiled me, and I did eat.

(Genesis, 3, 9-13)

There are five MG's in (2), with the following MP's: 1) *The man heard the voice of the God*; 2) *The man hid himself*; 3) *Somebody told the man that he was naked*; 4) *The man did eat of the tree whereof the Lord God commanded him not to eat*; 5) *The woman gave the man of the tree, and did eat herself*. The structure of (2) is shown in Scheme 2.

The 2nd and the 3rd MG's are convergent with the 1st MG and with each other; the rest of the MG's are contiguous. Each of the five MG's is interfering with the adjacent MG's. (The interference of MG's is indicated in the Scheme by shading, convergence—by intersecting quadrangles, and contiguity—by adjoining quadrangles).

It is easy to see in the Scheme the main features of the passage (2): a powerful drive issued from the opening cluster of three convergent MG's, rapid linear development of short MG's by contiguity, and, finally, viscous texture created by tenacious discontinuity of the MG's.

Closely interconnected sets of MG's form the units that belong to the next thematic level, with motifs as constituents. The sets of MG's take a shape that may be called "a ring of motifs" (when the MG's of the set are embedded in one of them), "a network of motifs" (when the MG's weakly interfere with each other), or "a clench of motifs" (when the interference reaches its peak, as it occurred in (2)).



Scheme 2. Motif structure of ex. (2).

To review the hierarchy of D-units: an R—a non-D-meaningful unit; an R-pair—a meaningful unit of D-interaction; an MG—a minimal thematic D-unit; a set of MG's. The scale of D-units is independent from the syntactic hierarchy. This comes to support the opinion that there is no direct correlation between the units of utterance and the units of content in a text. (Ivanov 1973, Benveniste 1969).

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Spectrum, Profile and Constituency Structure in Text Analysis

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0. Ultimately text analysis enables us to explain much of the morphosyntax of a language by observing the interface of the morphosyntax and the text. The assumption is two-fold: the whole is necessarily contained in and expressed in its parts; and the parts are to be explained in reference to the whole. In this paper I illustrate by reference to a limited corpus of English narrative prose the *spectrum* of varied verb forms which occur in a narrative; I also briefly indicate surface structure reflections of mounting tension in the development of a story; and how a rank-scheme of verb forms can be an important clue to the constituent structure of the paragraph.

The following paragraph from Mark Twain serves as the limited illustrative corpus of this study:

"In a minute a third slave was struggling in the air. It was dreadful. I turned away my head for a moment, and when I turned back I missed the king! They were blindfolding him! I was paralyzed; I couldn't move, I was choking, my tongue was petrified. They finished blindfolding him, they led him under the rope. I couldn't shake off that clinging impotence. But when I saw them put the noose around his neck, then everything let go in me and I made a spring to the rescue—and as I made it I shot one more glance abroad—by George! here they came, a-tilting!—five hundred mailed and belted knights on bicycles!"

Mark Twain. *A Connecticut Yankee in King Arthur's Court*
(An Airmont Classic, 1964, p. 240)

In this story a 'Connecticut Yankee' finds himself transported to the early medieval world of King Arthur's Court in England, and sets about to change things (including introducing the bicycle for the knights to ride on). One of the crises of the book ensues when a band of ignorant villagers capture King Arthur and his party (including the Yankee aide) and in a display of xenophobia decide to hang them—not recognizing the king as one of the party.

1. The spectrum (rank scheme) of verb forms.

1.1 The verb system of a language exists to facilitate discourse. Differing tense/aspect/mood/voice forms perform differing functions in the design of the whole. In that the paragraph here analyzed is narrative, dynamic verbs predominate. Most English narrative is told in the simple past tense, i.e. simple

past tense forms report successive actions and events which propel the story forward. In this paragraph the simple past tense forms which apparently perform this function are: (1) *turned away (my head for a moment)*; (2) *turned back*; (3) *missed (the king)*; (4) *finished (blindfolding him)*; (5) *led (him under the rope)*; (6) *saw (them put the rope around his neck)*; (7) *let (go in me)*; (8) *made (a spring)*; (9) *made (it)*; and (10) *shot (one more glance abroad)*. I discuss below *came in* *came a-tilting* which patterns a bit differently.

Interspersed among these story-line verbs are other forms which have various supportive and depictive functions. Thus, past progressive forms occur in the sentences: *In a minute a third slave was struggling in the air*, and in *They were blindfolding him*. These clauses report activities that are not viewed as punctiliar and are not in the strict sequence of actions/events; rather what they report is more in the nature of concomitant activities.

The verb *to be* occurs with adjectives and participals in stative/depictive sentences—which in this paragraph report the narrator's (the 'Connecticut Yankee's') evaluations and feelings: *was dreadful*, *was paralyzed*, *was choking*, *was petrified*. In somewhat similar function occur negative modals in this passage: *couldn't move*, and *couldn't shake off (that clinging impotence)*.

Having looked at story-line (past tense) verbs, past progressives, descriptives, and modals as they are woven together into the text, we now need to re-examine the function of certain of the so-called story-line simple past tenses. Notice that three of the simple past tenses occur in adverbial clauses: *when I turned back*, *when I saw (them put the noose around his neck)*, and *as I made it*. Each of these is a *back-reference* whose main function in the paragraph is cohesive rather than advancing the story-line. Thus, *when I turned back* refers to the previous clause *I turned away my head for a moment*; here we have a cohesive back-reference within the bounds of a coordinate sentence. The clause *when I saw them put the noose around his neck* is a script-based back-reference to *they led him under the rope* in that the actions reported are in predictable sequence. Finally, *as I made it* is a very clear back-reference to *I made a spring to the rescue*. All these adverbial clauses are grammatically and semantically subordinated to the story-line by virtue of being essentially linkage mechanisms. It follows, therefore, that the simple past tense verbs in such adverbial clauses are not part of the story-line of the paragraph.

One more verb remains to be discussed: the verb *came in* *here they came a-tilting*. Notice first of all that the clause in question occurs as part of the report of what the narrator saw; we shouldn't be surprised if it is essentially depictive. Notice also the progressive form *a-tilting* which occurs somewhat epexegetically following the verb *came*. It seems, therefore, that some simple past tenses in English are implicitly continuative and are more functionally akin to past progressives or even to statives. Indeed, the following two sentences in the Mark Twain passage are: *The grandest sight that ever was seen*. Lord, how the plumes streamed, how the sun flamed and flashed from the endless procession of webby wheels.

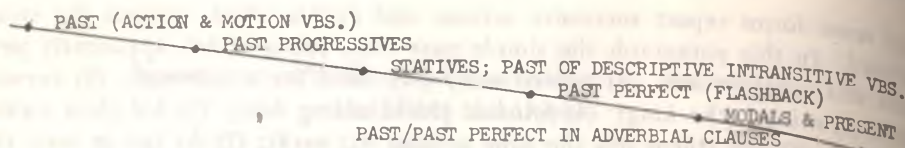


DIAGRAM: SIMPLIFIED SCHEME OF ENGLISH VERB RANKING

1.2 We are now prepared to arrange a rank scale of verb forms for English narrative so that the most dynamic forms occur at the top of the scheme and the more static forms at the bottom. Such a scheme is given in the diagram. In this scheme, the simple past (labelled "past") is at the upper left-hand side, while verbs in cohesive function in adverbial clauses are in the lower right-hand side; they are so far removed from the story-line that they function much like conjunctions. Other forms are ranged between: the past progressive, for concomitant activities, runs a close second to the story-line. Down two pegs in the scheme come statives and descriptives (such as *it was dreadful*) and the past of descriptive intransitives (such as *came, streamed, flamed, and flashed*). Past perfects, still a peg lower, do not occur in the above sample. Immediately after the first sentence we could, however, have had a parenthetical sentence: *They had already hung two slaves*. Such a sentence would have been a fairly routine use of the English past perfect in *flashback* function, i.e. for reporting an event out of its chronological order. Next to last in the scale occur modals and present tenses. Negative modals are illustrated in the paragraph here considered. The use of the present in a gnomic sense in a story could be illustrated in such a sentence as the following, were it to occur somewhere in the above piece of text: *That's what people do who hate strangers and regard all outsiders with suspicion*.

It must be cautioned that the above scheme applies only to narrative. Furthermore, in its present form it is tentative and needs to be subjected to cross-checking and elaboration. In some English stories, e.g. a whole story is given in an 'historical present'. In others, there may be a shift from past tense to present at a great moment of the story (see below).

2. Some further concerns

A story is not told at a uniform level of excitement but rather has points of maximum tension or release of tension which are specially marked as *peaks*. Even a fairly brief unit, such as a paragraph, can have such a peak. In the passage under consideration here, the climax of the paragraph is marked by two mild oaths *By George!* and *Lord!*, by use of assonance and alliteration (*flamed, flashed, webby wheels*) and by resort to a depictive passage which, in effect, opens a window on a larger world.

The verb ranking scheme can be used to yield a tree structure of the paragraph. While there is not space to present such an analysis here, note the following: (1) While sentences one and two constitute a unit, the past progres-

sive *was struggling* outranks the stative *was dreadful*; the whole is a Text-Comment unit in the larger framework of the paragraph (to which it serves as Setting). (2) Likewise sentence three with its simple past tenses outranks four which has a past progressive; the two constitute a Text-Reason unit in the broader structure. (3) Until the action line picks up again with *finished blind-folding* and *led*, I assign the intervening statives and modals (of sentence 5) lower status relative to three and four; three and four is again a Text with the complex of statives and modals serving as Result. (4) The whole paragraph turns on the word *But* in paragraph medial. This marks off the first half of the paragraph (reporting actions and the narrator's inability to react) from the second part wherein the narrator flies into action. Short of the paragraph climax, story-line verbs internally dominate both parts of this *antithetical paragraph*.

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- b. Sono ato ni tsubasa no tenshi no mure ga utau uta no yoo ni ooku no uta ga dooji ni kikoehajimeta.
 Sore *wa* gocchamaze ni natte hitotsu no oto to natta.
 Sono oto *mo* aru kimyoono shirabe no bansoo ni sugizu,
 umi no ue no moya no yoo ni, shinpi no oto no taikai ni tadayou ka ni mieta.
 (Koojiro 1963, p. 296)

$$\begin{array}{c}
 T_1 \rightarrow R_1 (= T_2 (= R_1) \rightarrow R_2) \\
 | \\
 T_3 (= R_1) \rightarrow R_3 \\
 | \\
 T_4 (= R_3) \rightarrow R_4 \\
 | \\
 T_5 (= T_4) \rightarrow R_5
 \end{array}$$

If we create a structurally near-equivalent Japanese sentence to that of English, as displayed by (1c), we find a rather serious gap of information, where new information must presuppose information that can only be given later.

- (1) c. Sono ato ni tsubasa no tenshi no mure ga utau uta no yoo ni gocchamaze ni natte aru kimyoono shirabe no bansoo ni sugizu, umi no ue no moya no yoo ni, shinpi no oto no taikai ni tadayou ka ni mieta hitotsu no oto to natta ooku no uta ga dooji ni kikoehajimeta.
 (fabricated)

$$\begin{array}{l}
 T_1 \rightarrow R_1 (= (T_2 (= R_1) \rightarrow R_2), (T_3 (= R_1) \rightarrow R_3 (= (T_4 (= R_3) \rightarrow R_4), \\
 (T_5 (= T_4) \rightarrow R_5)))
 \end{array}$$

Sentence (1c) is not a preferred form in Japanese since the flow of discourse is disrupted and the gap in information distribution makes it less desirable. (Note that R_1 presupposes elements in parenthesis, T_2 through T_5 which themselves presuppose R_1 through R_3 .) This is an extreme case where the principle of information flow is violated if relative clauses are used in Japanese.

Note that in data (1), the Japanese translation uses along with the order of elements, particles which convey the given/new dichotomy, namely, *wa/mo* and *ga*. Although not all occurrences of *ga* indicate new information, in this instance the writer uses the functional difference of *ga* and *wa/mo* effectively. To Language is equipped with a variety of devices to accomplish cohesion. To overcome the shortcomings of restricted use of the relative clause—caused by the difference in basic word-order—the Japanese language is equipped with the theme marker. The intersentential connection is made clear through the use of the so-called theme markers. Out of 43 cases of discrepancy, this thematization strategy was used 25 times.

Let us now turn to our second concern, the issue of functions of relative clause in discourse. Observe data (1a) again. The theme established in (1a) is "far-off hymns." By relativizing the description of "hymns" and "a melody,"

(1a) signals that these relativized clauses represent background information subordinate to the main theme. In terms of thematic hierarchy, "far-off hymns" holds a higher and therefore dominant position over the description of "hymns" and the description of "a melody." If relativized clauses become independent sentences (i.e., They, nevertheless, ... The melody seemed to ...), these sentential themes are not as subdued as appear in (1a). In fact, although semantically these themes still represent subordinate information, they are more prominently activated in the reader's consciousness. The function of relativization in English, then, is to place themes in a hierarchy (i.e., hierarchical linking) and to signal which theme is to be integrated with which. As observed in our data, by using many relative clauses rather than separate sentences, the writer provides information chunking whose inner thematic hierarchy is specified. Consequently, by signalling how information ought to be integrated, relativization contributes to cohesion in discourse organization.

Due to stricter constraints on the use of the relative clause in Japanese, where only the so-called restrictive relative clause is used, whose function is to identify one referent (i.e., the head noun) among other possibilities by specific descriptions often consisting of given information, the theme/rheme linking replaces relative clauses and plays an essential role in arranging the theme hierarchy. By limiting the relative clause, the speaker sustains a smooth flow of discourse in Japanese. It is stressed that the desire for smooth flowing discourse motivates various linguistic manipulation, including the Japanese theme/rheme frame which partially replaces the discourse functions of the English relative clause.

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Discourse Analysis of Translation

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1983
In my paper I should like to pursue two aims. First, I want to talk about translation and try to put this fast-growing communicative activity into the focus of linguistics, and text-linguistics in particular. Secondly, I shall reverse my view-point and concentrate on linguistics, again mainly text-linguistics, but in the light of translation studies and their particular insights. As we shall see, the two aspects are an instructive case of complementarity, a principle highly pervasive in all fields of human study and understanding. (1) The result and, more recently, the process of translation have been subjected to the most diverse kinds of investigation ranging from the avowedly subjective (in translation criticism) to the rigorously formal (in MT programmes). The predominant trends surfacing in the course of translation history would be an interesting topic in its own right. Within the last two or three decades traditionally more general, primarily prescriptively and, very often, aesthetically oriented approaches have systematically narrowed down to become more and more linguistically based endeavours. Fuelled by the need to train a growing number of professional translators for the most diverse fields, translation theorists and teachers have been compelled to single out and systematize grammatical, lexical and stylistic stumbling blocks which the future translator and interpreter will have to cope with. This utilitarian enterprise, although it has proved to be useful to a certain extent, has tended to fail to see the wood for the trees. Emphasis on how structures, words and figures of speech are translated, surely on the basis of contrastive grammar, lexicology, and stylistics of source and target languages, yields at best partial insights into the building blocks of translation. It may even blur the picture because it supports the illusion that we are actually translating structures, words and stylistic features when, in actual fact, we are translating texts. Texts, however, are not just sequences of grammatically framed words with a particular stylistic profile. They represent a structural level of their own, as recent studies of text linguistics have demonstrated. The actual *units of translation*, then, are not constituted on the levels of syntax or lexis, although they may be said to be shaped or represented by words in construction. Good or rather equivalent translations very often consist of grammatical structures and lexical material which are quite unexpected from the point of view of the contrastive grammarian or lexicologist. This is precisely because they are the result of a "top-down" procedure, a reconstruction of the source text under the textual conditions of the target audience. Instead of a "bottom-up" rendering of source language

material with target language correspondences the translation process is a holistic activity always keeping the whole textual product in mind when making each individual choice of words and structures. In order for the text world of the source to find its match a target text world has to be constructed. The respective text worlds are couched in different discourse conventions. *Translatability*, so often put into doubt because of glaring discrepancies between the grammatical and lexical source and target language systems, resides and is solved—if ever—solely on the textual level. It is, primarily, a function of the need of members of the communicative communities to put across and/or to receive a particular type of text. Under pressure of this urgent need which is as old as mankind and which has become of paramount importance particularly in our century all target languages, without exception develop textual conventions to assimilate source text worlds into their own.

The professed aim of text-linguists to describe and explain the intricate, but rule-governed interplay of discursive strategies and linguistic implementations characteristic of the users of a particular language coincides directly with the translation student's task of selecting just those target signs that are most likely to satisfy the target reader's pattern of expectation.

It goes without saying that the predictive power of textlinguistic assumptions about the structural features of texts varies from text type to text type. At one end of the scale we have the highly conventionalized text-classes such as—with growing complexity—stereotyped messages, legal formulas, instruction manuals, official and business correspondence, scientific papers etc. At the other end where almost nothing seems to be normative and thus predictable we have poetry and the various types of creative writing and speaking, although even here, as e.g. research into the discursive characteristics of story-telling has demonstrated striking formal features can be shown to be recurrent.

The crucial problem as to what constitutes the structured textuality of a piece of communication is the *repertoire* of principles and rules governing the syntactic and lexical representation of a text within a predictable "band-width". We have to admit that the instruments of text-linguistics to dissect stretches of discourse into patterns of coherence are still very blunt. At the moment the most promising methods are, in our opinion, the following, (a) analysis of global structures into hierarchies of macrostructures down to a microstructural level where the grammatical rules of cohesion take over, the interdependence of macrostructures being governed by macro-rules which set up a relatively simple tree-structure (van Dijk); (b) analysis into scripts and frames which characterize a number of communicative activities in a more or less predictable form allowing for variations at specifiable nodes in the discursive chain (Schank, Abelson); (c) setting up of text-world models where textual control centres branch out into dependency networks encompassing the sequences of primary and secondary concepts of a text into an integrative discursive whole (Beaugrande); (d) establishing a discursive description of form and content ranging from the upper level of the whole text via subtexts down to complex

and eventually simple sentences where external and internal conditions share in constituting a limited number of semantic relation types obtaining between textual units of varying complexity with propositional bases identifiable at every level of textual structure (Graustein, Thiele).

Whatever these and other methods to come to grips with discourse structure have singled out as their guiding principle, they are all bound to come up against a very serious problem which runs through any text-linguistic endeavour, a problem which grammarians of the sentence have long since learnt to disregard, viz. the intrusion or rather relevance of non-linguistic *knowledge* in the purposeful use of language, more specifically, in the planning and internalizing of discourse. The awareness of real life situations, of the social embeddedness of discourse, of the functioning of linguistic communication as a means of grappling with our natural and social environment, and of actively transforming the real world into a human place has led many discourse analysts to widen their linguistic frames of reference into what has been called a text scientific scope adopting a truly interdisciplinary stance which tries to combine both textual structures and discourse meaning, textual macrostructures and textual macropropositions. (2) In the light of translation this widening of discourse analysis into a science of texts is particularly promising, since it opens the way to a better understanding of how different speech communities and their respective communicative groups tend to codify equivalent pictures of the world into varying textual shapes. This leads me naturally to my second point, in fact, we are already right in the middle of the argument concerning the translational implications for discourse studies. Whereas "normally" texts occur and are consequently analyzed as crucial modes of social interaction in a monolingual setting, translations offer the unique opportunity of a controlled experiment in text-production and consequently textual analysis. In contrast to all other kinds of textual activity representing the normal cycle of knowledge \rightarrow text \rightarrow knowledge translations are *text-induced texts* duplicating so to speak two text worlds and enabling the analyst to study in detail which textual variants have been selected with the global structures and propositions being kept constant. Based on empirically verifiable *transpositions* (syntactic) and/or *modulations* (lexical) between source and target texts, generalizations as to *text-type specific variations* allow unexpected insights into *textual structures* which are the direct result of the translational perspective.

Discourse analysis of translation, then, is both a novel research tool and a scientific method to improve the very process of translation itself. As I pointed out at the beginning of my paper discourse analysis and translation are complementary ways to clarify and, I add, to optimize each other. They are, furthermore, a striking example of the expanding process of cross-fertilization which is typical of modern interdisciplinary research enriching theory and giving orientation to practice.

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What are the Main Semantic-Pragmatic Features of Stylistic Text Types?

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1186 According to many stylistic schools, style is a property of text—of every text. What, then, is this property? The chief question is whether style is a matter of merely the level of expression or whether it also belongs to the level of meaning, and if it does belong to the level of meaning, what part of the meaning is stylistic.

The stylistics that, up until now, has tended to exclude the semantic variation from the scope of style (see Talbot J. Taylor, *Linguistic Theory and Structural Stylistics*, Oxford—New York 1980, p. 17) has not been able to demonstrate what is synonymous variation at the expressional level and what is not, and if it were possible to isolate the synonymous variation, the scope of style would remain so restricted as to be of hardly any significance. Since style has generally been interpreted as a textual phenomenon, we should be able to demonstrate the synonymous variation in texts in particular. Is that generally possible even theoretically (in longer texts)? It is certainly possible in language to express the same thing in different ways even at the level of text, but that is a matter of dialectal variation, as Halliday has pointed out.

Faced with a blind alley, investigators have been obliged to define stylistic variation by saying that "roughly the same" content is expressed in alternative ways. This means that some semantic phenomena must be included in style, as certain authors clearly point out.

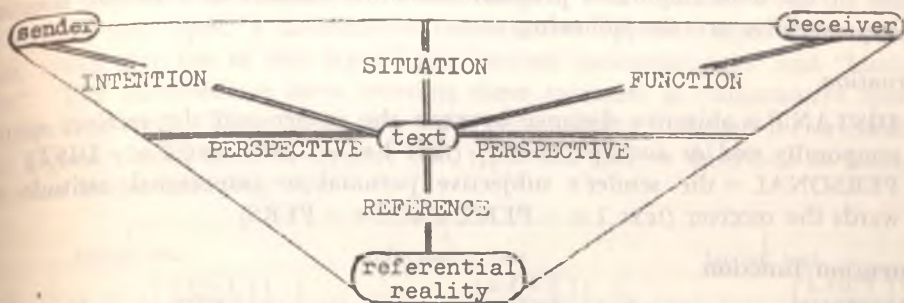
After this, we still have the problem of which part of the meaning is style and which is not. The total meaning of the text should have an identifiable invariant core which is not style. After all, not all properties of the text can be considered stylistic, because the concept of style would in that case lose its significance and become equal to text.

A suitable starting-point for theoretical considerations is the definition of "synonyms" being expressions with the same object of reference or the same denotation (e.g. Willy Sanders, *Linguistische Stilistik*, Göttingen 1977, p. 16, 22). One might say that the textual denotation with the same object of reference represents the ultimate core of the text, and the different semantic styles for expressing the same denotation are textual connotations (which are realized in certain grammatical and lexical expressions).

What would then be the connotative meaning of the text? In semiotics, a linguistic sign—in this case a text—has a two-fold relation to its extralinguistic environment: a relation to the referential reality and a relation to the users.

The former causes denotation and the latter connotation.

When the relation to the users is analyzed in more detail by taking into account the sender and the receiver separately, their mutual relation and their relations to the referential reality, it appears that the theoretically possible relations of a text to all these are the following:



The four different relations of text, reference, perspective, situation and intention/function as a common relation, represent the immediate systemic environment or context. The relations are pragmatic, but they are represented in the text as semantic components. (In accordance with the model of Halliday's system, it might be said that they activate situation, perspective and reference components in the text.) These four semantic levels thus have a direct, genetic causal relation to the extralinguistic context, i.e. the text and the context have a content-structure correlation.

Situation, intention/function and perspective—which are due to the users of language—together cause the generic connotative meaning of the text, i.e. the style, while reference causes the denotative meaning.

The following examples illustrate stylistic variation with the same textual denotation. The common referential reality of texts 1, 2, 3 and 4 is roughly the following: a thief, while collecting objects, drops a vase brought by the family from Italy, and the vase breaks into pieces.

The semantic-expressional differences between the texts 1 and 2 depend mainly on the situation:

- | | |
|---|--|
| 1. The wife's report in the records of the police:
"The thief had also broken a valuable blue Italian vase that had been kept on the sideboard." | 2. The wife's phone call to her husband, when she saw what had happened:
"He had even broken that blue vase." |
|---|--|

Different perspectives and intentions/functions cause the main differences between texts 3 and 4:

- | | |
|---|--|
| 3. The thief's companion at the time of the incident:
"Now the thing's smashed to pieces." | 4. The wife to her friend:
"It's awful that the scoundrel had even shattered that gorgeous vase into little pieces on the floor." |
|---|--|

Situation between the sender and the receiver can be described by the dimensions of distance, direction and attitude. What I consider the main functions of text are appeals to the user's emotion, reason and will, which are considered the three major regions of man's cognitive functions by psychology and philosophy. Perspective means the user's point of view to the referential reality. Some of the most important pragmatic-semantic features of situation, function and perspective are the following:

Situation

- ± DISTANT = objective distance between the sender and the receiver spatially and/or socio-psychically (text 1 = + DIST, text 2 = - DIST)
- ± PERSONAL = the sender's subjective personal or impersonal attitude towards the receiver (text 1 = - PERS, text 2 = + PERS)

Intention/function

- EMOTIVE = text appeals to the receiver's emotion (especially text 4)/
- INTELLECTUAL = text appeals to the receiver's reason
- VOLITIVE = text appeals to the receiver's will

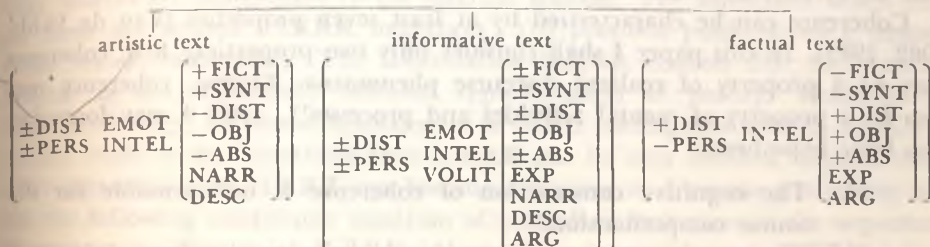
Perspective

- ± FICTIVE = the referential reality is seen directly as it is or indirectly with fictive modification
 - ± SYNTHETIC = the reality is seen in synthetic images, metaphors, or analytically as consisting of separate parts
 - ± DISTANT = temporal, spatial and psychic distance from the referential reality (text 3 = - DIST, text 4 = + DIST, relatively speaking)
 - ± OBJECTIVE = attitude, which expresses objectivity or different kinds of subjectivity (text 1 = + OBJ, text 3 = - OBJ DEROGATORY, text 4 = - OBJ CONDEMNING EXAGGERATING APPRECIATING)
 - ± ABSTRACT = the referent is seen as a single concrete phenomenon or an abstract generalization
 - EXPOSITORY = the referent is structured thematically by identifying and combining phenomena/
 - NARRATIVE = the referent is structured thematically by presenting temporal relations between phenomena/
 - DESCRIPTIVE = the referent is structured thematically by presenting locative relations between phenomena/
 - ARGUMENTATIVE = the referent is structured thematically by presenting causal and other relations between phenomena
- (The last four features are complementary; Egon Werlich, *Typologie der Texte*, Heidelberg 1975, p. 30.)

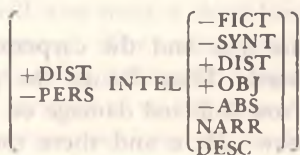
The situation, function, perspective and reference features seem to be hierarchical with the reference features lying at the deepest level: (situation function (perspective (reference))),

When we classify texts semantically into text types, we can employ either referential (denotative) or stylistic (connotative) features. There are thus referential text types (e.g. medical texts) and stylistic text types (or styles or style types).

The situation, function and perspective features are combined in certain constant ways in communication. Those constant combinations form the basis of stylistic text types. I differentiate between two opposite types of combination, which give rise to text types I call "artistic (aesthetic) text" and "factual text". The intermediate form between these extremes is "informative (communicative) text". These three text types consequently make up hierarchically the highest classification of stylistic text types and can be described as feature matrices in the following way:



The "informative" text type may be said to include e.g. journalistic texts and administrative texts, and the journalistic type subtypes such as *news*, the feature combination for which would be the following:



Coherence Relations in Texts and Inferential Processing

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1. On coherence and inferential processes

Coherence is the sun in the cosmos of discourse. This paper will attempt to clarify the meaning of this metaphor. In so doing, I shall focus on the reception of written discourse.

Coherence can be characterized by at least seven properties (Van de Velde, 1982, 1983). In this paper I shall consider only two properties: first, coherence may be a property of realized discourse phenomena. Second, coherence may also be a property of mental activities and processes¹⁾. Here I may formulate two basic hypotheses:

H₁ The cognitive construction of coherence is indispensable for discourse comprehension.

H₂ The cognitive construction of coherence results from inferential processing²⁾.

Let me provide an example which involves some of the riddles within the domain of coherence:

- (1) The gardener and his wife and the carpenter and his daughter walk together in the orchard. They discuss the price of fruit. Last April and May unwonted frost inflicted damage on the apple blossoms. Apples are very expensive now. Here and there they find a Golden Rennet. Five pieces of ripe fruit are already in their basket. Everybody eats an apple. Two are left.

The most important riddle is the following: can coherence be constructed for example (1)? I suppose that answers to this question can be found only when the human processor uses MUCH MORE than knowledge of the grammar rules. That is to say, that human reasoning processes should be actualized in different ways. I denote these human reasoning processes with the term 'inferential processes'.

From among these inferential processes I shall first consider the role of

1) I cannot deal here with the different properties of coherence. Doing so would require an extensive account of the interdisciplinary aspects of research (cf. Van de Velde, 1981a).

2) I cannot go into the empirical evidence which supports these two hypotheses. In a book on coherence (Van de Velde, 1981a) and in a monograph on inferences (Van de Velde, 1983) I pointed out that studying literary texts, reading scientific discourse, and interpreting (research on) aphasics' language offer clear indications which sustain my hypotheses.

syntactic inferences. In order to illustrate this role I will use the following sequence of upper-case letters:

(2) MARYSAIDTHETOPPLESSLADYISAMISS³⁾

The questions here are: what syntactic constituents are present in (2) and which relations can be identified between these syntactic constituents? I surmise that the human processor who searches for an answer to these questions will need syntactic inferences for the formation of pertinent hypotheses (cf. Van de Velde, 1982, 1983).

But the human processor will also be in need of other inferences to supplement the syntactic inferences. These supplementing inferences will to a great extent rely on knowledge sources related to lexical items. These knowledge sources are represented in the internal lexicon. The inferences which rely on them are termed 'ILRRR inferences'. (In previous publications I termed them 'lexical inferences'). The abbreviation ILRRR denotes that the internal lexicon is the cognitively reflected representation of reality. Research in neuropsychology, in neurolinguistics, in artificial intelligence and in cognitive science leads to the hypothesis that ILRRR has its own intrinsic organization. This organization of ILRRR can become operative in discourse processing under the following conditions: iteration of ILRRR information in the sequences of utterances; identity of ILRRR information; intersection of ILRRR information or inclusion of ILRRR information. For lack of space I will not be able to illustrate here how these ILRRR conditions are satisfied (cf. Van de Velde, 1981b, 1983).

The human processor will also need a third kind of inference which I refer to as 'semantic-logical inferences'. They allow us, among other things, to find the underlying propositions which belong to a specific discourse. In addition, semantic-logical inferences contribute to the detection of the semantic connections between the underlying propositions. In the local organization of propositions the semantic-logical inferences can become operative when the following conditions are satisfied: argument identity, predicate identity, argument overlap, and predicate overlap. In the global organization of propositions the conditions which should be fulfilled in order to make semantic-logical inferences work efficiently are: the consequence consistency condition, the pertinent prior knowledge condition, the topic compatibility condition, and the context integrativity condition. For further specifications and illustrations I must refer to Van de Velde, 1983.

The fourth main kind of inference is concerned with the fundamental role

3) This example is written in upper-case letters and without blanks because I want the reader to experience how inferential processes are already operative in graphemic decoding. However, inferences concerned with graphemic decoding will not be treated here (cf. Van de Velde, 1985). In the reception of written discourse, inferences concerned with graphemic decoding have to work together with the other kinds of inferences which I shall distinguish in the following.

which is to be assigned to 'action'. Multidisciplinary research on language and communication has made clear that we perform actions with discourse, that discourse denotes actions, that discourse creates action co(n)texts, that discourse acts upon communicative contexts, etc. So, inferential processes must unavoidably be directed towards actions. In that case, I term them 'action-oriented inferences'. In the following example I will only take into account the fact that discourse denotes actions:

(3) Arnold ploughs. Yesterday he manured. In a few days he will sow.

With respect to example (3) action-oriented inferences have to rely on schema-based knowledge (Rumelhart, 1980), 'frames' (Minsky, 1975), 'plans' (Wilensky, 1980), 'scripts' (Schank, Abelson, 1977) and on other information sources related to agricultural actions. The question 'what do we know as a result from reading (3) ?' is related to the question 'what inferences can be made from (3) ?'. The limits of space prevent me from providing more specifications on action-oriented inferences. Let me only make a general remark on the mutual relationship between the afore-mentioned kinds of inferences: action-oriented inferences have to cooperate closely with syntactic inferences, with ILRRR inferences and with semantic-logical inferences. Inferential processing of discourse is thus a multileveled complex of interacting and integrating inferences.

2. On supplementary hints

The main question which arises from the foregoing approach to inferential processing is the following: what about cases in which the language user is more than a reasoning human processor? In order to illustrate that this question is fundamental to the study of discourse processing I will take the following written version of an interview which an unexperienced student of psychiatry conducted with a twenty year old female schizophrenic patient⁴:

- (4) I.P. Where do you live ?
 C. (no answer)
 I.P. Yes, you must live somewhere.
 Where is your apartment?
 C. (no answer)
 I.P. What is the name of the street?
 Where do you live?
 C. In myself (C. answers in a tormented way)
 I.P. Where, where do you live? (I.P. is eager to know more about
 the patient and does not realize that he cannot come into
 communicative contact with her)

4) The example (4) and the comments on the different turns of the interlocutors have been borrowed from C. Scharfetter, 1977. The abbreviation I. P. denotes the interviewing student in psychiatry. C. is the initial of the patient's first name. The fact that the example (4) is a translation from German does not affect my argument in any way.

- C. I do not live.
 I.P. Yes, but what do you mean by that? (I.P. does not understand and urges the patient to be more explicit)
 C. (no answer)
 I.P. I want to know your address.
 Where do you live?
 C. Under my eyes (C's answer is barely audible)
 I.P. Where? (I.P. is unable to understand and is puzzled)
 C. Under my own eyelids

(Scharfetter, 1977, 183-184)

The important questions to be posed here are: what does the patient really mean? Are we in any way authorized to assign a definite meaning to her words? How are the diagnosis and the therapy to be designed when one is confronted with such problems of interpretation? For lack of space I can only point out here that the afore-mentioned inferential processes need a number of supplementary hints which have to do with the patient's affective attitudes, with emotional meaning, with value systems, etc. If searching strategies for such supplementary hints really do operate, then it may be inferred that the patient expresses extreme isolation and alienation (cf. Scharfetter, 1977).

3. Summary

In this paper I tried to make clear what the initial metaphor 'coherence is the sun in the cosmos of discourse' means. It is an illustration of my views on coherence being as irreplaceable in discourse as the sun is in the cosmos (Van de Velde, 1983).

In addition, I defended the view that the cognitive construction of coherence is dependent on the interactive functioning of different kinds of inferences. This position may be used to complement purely grammatical research.

In the final part, I argued that the different kinds of inferences, in turn, must cooperate with searching strategies concerned with additional hints. I am fully aware of the fact that many other supplementary hints have to be taken into account, especially with respect to so-called pathological language⁵⁾. It may, however, be the case that even within the area of so-called pathological language coherence will maintain its role that is so central to human understanding.

5) It should be emphasized that texts are used in social activity and human interaction. For that reason supplementary hints may also be taken from studies on conversational analysis, on patterns of speaking, on social roles, on therapeutic discourse, on psychiatric interviewing, etc. In addition, philosophical, psychological and linguistic issues pertaining to communicative maxims, cooperative principles, conversational postulates, and the like, may contribute to a better understanding of the complexities of face-to-face communication.

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Spontaneous Dramatization in Semi-Formal Conversation

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1. Topic

This paper is about a stylistic feature of spoken language which may be called 'spontaneous dramatization'. The term is meant to cover the dramatic mode of reporting something past, describing something present or preparing (rehearsing) something to come—in other words, our everyday theatre, our ~~aping~~ of others and our own gestures and ways of speech. Example: "I was running into Tom this morning. "Oh, dear!" he said. "I'm so glad Henry didn't turn up yesterday!" " I am sure we all know it from jovial coffee gossips and other informal gatherings. It is a style of reporting and characterizing whose alternative is the rather objective-like and analytic mode of expression usually manifesting itself as nominal clauses of indirect speech (*oratio obliqua*). Example: "I was running into Tom this morning, and he said (that) he was very glad Henry didn't turn up yesterday." To some people this alternative appear to be more proper than the dramatic mode, which by way of its linguistic form seems to carry with it so much of the reporter's or narrator's own subjective interpretation of and commentary on what is reported, described etc.

It is a banality to state that most spontaneously dramatized sequences are rendered as *oratio recta*. However, they also occur in the shape of *oratio tecta*, or even as mixtures of *oratio recta* and *oratio tecta*. This formal variation of spontaneously dramatized sequences should not surprise anyone either, though the stylistic terms used here derive from and are mostly applied in studies of literary language, and there are scholars who claim that *oratio tecta* is a characteristic of literary language in distinction to that of oral relation (Holm 1967: 152). Obviously, that is not so. Both stylistic terms mentioned above may prove relevant and useful even to studies of oral language.

Spontaneous dramatization may be a universal phenomenon. In child language we may observe it as "synpraxic speech" (cf. Luria & Yudovich 1959). However, during the first years at school it seems to be heavily reduced in the children's speech, though it hardly disappears totally. Is it relegated to the private and intimate sphere of language usage? Perhaps it is maintained and practised differently in different classes? Harold Rosen (1980: 158f.) has suggested that the use of the dramatic mode in everyday speech is so unevenly distributed in (the English) society that it is describable as a stylistic characteristic of working class dialects. I shall return to some of these questions.

2. The data corpus

The present study is a by-product of the large-scale Oslo urban speech project (TAUS = Norw. 'Talemålsundersøkelsen i Oslo'). The text corpus analyzed was tape-recorded in Oslo 1972-74; it amounts to ab. 185 350 consecutive words, some 81% of which are produced by 36 informants (native Oslo citizens 30-75 years old) in semi-formal conversations 20-70 minutes long. The informants constitute a balanced sample socially, sexually and age-wise. Thematically, the conversations are equally patterned for all informants, the topics being living conditions during the informants' childhood and youth. However, all informants were given ample opportunity to be as private and as personal as they liked. Thus, digressions were widely tolerated in the informants' text production. The semi-formal status of the conversations is due to the fact that the informants' conversational partners were strangers (viz. the field linguists). Despite the geniality and the aspired informal appearance and conduct of the investigators as well as their willingness to give the informants a loose rein in directing the conversation and thus deciding the modes of expression, the investigators' status as strangers and university professionals undoubtedly must have influenced the conversational situation somewhat in the direction of formality.

3. Methods of analysis

The texts have been analyzed on the basis of transcriptions of the original tape recordings. These transcriptions are made by trained linguists and controlled by one, sometimes (in cases of doubt) even two, colleague(s). The transcriptions are based on repeated and intensive tape listening. However, their phonetic sophistication is somewhat limited—for instance, no advanced instruments for acoustic or other phonetic analysis are used—as the transcriptions are made primarily for syntactic analysis. Nevertheless, pauses and pausal signals, breaks in intonation contours, rising, falling and level pitch patterns, tonemes, and emphatic stress are carefully noted in the transcriptions.

The linguistic analyses of spontaneously dramatized sequences and their boundary markers are carried out manually by the author. The sociolinguistic distributional analyses are made largely by electronic computing. As tests of significance multiple analyses of regression are applied (two-tailed tests).

4. Major results

A. Boundary/transition markers:

In the present Norwegian texts the transitions/boundaries between analytic and dramatized passages are marked in many ways. The are

I. syntactic markers, viz.

- (a) reporting clauses with the reporting verb in the preterite or in the (dramatic) present tense. These reporting clauses may precede and/or follow the dramatized sequences. Also, reporting clauses may be

inserted in and thus break up the dramatized sequences. Then, each of the reoccurring reporting clauses might seem to connect one or just a couple of dramatized sequences to the running reporting text. They seem, as it were, to be experienced (by the narrator) as necessary in order to maintain the textual coherence. However, longer dramatized sequences, for instance reporting more than one (third) person, may also occur without insertions of reporting clauses;

- (b) a preceding sentence which does *not* contain a reporting verb, but which is semantically closely enough connected with the following dramatized sequence to indicate its coming. In such cases a reporting clause is easily implied; when it is not expressed, it is probably due to the presence of other (especially suprasegmental) transition markers;

II. *morphological markers*, viz.

- (a) shifts of verbal tenses, usually from the preterite to the present tense;
- (b) the Norwegian conjunctions *at* (Engl. "that") and *om* (Engl. "if", "whether") and the pronoun *det* (Engl. "this", "that"). The conjunctions normally occur in transition to indirect speech reports, and according to normative Norwegian grammar, they may only be used in connection with nominal clauses of indirect speech. None the less they are used in ordinary conversational speech in passing on to *oratio recta* as well as to *oratio tecta*;

- (c) interjections and answering words (which, then, do not function as answers at all). Such interjectional syntagms may generally have a passage-delimiting function in Norwegian, e.g. as an alternative to pauses;

III. *pauses*, often combined with an interrupted preceding syntagm;

IV. *linguistic code-switching*, i.e. when the dramatized sequence is expressed in another grammar system (language or dialect) than the analytic, reporting context;

V. above all, *suprasegmental features* like

- (a) certain sentence intonations and pitch patterns, e.g.
 - i) discontinuous intonation contours (level pitch pattern, often combined with syntactic interruption) immediately before a dramatized sequence,
 - ii) falling or rising pitch patterns indicating termination of dramatized sequence; and
- (b) accentual switching, which includes more than variation of intonational contours, comprising, for instance, varying syllable stress, change in vocal quality and shifting allophonic realizations, but which, on the other hand, differs from code-switching in that it does not cover variation or change of grammatical features.

As has been pointed out so often in text grammatical studies, intonational conditions seem to be crucial as markers of boundaries between passages/paragraphs in spoken language. Very probably accentual switching is particularly

crucial when marking of the transition between analytic and dramatic modes is concerned. However, the present data also contain instances where such transitions take place completely without any noticeable suprasegmental marker.

Consequently, I have had to conclude that one or (more often) several of the mentioned transition markers are usually present, in particular the suprasegmental ones, but that *none of them appears to be indispensable*. This conclusion supports the statement recently made by the Norwegian text grammarian Bernt Fossetøl that obligatory or conventionalized paragraph markers are not to be found in Norwegian, as opposed to several other languages, and that "there is probably no single linguistic feature which defines what you could call a passage/paragraph" (Fossetøl 1980: 37; my translation). Also, it supports the assumption of Enkvist & Nordström (1978: 78) that the intonation patterns of sentences often, but not necessarily, contain clues revealing their positions in text units. In addition, it supports their proposal (loc. cit.) that a possible redundancy of intonational features as textual constituents would be an interesting subject for future investigation.

B. Sociolinguistic distribution of spontaneous dramatization:

Thinking of Harold Rosen's proposal that the dramatic mode, though probably being a universal feature of oral language text grammar, might be described as a working class dialect feature (Rosen loc. cit.), the present Norwegian data have been subject to sociolinguistic analyses. In short, the results of these analyses show that the occurrence of spontaneous dramatization among Oslo citizens differs significantly (on the 2% level) only according to *age*, in that dramatized sequences occur more often in the speech of older citizens than in that of younger ones. However, informants from working class areas (i.e. eastern Oslo) have significantly more *complex* dramatized sequences (i.e. sequences containing more than one remark and reporting more than one third person) than others ($\alpha = 1\%$). Otherwise, no significant differences have been traced, either according to formal education, profession, or to sex.

C. A thematic condition:

To conclude this paper, one rather obvious but not unimportant fact of thematic nature should be stated, viz. that spontaneous dramatization clearly seems to be related to the speaker's feeling of informality and personal security. Thus, for instance, in the present conversational texts working class informants from eastern Oslo typically dramatize easier and more often when the talk is *about school experiences* (which are basically common to all in Norway), whereas upper and middle class informants from western Oslo tend to dramatize spontaneously most often when talking *about life at home and holidays and recreation* (socially exclusive situations).

5. Conclusion

The study briefly and selectively reported here certainly doesn't prove much, but its results *indicate*

- 1) that all stylistic modes of reporting known from literary language are

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used in everyday conversations between ordinary people as well;

II) that several of a series of possible ways of marking transitions/boundaries between analytic and dramatic discourse—syntactic, morphological and suprasegmental—are usually applied simultaneously in Norwegian, thus creating a state of redundancy in this area of spoken language as in other areas, but that no single marker seems to be obligatory;

III) that a hypothesis of the dramatic mode of reporting etc. as being a working class feature is neither strengthened nor weakened; though admittedly spontaneous dramatization does seem to be a general conversational feature rather than a characteristic of the speech of a certain social group or class;

IV) that spontaneous dramatization is positively coaxed by an atmosphere of informality and personal security.

A detailed presentation and discussion of this study is available as a separate publication (Wiggen 1981) from the Dept. of Scandinavian languages and literature at the University of Oslo (Box 1013, Blindern, Oslo 3, Norway).

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Grounding in Basque Sentence and Discourse Structure

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The aim of this paper is to propose a solution for one problem in the linguistic interpretation of one grammatical phenomenon in one language. I hold that neither traditional grammar nor recent TGG developments offer adequate means for the solution of the problem. A survey of developments in current linguistic thought reveals a variety of TG grammars: Extended Standard Theory, Case Grammar, Relational Grammar, etc., etc. They all share some features, being a-priori models of sentence and text grammars, wherein any particular structure may be derived through algorithmic rule-application. All these are 'global' models that try to be internally consistent, exhaustive, and deductive. In contrast, my purpose is to find one local model for one phenomenon. It is my faith that the construction of any number of accurate 'local' models will eventually contribute to our understanding of the motivations for all realized sentences.

My theoretical stance is that an empirically determined form, morphological, phrasal, or sentential, can only be dealt with in relationship to the whole text where it appears. Hence, I set about making a contribution to text/discourse grammar. I limit my focus to the *simple narrative*, taking, for convenience the naive position that everybody *knows* what a simple narrative is, and hoping to show, indirectly, that the definition of textual genres is as much a linguistic as it is a literary problem. Another convenience of choosing a simple narrative text is that it defines its own pragmatic situation, relying on its own *mise-en-scène* more than any other textual genre: dramas, conversations, orations, etc. Each type presents natural language texts, offering data for the linguist's analysis. What a text contains, besides a number of sentences, does not deserve to be tossed into the deep pit of 'mere performance'.

I examine a specific problem in Basque verb morphology, attempting to determine the textual motivations for the occurrence of certain so-called 'aspectual' forms of the verb in Basque narratives. Traditional accounts of these forms have tried to define the meaning not of the form as it occurs, but the meaning of the label, pinned by a grammarian on form. In my first two examples, we find two different nominalized forms of the verb: *galtzen* and *galdu*. 1.) *parioa galtzen zuena* 'the one who lost the bet'; 2.) *parioa galdu zuen* 'the one who lost the bet'. A pure sentence grammar might lead us to think that the difference between the two forms is optional or to invent some claptrap about 'punctual' vs. 'progressive'. If we apply a simple commutation test by switching the phrases into grammar-book 'present tense', we see that the rela-

relationship between the two forms shifts considerably: 3.) *parioa galtzen duena(k)* 'the one who is losing the bet'; 4.) *parioa galdu duena* 'the one who has lost the bet.' A look-up grammar with convenient, deceptive labels hides the fact that here we have not mere tense difference, but text difference. This difference corresponds to what Harald Weinrich designates as the difference between the *besprochene Welt* 'commented world' and *erzählte Welt* 'narrated world' in an influential work, *Tempus* (1964, 1971, 1977). In Weinrich's terms, those morphological verb forms usually labeled 'present tense' predominate in the Basque commented world, the world of present, immediate experience, while the narrated world is one step removed, one way or another, from the act of narration and is sufficient unto itself. The narrative tense in Basque is predominately the 'past tense', which is Basque grammar is markedly different in morphology from the present tense. Things are turned up-side down. Our assumption of this attitude avoids discussion of real time-relationships, which will, until the nature of genres is clarified linguistically, forever go astray in hopeless metaphysical speculations and founder on the shoals of native intuition. A sentence grammar presents us only with an array of contradictions in meaning and use when individual grammatical sentences are drawn at will from an unclassified variety of textual types. A satisfactory genre-classification will arise not from exterior literary criteria, but from internal, linguistic features.

I defend the hypothesis that forms with past, i.e. narrative, tense, marking, containing the forms of the class of *galtzen* are used to indicate *backgrounded* information. Such information indicates circumstances against which the main action of a narration takes place. Sentences containing verbs with the *galdu*-forms are 'progredient' in that they indicate the progress of the narrative. They, by their nature, require exact sequencing. The backgrounded information in the *galtzen*-forms is revealed at the narrator's convenience. Often the latter forms are placed in subordinate clauses where their function as attending circumstances is quite obvious.

I make here only the distinction *narration* vs. *everyday speech*, a distinction necessary for the progress of this argument. I chose a short text to show the function of the two classes of nominal verbs in the course of a straight-forward narrative. It is a modern text, a self-sufficient section of a story, Joseba Sarrionaindia's *Maggie, Indazu Kamamila*, published in 1980. It gives us a 'slice of Basque life' in today's Manhattan.

While the morphology of the Basque language is complicated, its verbal morphology is prodigiously so. The greater number of inflected verbs in Basque are made up of a nominal form of the verb plus an auxiliary inflected for tense, mode, subject, direct object, indirect object, and person addressed. The phrase *galdu du* (also a complete sentence) consists of *galdu*, a participle that here operates just like the English past participle, and *du*, an auxiliary inflected for present tense plus reflexes of the subject and direct object. Loosely, we may translate this form as 'lost it-has-he'. Often verbs of this class are called *an-*

alytic. A much smaller class of verbs attach indicators of tense, mood and the pronominal reflexes directly to the verbal root, e.g. *zegoen* 'she remained', derived from the root, *go*, found in the dictionary as the participle *egon*. This sort of verb is referred to as the *synthetic* or *primitive* verb. In the oldest Basque texts (16th century), we find sixty such verbs, of which only thirty remain today. Most of these are defective (Tovar 1968:37). In analytic verbs, as is evident from sentences 1-4, two differing nominal forms may occur. In sentences 1 and 3, we find the nominal infinitive in *-te*, *-ite*, *-tze* plus an archaic inessive ending, *-n*. For illustrative purposes, we might translate the phrase *galtzen zuen* 'losing-in he-it-had' (Lafitte §456). Hugo Schuchardt (1923 (1968):7) laconically sums up the practice of most grammarians in his interpretation of the Biblical form *ethorten*: "*Ethorten da* corresponds, to be sure, in formation to English *he is coming* (*Ethorten* in coming), but it has been generalized and, as a matter of fact, has the same value as *dathor* he comes." There is good morphological reason for equating the forms *ethorten da* and *dathor*, as well as *ethorten zen* and *zeturten*, for real synthetic presents and pasts are rare in the Basque grammatical storehouse. A Basque teaching grammar will for the sake of paradigmatic presentation put such forms as *ikusten du*, *aditzen du* in the columns labeled 'present tense'. When we become aware of how the Basque system really works, we realize that this is an act of grammatical desperation. The fact that inessive nominal infinitives have not been fully drawn into the verbal paradigm is betrayed by the fact that this form appears dependent upon other verb complexes and adjectives: 5.) *Jausten ikusi dut* 'I saw him coming down'; *badaki mintzatzzen* 'He knows how to talk'; *Errox da erortzen* 'It is easy to fall'. Verb phrases with the participle, *galdu zuen*, seem to resemble English 'present perfect' and French 'passé composé', but do not be deceived. The assumption will ease the progress of this discussion. A systematic confrontation of sentences containing *galtzen*-forms with those containing *galdu*-forms reveals an important difference: 6.) (I, 1-2) *Goizero... iratzartzen ninduen amonak...* 'Every morning, grandmother awakened me...'; 7.) *Ile urdinak orraztu gabe etortzen zitzaidala ohera* 'Before coming her grey hair, she came to my bed...'; 8.) *Udazkeneko goiz batetan amona ez zen esnatu, ez ninduen iratzartu* 'One fall morning, grandmother did not wake up; she did not awaken me.' 9.) *Musu batez lehortu nituen amaren lehen negar anpulak*. 'With a kiss, I dried away the first great tears'. Sentences 6 and 7 are unspecified as to time: *goizero* 'every morning'; *Ile urdinak orraztu gabe* 'before combing her grey hair'. In sentences 8 and 9, however, the time is exactly specified. Observe that the two short sentences in 8 may not be reversed in order. Comparison with other sentences confirms this observation. 10.) ... *eta ohera sartzen saiatzen nintzan* 'and I tried to put her to bed'. (To translate this into English as 'and I was trying to put her to bed' would be totally wrong. It means something else.) The context tells us that this has happened many times. If we substitute *saiatu*, the participle, for *saiatzen*, we find that an exact time specification is needed. 11.) *Martxoaren 8-an ohi baino gaizkiago zegoelarik,*

ohera sartzen saiatu nintzen. 'On the eighth of March, because she was worse than usual, I tried to put her to bed'. Evidence from another part of the short story confirms the observation that past-tense *galtzen*-forms indicate unspecified times: 12.) *Lau miloi ahate hiltzen omen ziren hemen egunero eta miloi bat axuri, eta milaka zakar deslai New York hitsean*. 'Four million ducks, they say, died here every day and one million lambs and stray dogs by the thousands, in sad New York'.

The requirement for the strict sequencing of the *galdu*-forms in simple narratives is explained by the requirement for strict time indication in sentences that contain them. The *galdu*-forms represent show us the iconic aspect of text-formation, for they reflect the 'real' or 'natural' order of events. They may not be displaced, for this would destroy the veresimilitude of the text. Both narrator and reader/listener are well aware of these rules-of-the-road, for, if they are, violated, the narrative becomes confused and hard to follow. Deliberate displacement of these elements requires extreme sophistication. Only the very skilled narrator can, without offending his audience, use, for instance, the epic technique of *in medias res*. Since narrator and audience are sensitive to rules that generate well-formed texts, we may hazard the guess that such awareness is part of the linguistic competence of a speaker of a natural language. With this idea in mind, I agree with those who find in *grounding* a good candidate for one of the universals of language.

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(6) Goizero, gosaria berotu eta iratzarren ninduen amonak, lantegirat abiatzeko orduan. Bai, arras maite nuen, oroitzen dut atorluzcaz eta (7) eta ile urdinduak orraztu gabe *etortzen zirendala* ohera, egunsentiro, kamamila kehetsu bat eskuan. (8) Udazkeneko goiz batetan amona *ez zen esnatu, ez ninduen iratzartu*, loak hartua zegoen, baina betirako loak. hila zen. Gauetz hil zatekeen, iskanbilarik gabe, bizitzari atxikia irauteko ahaleginik gabe, deihadarrik gabe. Loaren antzera zetzan gorpu, hotzik. (9) Musu batez lehortu nituen amaren lehen negar anpuluak, gero, nigarretan ¹lagundu nuen, malko epeletan amamattoren arima gordetzeko. Biharamun arratsaldean thortzi genuen, panpina zahar bat bailitzan, eta lili more bi inpini genituen haren hilobiaren gainean, han, altzifrez garaien artean, horma zuriz hesinguraturiko heriotzlarreberdean. Amattok ez zuen sekula ahantziren. Hotzaren eta tristuraren ikaraz iragan zuen nega osoa. Horditurik kausitzen nuen egunero, lantegetik itzultzerakoan, eskaratzean edo ankaz gora logelan, auheneka. Lantandu eta (10) ohera sartzen saiatzen nintzen. "Every morning, after heating up breakfast, grandmother awakened me at the time for going to the workshop. Yes, I loved her completely. I remember the long shirt and that, before combing her grey hair, she came to my bed at dawn with a cup of steaming camomile in her hand. One fall morning,

grandmother did not wake up. She did not awaken me. She stayed asleep, but in eternal sleep. She was dead. She probably died during the night, without any noise, without being able to hold to life, without fuss. The body lay cold just as if she were asleep. With a kiss I dried mother's first great tears. Then I, in tears, helped her put away the soul of grandma in her tears. On the next day in the afternoon, we buried her as if she were an old doll and we placed two purple lilies on her grave, there, among the tall cypress trees in the green cemetery surrounded by a white wall. Mama was not about to forget ever. She spent the whole winter trembling with cold and sadness. I found her drunk every day, on the way home from work, in the kitchen or arms akimbo in the bedroom, lamenting. With outstretched arms I tried to put her to bed."

Metaphor

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A central question about metaphor is whether it is a phenomenon of *langue* or only of *parole*. Does metaphor, of some variety at least, belong among the resources of language we draw upon; or only to utterances, to the uses we make of these resources?

This question, however, is usually posed badly; in a way that rests on a natural, but false, assumption. When so posed, those who deny that metaphor belongs to *langue*—the 'parolistes'—look to be the winners; but when the question is corrected, those who insist that it does belong to *langue*—the 'languistes'—emerge with some justification.

The question, as usually posed, is: are there metaphorical sentence- and word-meanings, or not? The 'languiste' says there are: that many sentences and words possess, in addition to their literal meaning(s), metaphorical meaning(s). The 'paroliste' denies this: the only sentence-meanings are literal ones. Metaphorical meaning belongs only to utterances, and occurs when the speaker means something other than what the sentence he utters means. The thought expressed by the sentence itself is always what it literally means; but when I speak metaphorically, I 'nudge' or 'provoke' my audience into having a thought different from this one.

The main 'paroliste' objection is that metaphorical sentence-meanings are explanatorily idle: and since the point to assigning meanings to sentences is to explain use and interpretation, there can therefore be no warrant for postulating this kind of meaning. In standard cases, I grasp what you mean by uttering *s* because I know what *s* means: this is so whether what you mean by it is what it means or, as in irony, the opposite. When you speak metaphorically, I also have to know what your sentence means, but only what it *literally* means.

My interpretation goes *via* (i) grasp of literal sentence-meaning, (ii) recognition that this is not what you mean, and (iii) contextual and other non-semantic information. There is no room in this process for something that could be called understanding of the sentence's metaphorical meaning. If we do speak of this supposed entity, it could only be a misleading way of referring to what I take you to have meant by uttering the sentence. Certainly my taking you to have meant such-and-such did not require my *first* identifying a so-called metaphorical meaning of the sentence you uttered. The 'languiste's reply is to concede this objection in the case of novel 'creative' metaphors—like 'Music is the food of love' (when first uttered). Here, to be sure, interpretation can only go *via* grasp of literal meaning plus non-semantic information. But in the

case of established, 'dead' metaphors—like 'I've got into Herman Hesse'—interpretation surely goes *via* direct identification of metaphorical meaning ('I've become very interested in Hesse's writings', or something similar). The hearer, in this case, does not have to go the roundabout route *via* literal meaning, context, etc..

But this reply is unconvincing: for the 'paroliste' will retort that 'dead' metaphors are not metaphors—any more than dead husbands are still husbands. In cases like 'get into' we have, simply, a case of homonymy: the expression, these days, has two literal meanings, and it is grasp of one of these which accounts for interpretation. The new literal meaning is, no doubt, somehow derived from the earlier spatial meaning; but this is irrelevant to the synchronic fact that, in 1982, we have no more than homonymy.

The poor 'languiste', it seems, is always *too late* to produce a sentence with metaphorical meaning. While he is still speaking metaphorically, what he means is never what the sentence he uses means; but when, through repetition and convention, he manages to mean by it what *it* means, he is no longer talking metaphorically, but only with a newly acquired literal meaning.

So it seems: but the debate has been a bad one, which mistakenly equates the *langue/parole* issue with the question of whether there are metaphorical sentence-meanings or not. Both parties assume the following: there is only metaphor—or, better, metaphorizing—where there are metaphors; that is, individual sentences (for the 'languiste') or individual utterances (for the 'paroliste') with metaphorical meanings. But this is wrong. There is more to metaphor than metaphors; producing the latter is only one way of speaking metaphorically. If so, the fact that language contains no sentences with metaphorical meanings will not entail, as the 'paroliste' assumes, that language does not contain metaphor. This sounds paradoxical: let me explain.

Dancing sometimes involves performing a dance; but not always. Isadora Duncan could dance all afternoon without performing one or more dances. Symbolic activities sometimes utilize symbols; but not always. A ritual might be symbolic, but contain no discrete elements of which one could say 'That is a symbol for...'. We need to distinguish activities—dancing, symbolizing—from items (dances, symbols) which may or may not be present in the activities. The same, I want to say, goes for metaphor: we need to distinguish the activity of metaphorizing from the items sometimes produced in this activity, metaphors. We need to, for explanatory purposes.

According to the 'paroliste', the only linguistic knowledge I require to interpret a metaphorical utterance is grasp of literal meaning. This is surely wrong. When I first heard 'get into' used in a non-spatial way, I immediately and correctly grasped what the speaker meant. Crucial, here, was my acquaintance with a systematic practice of talking about interests, enthusiasms, etc. in terms of spatial immersion. People get absorbed in, caught up in, buried in books or music or hobbies. I did not have to go *via* the spatial meaning plus context, as I would if the speaker had said he'd got underneath Hermann Hesse.

But why should this worry the 'paroliste'? Can he not accommodate matters thus: many expressions, originally signifying spatial immersion, have acquired a further literal meaning in the area of interests. It was my knowledge of this background homonymy that enabled me to assign the meaning intended by the speaker to 'get into' when I first heard it being used in the 'interest' sense.

This won't do: a background of homonymy is incapable, in itself, of explaining how new uses, and interpretations of uses new to the hearer, are generated. For example: a pervasive kind of ambiguity is the use of many nouns both as stuff-nouns and as count-nouns referring to things made out of the corresponding stuffs. Glass/a glass of . . . , paper/my daily paper, tin/a tin of . . . , etc. But this phenomenon is not generative: you would be hard-pressed to understand me if I said 'Would you like half a mud?', or 'I have a flesh on my leg'.

So the ability to understand, or produce, for the first time, 'get into' used in the 'interests' sense, must be due to more than the background ambiguity of spatial immersion expressions. We are forced, surely, to say something like this: when speaking of being absorbed in a book, or of getting into an author, and the like, we are talking about one kind of thing *in terms of* something else, and not simply employing expressions which have another meaning in this other field. Nothing analogous can be said about the stuff- and count-nouns: we do not speak about glasses, tins, and so on, *in terms of stuffs*; we are not suggesting parallels, analogies, or trying to cast light upon. But we are doing all of this when using a battery of spatial immersion terms to talk about interests: we are metaphorizing—even though, as the 'paroliste' correctly says, the 'interests' use of these expressions is too well-established for us to call a sentence like 'I'm absorbed in a book' a metaphorical sentence or utterance.

The idea that, in metaphor, we talk about one kind of thing in terms originally appropriate to another, is hardly novel. But my aim has not been to establish that banality, but to see how it can be accommodated in a coherent philosophy of language. For we have encountered two views which fail to do this. The 'linguiste's claim was that we talk about X in terms of Y when we use expressions in the province of Y with special metaphorical meanings distinct from their literal ones. But there are no metaphorical expression-meanings, so this cannot be the right account. The 'paroliste' wanted to say that, except in the case of truly novel, 'creative' metaphorical utterances, it is in fact an illusion to suppose we speak of one kind of thing in terms of another. What happens, rather, is that expressions originally in the province of Y take on new, perfectly literal meanings in the province of X. Reference to being absorbed in a hobby is not to talk of interests in spatial terms; for 'absorbed in' has, in this use, lost all connection with spatial phenomena. But this left unexplained the generative capacity of the practice of employing spatial immersion expressions in connection with interests; wrongly implying that the only linguistic knowledge required for interpretation of utterances novel to the hearer was that of literal meanings. Put differently, my aim has been to encour-

age a shift in logical grammar: to shift interest away from metaphors—sentences or utterances—towards an activity, or set of activities; metaphorizing. Appeal to the activity is required in order to explain how metaphors are interpreted. It is a shift analogous to the useful one away from certain objects, symbols, to the activity of symbolizing.

The Problem of Direct and Indirect Speech Acts: A Referential Approach

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1. The Referential Structure of Impositive Speech Acts

The thesis underlying the present paper is that the distinction between direct and indirect speech acts should be analyzed in terms of the correlation that holds between the linguistic structure of the utterance, the intention of the speaker and the interpretation by the hearer. Within this frame of reference I shall investigate the referential component of two classes of speech acts: impositives and assertives.

In regard to impositive speech acts I have proposed elsewhere to define them as speech acts which are performed by the speaker to influence the intentional behavior of the hearer in order to get the latter to perform, primarily for the benefit of the speaker the action directly specified or indirectly suggested by the proposition. (Haverkate 1979:33)

Now, I wish to argue that the disjunction of 'directly specified or indirectly suggested by the proposition' should be taken as the basic criterion for distinguishing between the direct and indirect performance of impositive speech acts. Direct performances, then, are characterized by both the full specification of the act of the hearer and reference to the hearer himself. Indirect performances lack such a specification as well as an explicit reference to the hearer. Therefore from this formal point of view, the classical example

(1) *Can you pass the salt?*

is not an indirect, but a direct speech act by virtue of its propositional structure. It is important to note in this connection that, as far as the reaction of the hearer is concerned, it is the complete description of the act together with the explicit second-person reference which enables him to correctly interpret the impositive character of the speech act without any particular background information.

The next point to be made is that there are different degrees of indirectness as determined by the following conditions on propositional output:

- (1) specification of the act to be done without specific reference to the hearer
- (2) no specification of the act nor specific reference to the hearer.

A relevant example of the latter category would be

(2) *I am terribly cold*

uttered by a speaker who wants to get the hearer to close the window. Examples characteristic of the former category are

(3) *Somebody has to close the window*

and

(4) *The light in the kitchen has not been turned off*

under the interpretation that it is the person addressed or one of the persons addressed who is supposed to close the window or to turn off the light, respectively.

Focusing on referential structure, we find that (3) is marked for non-specific reference, and (4) for implicit reference to the hearer, as determined by the agentless passive structure of the sentence. (2), on the other hand, is not marked either for non-specific or implicit reference.

On account of these facts we can assign to (2) a higher degree of indirectness than to (3) and (4), because for the hearer to correctly interpret (2) he needs to have specific background information in order to figure out whether the speaker wishes the door to be shut, the heating to be put on, the window to be closed, etc. For the non-cooperative hearer, therefore, it is relatively easy to neglect the impositive purpose of the speaker and react to the assertive component of the speech act only. (3) and (4) represent a less indirect type of speech act than (2) because of the overt description of the act to be performed by the hearer. However remember that (3) and (4), at their turn, differ from the direct impositive (1) in that they do not contain an explicit reference to the hearer.

To conclude this section I would like to concentrate upon one more example of indirect reference. Therefore let us look at

(5) *Now we are going to wash our hands*

Here the use of 'we' typically reflects a communication situation in which the social relation between the speaker and the hearer is characterized by the superior position of the former with respect to the latter. As a result, impositives such as (5) are illustrative of the interaction between parents and children, and between nurses and patients. As regards referential scope 'we' expresses a special kind of inclusive reference. The set of referents involved normally consists of two members; one of them is supposed to perform a particular action, while the other supervises that action.

2. The Referential Structure of Assertive Speech Acts

Unlike impositive speech acts assertive ones are not subject to propositional constraints. Consequently, predicate selection does not serve as a parameter for determining indirectness. However the parameter of indirect reference

does apply. Since the corresponding strategies mainly involve the participants in the speech act, in what follows I shall concentrate upon expressions referring to the speaker and the hearer.

Starting with reference to the speaker we have to make a distinction between non-specific and implicit reference. A characteristic type of non-specific reference is the use of 'we' in persuasive contexts, as illustrated, e.g., by

- (6) *Here we are faced again with the fatal consequences of the present wage policy*

In Haverkate (1980:162) I pointed out that the referential scope of 'we' in assertions such as (6) is inclusive, since it is the intention of the speaker to refer to himself, his audience, and possibly an unspecified set of other persons as well. This use of the personal pronoun first-person plural is typical of political speeches and scientific treatises.

By using 'we' instead of 'I' the speaker aims at avoiding a direct confrontation with the hearer suggesting at the same time that the latter is supposed to share the responsibility for the point of view put forward by the speaker.

I now turn to the analysis of the indefinite pronoun 'one', which by virtue of its inherently non-specific meaning, may be used by speakers to indicate in an indirect way their involvement in the state of affairs expressed. This may be seen from an example such as

- (7) *So, one comes to the conclusion that you haven't done your work well.*

Under the interpretation that the conclusion referred to in (7) is in the first place a personal conclusion of the speaker, we can say that the use of 'one' serves the purpose of minimizing his role in the action described. Finally, we have the category of implicit reference to the speaker as manifested by agentless passive sentences. Thus

- (8) *At the meeting it has been observed that you have overlooked an important problem*

may be uttered by a speaker who wishes to avoid overtly referring to his responsibility for having brought about or contributed bringing about the state of affairs denoted by 'it has been observed'.

Let us look next at indirect reference to the hearer. Here we have to deal mainly with the implicit reference inherent in passive constructions without a specified agent. The communication situations involved are typically those in which speakers convey information they assume to be unpleasant for their hearers.

Thus, a speaker wishing to display a non-contrastive form of behavior will prefer the indirect assertion

- (9) *This work has not been carried out correctly*
to its direct counterpart

(10) *You have not carried out this work correctly*

since the latter, unlike the former, explicitly refers to the hearer as the one who is responsible for having brought about an undesired state of affairs. It is to be noted, in conclusion, that, as far as the interpretation by the hearer is concerned, the context or situation normally contains sufficient clues as to enable him to infer that he is implicitly addressed by the speaker.

3. Conclusion

Let me finish with some concluding remarks. I have argued that the analysis of direct and indirect speech acts calls for a distinction between formal and functional properties. The former have to be defined in terms of propositional structure, the latter in terms of the correlation between the intention of the speaker and the interpretation of the hearer. From the functional point of view indirect speech acts are frequently explained as particular expressions of politeness. Although this may be true of such standard formulas as 'Can you pass me the salt?' and 'Will you open the door for me?', we also have indirect speech acts which do not express any polite intention on the part of the speaker at all. This may be illustrated by

(11) *The door is not closed!*

which, if uttered with the appropriate intonation contour, can be interpreted as an authoritarian order to close the door. From this it follows that politeness does not serve as the basic criterion for determining the indirectness of speech acts. The criterion to be applied instead is the distinction between accommodative and non-accommodative behavior. Utterances such as (11) are characteristic members of the latter category, because the speaker issuing the order has the intention of creating a social distance between his interlocutor and himself. However most of the examples discussed throughout this paper reflect forms of accommodative behavior. Speaking in general terms we might say that accommodation is the regular form of verbal behavior by virtue of certain principles of co-operation which govern human interaction.

In summary, with respect to impositive speech acts, indirect reference to the hearer may serve to avoid making explicit the power the speaker has over the hearer, which is equivalent to stating that the former adopts an accommodative attitude towards the latter. This is clearly illustrated by our example (5) *Now we are going to wash our hands.*

In relation to assertive speech acts, lastly, we arrive at the conclusion that non-specific or implicit reference to the speaker or the hearer is intended to manifest two different forms of accommodative behavior. In our discussion of example (6) we have noted that by using a first-person plural expression the speaker develops a strategy which aims at creating the impression that the hearer shares the responsibility for the assertion.

Example (9) *This work has not been carried out correctly*, on the other hand,

reflects the fact that implicit reference to the hearer is not an uncommon device handled by speakers who wish to convey bad news or unpleasant information in an indirect way.

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Why There MUST be a Semantic Representation

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Language is a mapping between sound & meaning. The sound is usefully divided into 2 parts, the sound itself, & its distinctive aspects. So far, however, we do not have agreement on what meaning is or how to divide it up. The Saussure-Hjelmslev logical division into distinctive & non-distinctive parts still seems a good working assumption, as it simplifies many problems in semantics. But rather than no assume this without justification, I would like to argue for something like it: that there must be a semantic representation over & above the processes of interpreting texts into a world.

In the context of natural language understanding algorithms based on cognitive networks, or nearly the same thing, belief-structures & processes of inferencing, or in the context of Montague grammar & systems inspired by the interpretations of formal languages, the concept of a semantic representation as containing the content of a text, but not its myriad implications, seems to be nearly drowned into insignificance, or else reduced thereto by a wealth of pragmatic detail.

Here I want to summarize some arguments that a structured representation of "the meaning of a text" exists & is significant for the interpretation of linguistic acts, as well as for a concise account of processes that are reflected in syntactic facts.

A number of pseudo-arguments are possible, based on the supposed need to distinguish the "meanings" or "readings" of a "truly ambiguous" sentence from the "uses" or "interpretations" of an only apparently ambiguous sentence. Arguments such as these, & all these terms, assume some significance for the distinction between the case where a sentence can be true or false in some situation, depending on how it is interpreted (called "ambiguity"), & where there can be no such situation. While that distinction has a long & respectable history, it is not hard to imagine a semantic interpretation system in which this distinction is a low-level fact of no consequence. This is the case for phonetic facts such as the distinction between bilabial & labiodental, which is almost totally devoid of distinctive value in any language; it could well be the same with traditional notions of ambiguity & vagueness of meaning.

If we are going to argue for the existence (or assume it, for that matter) of a semantic representation, it is worth our while to rest its definition on some clearly significant properties, which is my aim here. Thus these arguments will not & cannot be predicated on traditional distinctions, unless their relevance can also be demonstrated. In this sense, the present arguments are

basically non-theoretical, & are all the more robust for their independence of other theoretical systems or notions.

I have argued elsewhere (1981) that it is not simply a representation of the meanings of isolated sentences that is necessary, but more than that, a representation of the integrated meaning of all the sentences preceeding a sentence, that is needed for the accurate interpretation of that sentence. This is easily demonstrated by such a simple device as reversing the order of sentences in a text & thereby destroying or radically modifying its meaning, & it condemns to sterility all those attempts to interpret single sentences into quantified formulas of the predicate calculus or other formal language.

It is in this sense of a semantic representation (ie. the integrated meaning of a series of sentences) that I argue here to be necessary, over & above the knowledge & inferences that a person uses sometimes in interpreting a sentence. I will present several demonstrations of increasing formality that there must be such a semantic representation.

The first & simplest demonstration is that the human being does not—when reading a novel, listening to a political speech or studying a scientific theory—integrate what he hears into his knowledge of the world. He can & usually does hold it apart, to be evaluated in the weeks or years that follow, as being true or not. This shows at very least that the human linguistic-cognitive abilities include keeping a complex of ideas without necessarily believing them.

A 2^o demonstration is to observe that the normal person “knows” of many different worlds, & that these worlds differ by what is a fact & what is not a fact. As scientific examples, Riemannian & Euclidian geometries are worlds differing by whether parallel lines meet or not, & any pair of competing theories in physics or other science are contrary worlds. In the realm of ordinary life, most English speakers know of at least the following 4 worlds, which contain different objects, different possibilities, different events, &c: the world of Greek mythology, the world of Sherlock Holmes, the world of James Bond, & the “real” world.

If a person must incorporate knowledge of 4 or so distinct worlds, & it is probably closer to 40 than to 4, then we may say he has as many cognitive networks, of various degrees of detail & completeness. As this is so, then no additional theoretical apparatus is needed to add one more cognitive network to account for his keeping of the meaning of the conversation currently in progress. This is no more or less than a semantic representation of the discourse, though we may observe that it differs in content & function.

A tighter, but longer & more difficult demonstration, the 3rd, is to show that “inferencing”, ie. drawing the inferences out of the use of some expression—roughly equivalent to moving along arcs in a cognitive network—is NOT undertaken until the semantic operation of INTEGRATION is attempted. This operation, which combines the semantic contributions of sentences together, depends heavily on a principle whereby each successive sentence is interpreted in the most redundant way possible with its prior context, i.e. so

that it "overlaps" maximally with the context provided by the previous sentences. This principle, PIM, after its French name *principe d'interprétation minimale*, has been explored in Hofmann (1978), (1981).

With this principle, we can show that inferencing appears to be undertaken only when integration is blocked for lack of overlap of the expressed meanings of the component sentences. This "expressed meaning of the discourse" is nothing more or less than its semantic representation. If we can show that this principle of maximum overlap with prior context is applied before any inferencing is attempted, then we will have shown that a semantic representation of that prior context exists, & that it is available when inferencing takes place, which appears to be when sentence-meanings are integrated together.

There are apparently several radically different types of inferencing. One is the recall of appropriate frames, scenes & scenarios when needed to interpret a sentence, as e.g. "that picnic was great, but *the* beer wasn't cold" without a prior mention of beer. We shall leave this till later. Another is the use of collocational ("selectional") information & knowledge of what subclasses make up what classes, to disambiguate a sentence.

It is well known that collocational information can disambiguate a sentence, as in

- (1 a) I saw a dog playing with a cat...
- b) ... & *it*₁ was purring!
- c) ... & *it*₂ wasn't barking!

Here, the verb *to purr* in (1 b) shows that *it*₁ refers to the cat, while *to bark* in (1 c) shows that *it*₂ refers to the dog, even in spite of being negative, *not barking*. Thus collocation is used to find the referent of a pronoun.

Yet this principle PIM overrides this use of collocational information. By noting that one would indeed know he had seen it, & would describe it as such, if one ever saw a barking horse, J. Lyons showed that collocational information is knowledge about the (normal) state of the world. In an abnormal situation as follows, the collocational information which should make *it* co-referential with *a dog* seems forgotten without a trace in (2 b).

- (2 a) I saw a strange horse; it had grown up with a dog. Naturally, *it* used to bark at all the cats...
- b) ... with the dog.

The fact that with the continuation (2 b), *it* refers unambiguously to the horse, but without that continuation, *it* refers just as unambiguously to the dog, can best be explained as an overlapping of the meaning of the 2° sentence with the 1°. Only then, if there are ambiguities remaining, collocational information is used. Note also, the fact that there is no sentence boundary between the turn-coat *it* & the phrase that overlaps with *a dog* is critical, implying that at a sentence boundary, during integration, all ambiguities that can be removed are removed by knowledge of the world, & apparently, not before then.

Other examples are possible to demonstrate this point, but let us turn to the more serious use of inferencing, where a whole scenario may be drawn up from memory to provide a referent, as we saw for the expression *the beer* above, which needs no antecedent if a picnic was priorly mentioned. The scenario of *picnic*, it may be said, includes hot dogs, beer & baseball, as 3 things that may be expected on a picnic, & may be referred to without further ado. Yet if some beer was mentioned prior to the picnic's mention, the scenario of *picnic* never appears, as in the sequence:

- (3 a) Where's *that beer* you bought this morning?
- b) The *picnic* was fine but the hike back made me hotter than all get out;
- c) this wine's good, but *the beer* was better.

Here again, the scenario appears never to have been called up by the *picnic* in (3 b), thus allowing or forcing *the beer* in (3 c) to refer to the beer mentioned in (3 a). If we have an adequate antecedent, the scenario is not used.

In sum, & without a lot of other examples that space prevents, when there are adequate referents, which are most generally found by overlapping in the least informative way, cognitive networks or the inferences of items like *to bark* or *picnic* are not used. It thus follows that some representation of these referents & the expressed semantic structure needed to find them (see Hofmann (1975) must be available when sentences are interpreted.

These 3 demonstrations show with increasing formality that there does exist some cumulative representation of the expressed meaning, not including the implications which can be drawn from that meaning. The last shows that this INTEGRATED semantic representation plays an important part in deciding what meaning is assigned to sentences that would be ambiguous or ill-formed if shorn of context. This is an important if not the most important role of a semantic theory.

We may note additionally the global relevance of this: because the comprehension of sentences in context not as ambiguous, but in their intended meanings, is an essential part of language behaviour, much more than is the ability to detect ambiguities or synonymies (or even hyponymies), this use of (& definition of) an integrated semantic representation is as essential part of an adequate theory of language—more so, at least, than the traditional philosopher's properties of ambiguity & synonymy.

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Shifting In and Shifting Out: Remarks on Deictics

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A pragmatic theory of language considers all language fragments to be modified by the I-sayer (or its substitutions). The I-sayer holds the First Person Authority consisting of shifting in and shifting out of the discourse. It is my purpose to relate the analysis of the First Person Authority (see D. Davidson, "The First Person Authority", unpublished paper, 1978) to a modified version of Jakobson's theory of deixis and especially of language shifters. However, it should be evident that the right of the I-sayer is interactionally constrained and, in fact, delegated by the interpreter to the speaker. The underlying notion of meaningfulness indeed rests upon a (pragmatically oriented) theory of meaning as a theory of understanding.

1. First Person Authority

Pragmatics reevaluates *subjectivity* in language. To the presence or absence of this subjective aspect of language we can assign the term "attitude", "style", "expressivity", and even "performance". Special linguistic forms are reporting the presence of the speaking subject—they can be morpho-lexical, sentential or textual. The "attitudinal", "stylistic", "expressive" or "performative" functions can be isolated because the speaking subject and his utterance become not merely the transparent vehicle of expression and communication but the object of a self-conscious attention on the part of language turned back upon itself. These subjective aspects of language have been neglected for a long time by philosophical and linguistic semantics. Even so-called 'natural logic' (or logic applied to natural languages) and, more generally, modal logics are still considered to be deviant with regard to propositional and truth-functional logic where (Quinean) regimentation of natural language phenomena (deictics and modalities) is proclaimed a methodological necessity. A similar distrust in the subjective embedding of language fragments can be noticed in many trends of contemporary linguistics (structuralism, generative grammars): the *langue/parole* and *competence/performance* dichotomies have as a purpose to eliminate subjective embedding in the domain of the non-graspable residual. The pragmatic perspective on language and discourse on the contrary insists on the context-boundedness of meaning production, on the specificity of *discourse* bound rationality ('non-natural'-prescriptive inferences), and on the special heuristic function a theory of understanding has to the overall theory of language.

To reintroduce subjectivity as an operational category of linguistic theory (as Benveniste did in his *Problèmes de linguistique générale*) is not a move toward psychologism and subjectivism. Subjectivity here is not the individuality or the idiosyncratic personality of the speaker—something like a bundle of original and internal psychological states—but it exists only as a set of determined properties of the speaker's discourse. It is true that attention should be reintroduced to deixis and to the broad field of functioning of discourse as in demonstration (pronouns, demonstratives), argumentation and persuasion. But the expressivity of the speaking subject is subordinated to its communicability. Etymologically, the concept of communication stems from the two roots *communicare*, i.e. a one-way process of transmitting a content, and *communio*, i.e. a two-way process of sharing a content. In my view, the role of the I-sayer is not relevant but in an intersubjective community. The entire metaphors of contemporary linguistics suggests evidently that the production of linguistic sequences should be considered paradigmatic of linguistic activity (competence as a generative device, for instance). A pragmatic 'communicative competence', on the contrary, is a competence of *understanding* or 'discovering' the significance of discursive sequences. Thus there is an essential asymmetry between production and understanding in pragmatics. However, understanding is not a mental state or a specific experience—it is rather an extrinsic ability or an operation-in-the-world. Understanding is a practical, or interactional operation: to understand the I-sayer is to *delegate* to the speaker the First Person Authority.

Interpretation and First Person Authority are dialectically intertwined. On the one hand, an explanation for this authority can be found in the nature of the interpretive act, and one can argue that interpretation depends on the interpreter's delegating this authority to the speaker. On the other hand, there is no authority which is not intersubjectively valid (authority should be recognized as such—this is its 'essential' condition, as it is the case for all directive speech acts), thus delegation of authority is a *conditio sine qua non* of speaker's subjectivity itself. Given the nature of interpretation and understanding, First Person Authority follows. Given the special authority of the speaker which is not earned nor won but delegated, the act of interpretation or of understanding constitutes speaker's subjectivity. The First Person Authority, delegated by the community (the interpreters, and these are, in the end, all human beings on earth) to the I-sayer, consists of the fact that the I-sayer is *assumed* to have the right of the designation of the objects, events and states of affairs in the world, and foremost the subject, event and value he means by saying *I*. He does not have to know more about himself than the *role* he plays in interaction and intersubjectivity; saying *I* (*you, we, this / that*), the I-sayer refers to his role in the interplay, and he *intends* to designate himself in this role, and he is *assumed* to intend so by the community. Therefore, *I* is a designation rule or a designator, but *not* a rigid designator: *I* does not have a transparent referential function—on the contrary, *I* is an opaque condition on force and a principle

of rational and cooperative language functioning. This is what is understood when the First Person Authority is delegated and speaker's subjectivity constituted by the act of interpretation.

2. Shifting in

The workings of the *I* on discourse as a whole go further than the grammatical categories of demonstratives. The whole structure of language is organized around the *I*-sayer and his referring to the role delegated by the community. This is no doubt the essence of the pragmatic view on language, and it would be of great interest to see how this anthropologically oriented position on the subjectivity in language use would behave under testing conditions in empirical linguistics. However, an insuperable methodological difficulty arises around the *empiricity* of subjectivity in language. Even when the subject becomes the 'object' of a self-conscious attention on the part of language turned back upon itself, it is still the case that subjectivity "shows" itself without saying it propositionally. To say versus to show are classical contrastive categories, exploited by Wittgenstein, Austin, Benveniste, Bühler (see *Zeichen* vs *Anzeichen*).

Any methodology faced with the subjectivity "showing itself" will be reductionistic, and only a partially valid heuristics can be unfold. In my view, subjectivity-in-language "shows itself" (thus can be interpreted with its First Person Authority) by *deictics* and by *modalization*. Deictic and modal categories and operations are privileged approaches to subjectivity—they are partial and complementary heuristics. When discourse is *modified* by the *I*-sayer, it is in the first place by deictic and modal evidence. There are parallel typologies of deictic and modal modifiers. On the one hand there are *distributional modes* (grammatical categories in the strict sense, such as modal verbs and adjacent morphemes or syntagms), *propositional moods* (alethic, epistemic, deontic), *illocutionary modalities* (realized by actional units such as speech acts) and so-called '*axiological*' modalities (logico-semiotic deep structure categories determining actantial competences); on the other hand (and parallel with the first typology) there are again four types of "deictic modification": by the *I* as a grammatical-distributional rule, by the *I* as a propositional function, by the *I* as an interactional condition, and by the *I* as an 'axiological' principle (cooperation, veracity, coordination, authenticity, and so forth).

Within the deictic heuristics, one can say that the *I* is "shifting in" in discourse as a quadruple [rule, function, condition, principle]. I should restrict myself to two comments here. First, my view rests upon a particular organization of deictics. The theory of demonstratives and of deixis can take many shapes, only one of which is seen as relevant to the adequate theory of meaningful language use: the one in which the whole domain of demonstratives is concentrated around *I*. This is an *egocentric* theory of demonstratives, as opposed to the *ostensive* theory of demonstratives (Russell, for instance; the paradigm of demonstratives here is *this/that*) (see my paper "Demonstratives and the *I*-sayer", in J. vander Auwera (ed.), *The Semantics of Determiners*, Croom Helm,

London, 1980, 96-111). The egocentricity of the deictic domain has far-reaching consequences I cannot dwell upon here. My second comment is on the use of the notion of "shifter". It is evident that the way I introduce the term is far from orthodox with regard to Jakobson's use of the category. There are mainly three differences. In my view, not shifters as stabilized morpho-lexical categories are relevant, but *shifting* as a discursive (mainly sentential-textual) operation; moreover, shifting does not concern only the presence of the *I* as a grammatical-distributional rule: the total quadruple "shifts in" in discourse, and the workings of the *I* on these deeper levels of the proposition, the action unit and the actantial structure have a broader scope than just surface grammatical categories and operations; and finally, *shifting* in is in perfect balance with *shifting out*: there are procedures effectuated by the *I* as a quadruple of *being present* and of *being absent*. I consider this third property as almost universally forgotten in current linguistic theories, even in the pragmatically oriented ones.

3. Shifting out

It can be noticed in discourse analysis that some peculiar types of discourse are characterized by the fact that the I-sayer has the special authority to withdraw (to become 'absent'): this is the case with scientific and didactic discourse. The procedures of shifting out are generally less observable and truly complex and subtle: the *objective* way of speaking in scientific language use is, in fact, a subtle way of hiding the originating subjectivity with its general anthropological and also specific purposes and motives. But indirect communication as well, and all kinds of 'deviant' communication (lying, and manipulating in particular), are cases where the speaking subject is "shifting out" of his discourse. There are techniques of simulating the absence of performativity, expressivity and subjective investment in language fragments, and these are the phenomena which are of the greatest difficulty to be described and systematically explained. The problem consists of the fact that "shifting out" is an evident attack on communication and solidarity. The communicative *norm* is that the speaker *expresses* contents and his intention to be communicative.

Shifting out can mark all three aspects of deictics: personal or actantial deictics, temporal deictics and spatial deictics. *I* shifts out as a person by effectuating procedures where a kind of neutral *He* is realized (truth, for instance, is an instance of universality and it is not subject-bound). *Temporal shifting out* realizes a kind of "*u-topic*" time where temporal indexicality is put into brackets and idealized. *Topological* neutralization is equally possible when the speaking subject spatially located withdraws or manipulates the interpreter with regard to possible detection of spatial coordinates.

The Cooperative Principle, Maxims and Language Specificity

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The starting point for my paper is the assumption that though the Cooperative Principle and the maxims as abstractions are universal, metaphorical expressions, created through exploitation of a maxim, can be language specific.

Metaphorical expressions are not just ornamental accessories, but very essential part of a language. Therefore, in order to gain full understanding, one has to be able to understand metaphorical expressions as well. However, very often a non-native speaker of a language finds some metaphors in the language blocking his or her understanding. I will investigate to what extent and in what way such metaphorical expressions block a non-native speaker's understanding, and further, what constitute understanding of such expressions, when it is possible, in a cross-linguistic perspective.

I have chosen examples from metaphorical expressions in proverbs and will also look at "orientational metaphors" in Lakoff and Johnson (*Metaphors We Live By*, 1980). Metaphors in proverbs are of the most conventionalized type, but since they are such, they reflect the way of thinking of those in that particular culture. Also, a proverb in a language often finds an almost exact counterpart in another language. Donald Keene (Preface, *Proverbs*, 1982) writes, "Proverbs are at once unique and universal. Their uniqueness stems from special qualities that a people, a language or a region gives to the expression; their universality comes from fidelity to the human situations described."

I will present three groups of proverbs: (1) (2a) and (2b).

- (1) A proverb in Japanese has an almost exact counterpart in English.
- (2) A proverb in Japanese does not have such an exact counterpart in English, but there is in English one or two proverbs which match the one in Japanese as to its intention.
 - (2) is further divided into two subgroups:
 - (2a) The meaning of the proverb in Japanese can be inferred from its components if you know their meanings. That is, there is a part-to-whole relationship in the Japanese proverb.
 - (2b) The meaning of the proverb in Japanese cannot be inferred from its components. That is, there is no part-to-whole relationship.

Japanese proverbs

- (1) a. kabe ni mimi ari.
(Walls have ears.)
- b. kawa ni mizu hakobu.
(To carry water to the river.)
- (2a) c. mateba kairo no hiyori ari.
[If you wait, you'll have a good day for a voyage.]
(The end result is worth the wait.)
- d. mago nimo ishō.
(Even a packhorse driver looks good when he's dressed up.)
- e. marui tamago mo kiriyō de shikaku.
[You can make cubes out of a round (boiled) egg.]
(What is important is how to express it.)
- (2b) f. nai sode wa furarenu.
[How can I wave a long (kimono) sleeve when I don't have one?]
(How can I share anything with you when I have nothing myself?)
- g. miso o suru.
(To mash others' bean paste.
(To flatter.))
- h. mayu ni tsuba o tsukeru.
(To apply spittle to one's eyebrows to see clearly.)
(Note: This was once considered a way to prevent oneself from being deceived.)
- i. me kara hana e nukeru.
(To enter at the eye and go into the nose. (To be extremely clever.))

English proverbs

Walls have ears.

To cast water into the sea.

Everything comes to him who waits.
The net of the sleeper catches fish.

Fine feathers make fine birds.

Apparel makes the man.

A good tale ill told is marred in the telling.

If you squeeze a cork, you will get but little juice.

There is no such flatterer as a man's self.

[To apple-polish.]

To have one's wits about one.

The story must be taken with a grain of salt.

To be as sharp as a needle.

From Asahi Evening News, *Proverbs* (1982)

Expressions in square brackets are supplied by the present writer.

Metaphorical expressions in (2b) are either those which require culture-specific information, or those which involve idiomatic combinations of their component parts. They are the ones which most often block a non-native speaker's understanding. For example, in order to know the intention of *f* in the Japanese version, one has to have the information that the *sleeve* there is a kimono sleeve; for *g*, mashing been paste was tiring work in old days; for *h*, one has to know the information given in the Note; for *i*, one has to be familiar with that particular combination of the component parts.

For (2a), what characterizes the Japanese proverbs as a group here is that it is possible to get at their meaning from their components. And each of the components brings out an image common to the speakers of the two languages; for example, a good day for a voyage in *c*, and the shape of an egg in *e*. A packhorse driver in *d*, however, presents a problem: the mental picture a Japanese draws must be different from the one for an English speaker. Nevertheless, the message can be understood by an English speaker: even a boy who drove a packhorse in old days looked good when he was dressed up. Then how was he dressed usually? He wore shabby clothes. This last piece of information which is needed for interpretation of the proverb, or the feature "highlighted" in it, is not one of the semantic markers or distinguishers for a packhorse driver, if we speak in those terms. Or a stereotype of a packhorse driver does not contain that feature, either. The information is supplied as an extension of the stereotype or as a sort of pragmatic knowledge from the cultural background. If this is the information highlighted as essential for understanding of the metaphor, it should be given due consideration when we discuss meaning.

It can be argued, however, that those who do not know how a packhorse driver was dressed can still infer the meaning of the proverb. This is true. So we have the English proverb: "Apparel makes the man." We can change the Japanese proverb into "Anyone looks good when he is dressed up" if we are satisfied with a less colorful version of it, though it seems all the flavor the metaphor had is lost.

This question of understanding the meaning fully or less fully brings us back to the proverbs in (1). We have similar proverbs in the two languages, similar both in form and meaning. Even here, two persons from different language backgrounds are more likely to have different pictures of, for example, *wall* in *a*.

To summarize what I have stated so far:

In proverbs, we find many conventionalized metaphors. Some of them are language or culture specific as those in (2b) of my examples, but others are not. The metaphors which can be understood across linguistic or cultural boundaries present a different problem. That is, there are features or information needed for understanding of metaphors outside of so-called dictionary information which is usually presented in terms of markers, distinguishers or stereotypes. We have to either work our inference or refer to what we have as our cultural background, our pragmatic knowledge. In this case, our

pragmatic knowledge which is needed can either be of a universal type or of a language-specific type. Here reconsideration of what constitute meaning is needed.

Another point I would like to add is one of Lakoff and Johnson's orientational metaphors which have to do with spacial orientations. In the English conceptual system, they write, *up* is closely tied with happiness, and well-being in general. But what about the following three sentences?

Parliament is *up*. I'm afraid our time is *up*. It's all *up* with him. The *up* here signifies "to a state of completion, to an end." If completion is a fullfilment, it matches the cases of *up* which are tied with happiness or well-being, but in the sentences, especially in the last one, the sense of fullfilment is not at all observed. I should add the following to the summary: even in the same language, and with the same expression or linguistic form, what is highlighted can differ in the contexts, verbal and situational. This contextual information is necessary for understanding.

Semantics and pragmatics have to go hand in hand in comprehension of linguistic expressions.

"Two aspects of Pragmatics: Topicality and Iconicity"

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The "pragmatic" property of language is here understood to be the ready re-ferrability of utterances to what these are about. This re-ferrability concentrates, grammatically, on topicality mainly, i.e. the eligibility of certain constituents (normally: NPs) to maintain an identical topic during a discourse stretch. Lexically, this re-ferrability is called "iconicity".

Topicalization is by Subject in accusative syntax; in ergative syntax, by objects. (Syntactically ergative languages are shown to be far more numerous than currently supposed by most linguists.) Accusative Subject and ergative Object have one thing in common: unmarkedness. It is then shown that unmarked NPs are more iconic than marked NPs.

Thus topicality and iconicity are shown to converge pragmatically, strongest iconicity coinciding with highest topicality. Strong iconicity is independently confirmed by the use of the unmarked form as "citation form". Marked NPs are weaker in iconicity because of their "structural", as opposed to purely "pragmatic", properties. Thus "structural" ("grammatical") properties of language and "pragmatic" ("iconic") properties of language seem to be competitive in regard to one another, but they naturally converge in "unmarkedness" as the "structural" realization of lexical identity.

Language in Modern Drama as Compared with Authentic Spoken Discourse*

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In German drama since about 1967 one can observe a realistic direction, the language of which, in its faithfulness to reality, seems to surpass all previous conceptions of realism and naturalism. Its origins are rooted in the general political and cultural climate of the time as well as in influences from contemporary foreign dramatists such as Bond, Wesker, and Pinter, and in the rediscovery of two German authors of the twenties and thirties. In those years Marieluise Fleisser and Ö. v. Horváth took up the old tradition of the Volksstück (folk or milieu play) whose foremost representatives were the nineteenth century Austrians, Nestroy and Raimund, but in theme and language they differed considerably from this traditional type. In place of the witty dialogues of the worldlywise common folk who know how to turn a phrase, we get in Fleisser's dramas the plodding, often impoverished dialect of simple people in their Bavarian setting, a unique artistic idiom ("lingua Fleisser"), which became stylized under the direct influence of Brecht. It is this style, as well as the social criticism which is inherent in the creation of language and character, that has strongly influenced the authors of the contemporary Volksstück. Horváth, moreover, wanted his dramatic language to be understood critically. He was concerned with showing that the petty bourgeoisie of his time no longer commanded a genuine language of their own, but possessed rather a dialect shot through with a kind of pseudo-educated jargon ("Bildungsjargon"), whose false or empty phrases prevented them from recognizing the true state of affairs and hence from bettering their lot¹.

This historical background is of particular significance for the most successful contemporary writer of the Volksstück in the Federal Republic, F. X. Kroetz. In his early plays (e.g. 'Wildwechsel', 'Heimarbeit', 'Stallerhof', 'Oberösterreich') he sharpens Horváth's conceptions to the extent that the proletarian is literally shattered by the restrictedness of his code. Since the language of his characters is quite limited, they cannot grasp their situation, and solve their problems, if at all, extra-linguistically, i.e. violently. For other representatives of the new Volksstück (M. Sperr, W. Bauer, P. Turrini, F. Kusz

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1) Surveys of the development of the Volksstück are given in Hein (1973) and Innes (1979: 218ff.).

and many others, writing mostly in Bavarian dialect), a language faithful to the milieu is less the starting-point for dramatic conflict than the stylistic device for creating a realistic frame of reference for the action. Alongside this new interest in the Volksstück from a social and political motivation, German drama has also turned anew to the reflection of human problems in the subjectivity of the individual. Botho Strauss can be considered the principal representative of this so-called "sensibler Realismus". In his plays, located in the middle class, contemporary German appears as a language which has fallen into numerous jargons, his figures sliding back and forth between them. At the same time their language is laced with literary, poetic, and occasionally even mystical modes of expression. There is hardly anyone who better captures the conversational tone and the contemporary language of the media and particular groups than he does, and in this he is in part one step ahead of the documentation of contemporary language by linguists²⁾.

All of the authors just named have this one feature in common, that they all make use of a language which strikes the listener as genuine and natural. Linguists, stylists, and literary historians, such as Grosse (1972), Larthomas (1972), Page (1973) and Kennedy (1975), have pointed out independently of one another that the "persuasive effect of colloquialism" of speech in literature generally depends "upon only a very limited and selective observance of the features of actual speech"³⁾ and that especially in drama a new "language is created at some point on the line at which a tension between 'imitation' and 'patterning' takes place"⁴⁾. Although they all emphasize the importance of comparison with authentic dialogues, only Grosse draws upon research on the spoken language which has been undertaken in the Federal Republic since the mid-sixties⁵⁾. He concludes that over the last one hundred years the signals for indicating spoken language have changed in the lexical domain. But in syntax, certain fixed ideas persist, which are not confirmed by the analysis of authentic texts. Above all, he points out certain characteristics which are not usually found, such as redundancies, corrections, fresh starts, attention signals. Other devices, however, such as ellipsis and aposiopesis are traditionally used for the theatrical effects of conciseness and the heightening of tension. In the neo-realistic drama of the present, the catalogue of the syntactic devices used has no doubt burgeoned when compared with traditional stage language. Left- and right-dislocation is employed fairly regularly as an indicator of the spoken idiom⁶⁾. In Kroetz, forms occur which even go beyond these established conventions, and which allow one to recognize the breadth of expressive variation

2) One can compare the kind of texts which are to be found in the most extensive published corpus of spontaneous German conversation to date (Freiburger Texte I-IV) with the lesbian and punk jargon in a scene from Strauss' 'Kalldeuey' (first performed 1982).

3) Page (1973:4).

4) Kennedy (1975:15), cf. Larthomas (1972:175ff.).

5) Cf. the survey of research by Betten (1977/1978).

6) Ochs (1979:77) makes the same observation for English-language authors.

in spontaneous speech⁷⁾. Occasionally sudden switches in the middle of constructions are attempted as well as mixtures of anacolutha, parentheses, ellipses, resumptions, and fresh starts, which cannot easily be defined formally, just as we find them in authentic texts⁸⁾. Repetitions also occur frequently, for example, in countering sequences (as a sign of agreement or questioning) and for emphasis, both long practised in dramatic dialogue. But today repetitions are also found tightly packed in a brief sequence, e.g. as a sign of intellectual immobility (especially in M. Sperr)⁹⁾, or in longer passages as a reflection of the circular or spiral movement of thought (typical of T. Bernhard). To this extent, the repertoire of syntactic devices taken from the spoken language and used in these plays is broader than Grosse assumes.

On the other hand, even if a general tendency can be observed toward a loosening of the sentence structure prescribed by the norm of the written language, and even if no classification in terms of coordination, subordination, insertion, afterthought or ellipsis is feasible, still, understanding in reading and hearing remains a cardinal rule of dramatic dialogue, which unlike transcriptions of authentic dialogues is conditioned by its medium. The imitation of real speakers' permanent struggle for intelligibility, with such syntactic consequences as false starts and self-correction, would only distract us from the communicative interests of the author¹⁰⁾. If a device is selected from this area, then it is done in the traditional fashion to characterize the individual traits of a figure¹¹⁾, or more rarely, as an intentional sign of spontaneity. Signals indicating the dialogue structure and those used for ensuring comprehension¹²⁾ are likewise underrepresented, since they have no function under the communicative conditions of the theatre, although they play a significant role in face-to-face communication. Routine formulae such as introductory signals (openers or starters) are chosen primarily as indicators of spoken language, but in plays taken as a whole, these formulae usually assume another function, i.e. to create

7) Examples from Kroetz's 'Mensch Meier' and 'Der stramme Max': a) Left-dislocation: instances which deviate from the norms of the written language in the choice of the co-referential deictic pronoun: *Der Adlermist, wenn man ihn in die Augn kriegt, wird man blind! Mein Gott, ein Pilot, das is ebn gefährlich: Deutsche Mädchenwunder, da is sogar der König von Schweden gefangen!*—b) Right-dislocation with an anticipatory co-referential pronoun, not corresponding to the norm: *Das is ein Dieb, der Herr! Jetzt, wo es ausstandn is, die Angst.*—c) Right-dislocation or topicalization of the accusative object and verb ellipsis or elliptical sentence with an afterthought?: *Keinen vernünftigen Gedanken, der Mensch.*

8) Cf. Betten (1976).

9) An example from Sperr's 'Landshuter Erzählungen': Laiper: [...] *Der Sorm ist rücksichtslos.* Martha: *Ja. Richtig rücksichtslos ist der.* Laiper: *Prost! (Sie trinken) Wie ein Mensch so rücksichtslos sein kann.* Martha: *Der Sorm ist rücksichtslos.* Ja Laiper: *Sowas Rücksichtsloses.* [...]

10) Cf. Page (1973:11, 52f.).

11) Cf. Lecch/Short (1981:165).

12) In German "Gliederungs-" und "Verständnissicherungssignale", "Routineformeln", in English "attention signals" and "linking signals", "connectives", "gambits" etc.: for terms and definitions see Edmondson/House (1981).

and support a certain psychological atmosphere¹³). In this way, instead of functioning to move the dialogue in a particular direction, their task is similar to that of the lexicon and phraseology, which have always been the most significant levels for representing true-to-life language, since they best allow the connection between a depiction of the milieu and an implicit criticism of language.

More precise investigations show that despite a broadening of the linguistic repertoire, each of these dramatists still makes a limited selection from the phonological, morphological, lexical and syntactic resources to be found in spoken language, and thus creates his own unique language by combining them in various ways. For each of the leading authors, the individual selection stands in a close relationship to the content of the play concerned, by expressing something about the manner of communication and communicative problems, something which all of these dramas ultimately treat¹⁴). An investigation of the code needs to be supplemented by the analysis of typical speech-act sequences and strategies. The latter have been convincingly displayed by Ohmann (1973), Fish (1976), Noguchi (1978), Burton (1980), and in Hess-Lüttich (1980) for various dramatic texts. However, to regard this approach as the only meaningful contribution of linguistics to the analysis of literary dialogues appears to me just as one-sided and faulty as the previous concentration on single code phenomena. Linguistics today, given its knowledge of natural language, is capable of describing the particular quality of literary texts (and especially of fictional dialogues) with a view to revealing their conception of linguistic reality as well as their stylization, on both levels—the code itself and dialogue structure¹⁵). It should be the goal of stylistic analysis to work out the basic structural patterns and their largely complementary relations to one another in both areas. In doing so, it should reveal the essence of what the author intended to achieve on the superordinate level of communication between author and recipient.

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13) In Bauer, for example, the predominant introductory signals (starters) are *naja*, *na*, *hm*, *was wuß i*, which reflect the defeatism of his decadent Bohemian figures and contribute to the creation of this mood.

14) For Kroetz, described in exemplary fashion by Burger/v. Matt (1974).

15) One goal of my full-scale investigation of language in German neo-realistic drama (forthcoming) is to contribute to the debate on realism among literary scholars, by offering more precise judgments about the linguistic form of literary (especially dramatic) works.

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1203

“Metaphor and Poetry: Problems in a Hermeneutic Theory of Metaphor”*

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The purpose of this paper is to clarify the limitations of a hermeneutic theory of metaphor proposed by Paul Ricoeur by applying it to Japanese haiku poetry. It will be demonstrated that some of Ricoeur's notions used for the explication of metaphor present difficulties in interpreting haiku, Japanese traditional metaphorical discourse, to the extent that they are implicitly based on Western culture.

I. Major Traits of the Hermeneutic Theory of Metaphor

Main theories of metaphor so far proposed may be roughly divided into three types: i) rhetorical view, which regards metaphor as an ornament or for persuasion; ii) semantic view, which regards it as a transference of meaning; and iii) hermeneutic view, which regards it as a creative function.

The hermeneutic theory, to which I will confine myself here, is proposed by Paul Ricoeur mainly in his work, *La métaphore vive* (1975).¹⁾ He attempts to give an account of what he calls ‘living metaphor,’ defining it as “a strategy of discourse that, while preserving and developing the creative power of language, preserves and develops the *heuristic* power wielded by *fiction*. (Italics in the original.)”²⁾ The hermeneutic theory sets out to explore the problems of metaphor on the level of discourse which ranges from a sentence to a whole text, presupposing that metaphor is a poem in miniature.

This theory is characterized by two major problematics, i.e., metaphor as ‘conflicting structure’ and as ‘creative function.’ The hermeneutic view regards ‘conflict’ or ‘tension’ in metaphor as a crucial point. Metaphor is accompanied by tension between semantic impertinence and pertinence, between literal and metaphorical interpretations, etc.³⁾ In other words, tensive elements are mediated in metaphor by the work of predicative assimilation, although their conflicting structure is preserved. This tensional structure is regarded as essential to the creation of meaning and reality. ‘Creativity’ here signifies both semantic and ontological productivity of metaphor. Namely, metaphor constitutes a momentaneous creation of linguistic meaning generated by the

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procedure of contextual action on one hand, and renovates the referential meaning by redescribing reality in a new light on the other.

Thus, the hermeneutic theory provides a strategy for interpreting the world as redescribed by metaphor. Following Monroe Beardsley (1958),⁴⁾ Ricoeur assumes that the explication of metaphor is to serve as a test-case for the method of interpretation of larger entities such as the entire poem. That is, a poem is regarded as a metaphorical discourse in a broad sense. In order to examine this hermeneutic theory, let us apply Ricoeur's explication of metaphor in terms of 'conflicting structure' or 'creation through conflicting mediation' to haiku, Japanese traditional poetry, and see whether and to what extent his method holds good for the understanding of the Japanese traditional metaphorical discourse.

II. Application of the Theory to Japanese Haiku Poetry

Let us take up a following haiku poem by Bashō, and examine the applicability of the hermeneutic theory for it.

"Shizukasa ya/ Iwa ni shimiiru/ Semi no koe."

("Such stillness—/ The cries of the cicadas/
Sink into the rocks.") (Translated by Donald Keene.)

This poem was inspired when Bashō visited the Ryūshakuji Temple during his travel to the Northern part of Japan. It was near sunset in early summer. The mountain temple was quiet. There was a pile of numerous mossy, gigantic rocks. A few cicadas cried in a rather low, monotonous tone.

In this poem, 'shimiiru' (to penetrate) is a metaphorical word, and also a whole text itself is a metaphorical discourse, since it has equivocal meanings, not only speaking superficially of the natural silence, the voice of cicadas and the rocks, but also of something higher and profound in the poet's mind, i.e., the eternal stillness.

If we follow the hermeneutic theory, this metaphorical discourse could be explained as conflicting mediation between two opposing factors, e.g., between 'penetration' and its context—'the rocks,' 'the voice of cicadas,'—and between 'the voice of cicadas' and 'the surrounding silence,' etc. According to Ricoeur, the conflicts would not melt into one, but be put in confrontation, and through such conflicts a new meaning is to emerge and a new referential relation in reality is to be established.

III. Problems

It seems that at the semantic level, Ricoeur's explication is plausible as it clarifies the immanent sense of this haiku; however, at the hermeneutic level there arise the following questions. If as Ricoeur assumes, metaphor projects a world of its own, the way of being, then, should the metaphorical discourse in haiku be explained in accordance with the world of haiku, the way of being in Bashō's poem? Are the explanatory notions of hermeneutic theory, especial-

ly the notion of 'conflicting structure' or 'conflicting mediation' adequate for the understanding of the world in Bashō's haiku?

Bashō's words give us a clue: "Learn about pines from pines, and about bamboos from bamboos."⁵ What is meant by this is that if a haiku does not arise naturally from the object, the object and its observer become two, and the observer cannot realize the feeling of the object, since his self intervenes. This teaches that what a poet must do is to be one with what Bashō calls 'zōka,' i.e., to be fused with nature. Then, we come to a real awakening. Here, we find the idea which makes a sharp contrast to Ricoeur's notion of 'conflict.' It is the notion of 'original oneness.'

Let us return to our example and see how it works. The poem essentially describes the profound stillness prevailing in nature including the poet himself. Bashō pictures his theme with a touch of vivid action, the voice of cicadas, but only in order to intensify stillness. The voice may first be felt as disturbing; then, all the more a deepened mood of stillness prevails in the poet's mind. The voice of the cicadas is absorbed into the stillness. Thus, the voice penetrates the rocks. Stillness is resonant with the tranquility of rocks and the eternal loneliness of the poet. The metaphorical word 'shimiiru' represents this whole process of becoming still. The whole poem as a text metaphor crystallizes this moment of becoming or melting into the eternal stillness. Such sense of fusion in haiku is a way of 'satori,' i.e., awakening in Zen Buddhism, as R. H. Blyth (1949) says:

"haiku is a kind of *satori*, or enlightenment, in which we see the life of things. We grasp the inexpressible meaning of some quite ordinary thing or fact hitherto entirely overlooked. Haiku is the apprehension of a thing by a realization of our own original and essential unity with it, . . ."⁶

It is not 'the creation' in the Western sense nor is it 'conflicting mediation,' but an awakening or enlightenment by returning to the original oneness. It seems that the grasp of such unity cannot be well explained by the notion of 'conflict,' because the latter presupposes dichotomy of two objects.

So far, we discussed and demonstrated that Ricoeur's hermeneutic explanation of metaphor has some difficulties in interpreting a haiku. Why is it? What we notice when encountering such difficulties is the presupposed assumption of the explanatory notion of metaphor itself, i.e., the fundamentally dualistic or dialectical approach within the Western frame of reference.

Therefore, it can be pointed out that the hermeneutic problem of metaphor must be more concerned with the cultural aspect. It might be further questioned whether the notion of 'metaphor' itself basically reflects its etymological origin of the Greek *metaphora* (to carry over), and then, it will be asked to what extent it could adequately account for seemingly similar figures in non-Western languages such as Japanese, since the Japanese 'yu' in the term 'inyu' or 'hiyu' means 'to make aware,' or 'to realize.' These problems wait for further

research.

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The Tone of a Text in Linguistic Terms

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"Tone" is a term frequently used in the description and analysis of texts. In his *Practical Criticism*, I. A. Richards suggested that tone reflects the speaker's attitude to the listener,¹⁾ but otherwise the term seems hardly ever to be defined, and the question of how the tone arises out of the text has been neglected. In the present paper an attempt will be made to explore the meaning of the term as it is actually used and to find out to what extent and in what way tone is conditioned by linguistic facts. It will appear that the use of the term is not limited to the relationship mentioned by Richards. We will be concerned in the first place with prose fiction, and examples will be taken chiefly from the literature of English-speaking countries.

"Tone" is basically an auditory term, but in the case under discussion it is used figuratively and has near-synonyms such as "spirit", "style", "character", "tenor", "atmosphere", "vein". In fact, many of these enter into the definitions given for "tone" in our sense in the dictionaries. Here is an instance: "A special or characteristic style or tendency of thought, feeling, behaviour etc.; spirit, character, tenor; *esp.* the general or prevailing state of morals or manners in a society or community."²⁾ It seems advisable to differentiate our "tone" from its near-synonyms, especially "style". Let us say that "tone" indicates the non-cognitive part of the response evoked by a text. Under this definition tone is not normally associated with a purely cognitive content. Cf. Bühler's Symbol function below.

The actual sense in which a term is used can be shown with a certain degree of objectivity by examining its collocation with other words. This method has been applied to the term "tone" as it appears in c. 30 items of the series *Writers and Their Work* and in a few other works of literary criticism. We find a large number of adjectives collocating with *tone*. Most of them represent the following semantic fields:

- 1) an inner, individual field, in which adjectives reflect mental attitudes which may be
either comparatively permanent (temperament), e.g. *brooding, dispassionate, grave, light, melancholy, pessimistic, restrained, serious*. A subvariant of this type is associated with drama, e.g. *comic, burlesque, farcical, tragic*; or comparatively temporary (temper), e.g. *aggressive, angry, exuberant, facetious, indignant*;
- 2) an outer, social field, in which adjectives reflect

either manners and morals, e.g. *blasé*, *brutal*, *emotional*, *flippant*, *moral*, *pathetic*, *satirical*, *sentimental*, *sophisticated*, *witty*, *wry*; or esthetics, e.g. *elegant*.

Some of these categories naturally shade into each other. What strikes one is the absence of auditory adjectives. Such terms seem rather to collocate with note (a deeper note, a false note).

A distinction should be made between the *basic* or *underlying* tone of a text and its *surface* tone(s). This distinction is borne out by many passages in the material. Thus Elizabeth Bowen is said to treat a tragic theme in the vein of comedy³), and Anthony Burgess' comedy is described as keyed in a pessimistic tone⁴). The most glaring instance is perhaps Swift's *Modest Proposal*, which has been described as "a masterpiece of macabre humour, written from a fund of despair"⁵). A similar instance in modern fiction is Achebe's novel *A man of the People*, which is reviewed as follows: "...a sparkling piece of satirical virtuosity, yet we feel throughout that deep anger, bitterness and disillusion are never far beneath the surface."⁶)

The surface tone is more accessible to linguistic analysis than the basic tone. Several linguistic models come to mind as possible instruments for this analysis. There is no doubt that Bühler's Organon model, in which the three functions of symbol, symptom and signal are distinguished, is of some relevance here. The same is true of similar later models, such as those found in speech act theory. But for a more detailed analysis these models seem to be insufficient. Let us turn instead to Crystal's and Davy's well-known model in *Investigating English Style*.⁷) The authors here account for the levels of linguistic description—phonetics/phonology, grammar, vocabulary, and semantics—and relate them to nine dimensions of situational constraint, namely, individuality; dialect; time; discourse, comprising medium and participation; province; status; modality; and singularity. Let us briefly consider the relevance of these dimensions for the rise of a surface tone.

Individuality. It is easy to point to individual features of style that contribute to a specific tone, e.g. Henry James' gerundial and relative constructions and Samuel Johnson's latinising vocabulary. *Time* is all-important for the surface tone e.g. in Scott's historical novels. In Golding's *The Inheritors* the extremely simple syntax and vocabulary of the dialogue helps to visualize a primeval community. *Discourse*: direct speech in a novel necessarily adds a dramatic tone to the text. Dialogue, a basic structural device in the novels of Ivy Compton-Burnett and Henry Green, makes a powerful contribution to the surface tone of these novels. *Dialect*. Regional dialect has been used by many writers, often to create a comic tone, but in more modern times also for serious or even tragic matter, as in Dickens' *Hard Times*. (an example of functional change in the field of tone). Class dialects mostly contribute a comic tone.

Province. Legal language contributes much of the tone in Dickens' *Bleak House*, as does the language of religion in Graham Greene's *The Heart of the*

Matter and political language in Meredith's *Beauchamp's Career* and Joyce Carey's *Prisoner of Grace* with its sequels. *Status*. The social relationships between protagonists is decisive for the surface tone e.g. in Forster's *A Passage to India*, Alan Paton's *Too Late the Phalarope* and Kingsley Amis' *Lucky Jim*. *Modality*. Joyce Carey's *Not Honour More* starts out affecting the tone of a shorthand report. In Pepys' *Diary* the surface tone is surely conditioned by the genre to which the text belongs. *Singularity*. The outstanding instance here is James Joyce's *Finnegans Wake*. A surface tone based on singularity is also found in Anthony Burgess' *A Clockwork Orange*.

To these dimensions we might add Place as a counterpart to Time and exemplify it with Egdon Heath in *The Return of the Native* and the moors in *Wuthering Heights*. We might also add Persona, or Mask, as a potential contributor to the surface tone; or else it can constitute a tone level intermediate between the other two levels.⁸⁾ Normally, author and reader share the same or very similar attitudes to the concepts expressed in terms of these dimensions. If that is not the case, misunderstandings or reactions not aimed at by the author may arise in the reader. A regional dialect may, for instance, cause widely different reactions. Cousin Feenix's class dialect in *Dombey and Son* may not even be noticed by some readers.—It should be added that the surface tone frequently changes in the same work, thus e.g. in Sterne's *Sentimental Journey* and in Dickens' *David Copperfield*, where the Little Em'ly text and the Micawber text are in glaring tonal contrast.

The basic tone, on the other hand, may be complex, but it is normally homogeneous throughout the work. It is one of the unifying elements of the text. The dimensions of situational constraint discussed above do not suffice to explain the basic tone. It may, but need not, coincide with the surface tone. It is a function of the writer's vision and as such, in Bühler's model, associated with the Symptom aspect rather than the Signal aspect; the latter ties up with Richards' definition of tone (see above).

Like the surface tone, the basic tone may be lost on the reader. *Gulliver's Travels*, superficially a travel story, conveys a basic tone which is missed by children and indeed by many adult readers. The basic tone, too, may be replaced by another in the mind of the reader, depending on his different outlook, taste, experience, situation etc. This seems to be the case with most Gothic novels, which were meant to inspire terror but now tend to appear ridiculous. The basic tone may also be genuinely ambivalent or complex: the ending of Hardy's *Far from the Madding Crowd* is a mixture of tragedy and comedy and apparently intended as such.

A serious or even tragic basic tone is frequently associated with a comic surface tone, thus e.g. in Fielding's *Joseph Andrews* and in several of Elizabeth Bowen's novels. The reverse, a comic basic tone coupled with a serious surface tone, seems to be less common in novels. It is, of course, one of the techniques of parody. In recent times it has been manifested in so-called sick humour.

We have found that linguistic models, more particularly in the pragmatic

terms of situational constraint, can help to explain the rise of a surface tone, whereas they are less adequate in connection with the basic tone. The basic tone emanates from the author's vision and is subjected to the same uncertainties as that vision.

The two levels of tone contribute in interaction to the overall impression conveyed by the text. Perhaps we should reckon with more than two levels, e.g. in cases where a persona or mask is involved. This question requires further study. For the overall impression established by the interaction of tone levels the term "mood" might be appropriate.

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Chinese Elements in the Tangut Script

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1. Introduction

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The Tangut script as promulgated in 1036 is generally regarded as one of the most complicated writing systems in the world. Due to the efforts of many scholars, especially Lo Fu-ch'ang (1914), Nishida (1961, 1964, 1966), Kyčanov (1964) etc. and to the translation of the *Wen-hai* and the *Wen-hai tsa-lei* into Russian by Soviet scholars (Keping et al. 1969), we have come to understand the composition of quite a number of Tangut characters.

There is no doubt that in the formation of the Tangut script the Chinese writing system played an important part, but it remains obscure exactly what kind of influence the Chinese writing system exerted on the Tangut script. Some attempt has been made to look for the origins of Tangut Characters in the Chinese script. An excellent study of this sort was made by Kyčanov (1964) and further investigation in this direction is certainly worthy of being pursued. However, in the present paper I shall focus my attention on the principles underlying the formation of the Tangut script and try to determine the traces of Chinese influences in them.

2. The Influence of Chinese Vulgar Script on the Tangut Script

2.1. Graphs with the left and right side reversed

In the Chinese script there are graphs which contain the same elements in different positions. They are sometimes variants of the same graph and sometimes different graphs with different sounds and/or meanings.

In the *Lung-k'an-shou-chien* 龍龕手鑑 a dictionary of Chinese characters compiled by a Kitan monk around A.D. 997, such graphs appear on a large scale. The dictionary is said to have recorded the vulgar script actually used in the manuscripts of Buddhist sutras. It is all the more significant, since this immediately precedes the invention of the Tangut script around A.D. 1036. As we find the same method of graphic composition extensively applied in the Tangut script, we have to conclude that it owed to Chinese influence. Following are but a few examples of this kind of graphic composition in the Chinese and Tangut script:

Chinese: 𩇛 vulgar
𩇛 current
𩇛 vulgar

𩇛 regular
𩇛 regular
𩇛 regular

to whirl, as the wind
pustule, pimple
plume

Tangut:	𐞳 1jə²	earth	𐞳 phu¹	earth
	𐞳 khwei²	big	𐞳 liɛ²	big
	𐞳 sjwɪ¹	ngin²		whirlwind

It is interesting to note that some Tangut graphs standing for Chinese loanwords are formed in this way.

Tangut words	Chinese loanwords	Chinese words
𐞳 dɪə¹ break	𐞳 swei¹ break	碎 suai⁰ break
𐞳 niɛ¹ wild animal	𐞳 sai¹ wild animal	牲 ɔ.sɒŋ animals

2.2. Graphs formed on the principle of semantic compounds with "not" as its constituent element

In the presentday Chinese script, graphs composed on the principle of semantic compounds with "not" as its constituent element are extremely rare. The graph 覓 "to search for" is composed of 不 "not" and 見 "see". In the Yü-p'ien it is registered as a vulgar script for 覓. Another graph in current use is 歪 "aslant, askew", which is composed of 不 "not" and 正 "upright". This graph seems to have made its first appearance in the *Lung-k'an-shou-chien* together with the following graphs which are no longer in use today:

𐞳 𐞳 < 不	𐞳 𐞳 < 不	𐞳 𐞳 < 不	𐞳 𐞳 < 不
𐞳 𐞳 < 不	𐞳 𐞳 < 不	𐞳 𐞳 < 不	𐞳 𐞳 < 不
𐞳 𐞳 < 不	𐞳 𐞳 < 不	𐞳 𐞳 < 不	𐞳 𐞳 < 不
𐞳 𐞳 < 不	𐞳 𐞳 < 不	𐞳 𐞳 < 不	𐞳 𐞳 < 不

The Tangut script made extensive use of this method and composed dozens of graphs with the graphic element 丩, which is an abbreviated form for 𐞳 mi: "not". Following are examples:

𐞳 ywai¹ < 丩	𐞳 𐞳 < 丩	𐞳 𐞳 < 丩	𐞳 𐞳 < 丩
𐞳 dzwa¹ < 丩	𐞳 𐞳 < 丩	𐞳 𐞳 < 丩	𐞳 𐞳 < 丩
𐞳 li¹ < 丩	𐞳 𐞳 < 丩	𐞳 𐞳 < 丩	𐞳 𐞳 < 丩
𐞳 gie¹ < 丩	𐞳 𐞳 < 丩	𐞳 𐞳 < 丩	𐞳 𐞳 < 丩
𐞳 ma¹ < 丩	𐞳 𐞳 < 丩	𐞳 𐞳 < 丩	𐞳 𐞳 < 丩

2.3. Graphs formed on the principle of fan-ch'ieh

In translating Buddhist sutras into Chinese, the necessity arose to render Sanskrit sounds into Chinese script. Since, in Sanskrit, there were sound combinations which did not occur in Chinese, the transliteration did not always give the exact Sanskrit sound. For instance the syllable *dhya* did not occur in Chinese, so it was rendered either in two syllables as *diei-ja* 第耶, or simply as *dā* 𐞳.

In the course of time, there appeared in the new translations of sutras an attempt to coin graphs by the fan-ch'ieh method, that is, to mark the sound of a character by means of two characters. The first character was used to mark the initial and the second to mark the final. For instance, in the *Hsin-i-shih-*

li-ching 新譯十地經, we find the Sanskrit upādhyāya rendered as 烏波于耶 *uo-puā-dia-ja*. The new character 𐰚 *dja* is made up of two characters 亨 *dieng* and 也 *ja*. The *Lung-k'an-shou-chien* (A.D. 997) has recorded twenty-odd such characters. Following are only a few examples:

𐰚 *tja* < 丁 *tieng* + 也 *ja*
 𐰚 *tja* < 丁 *tieng* + 夜 *ja*
 𐰚 *dja* < 亨 *dieng* + 夜 *ja*
 𐰚 *nja* < 寧 *nieng* + 也 *ja*

In the *T'ung-yin*, a Tangut dictionary compiled in A.D. 1132, the Tangut characters formed on the principle of fan-ch'ieh are marked with the two component characters. Following are examples:

𐰚 *tia*² < 𐰚 *ti*² + 𐰚 *ia*²
 𐰚 *thia*² < 𐰚 *thi*¹ + 𐰚 *ia*²
 𐰚 *dia*² < 𐰚 *din*² + 𐰚 *ia*²

3. Chinese Elements in the Tangut Script

3.1. Graphs similar in shape between Tangut and Chinese

In the Tangut writing system there are graphs, which on the whole resemble Chinese graphs of the same meaning or sound. For example, the Tangut graphs 𐰚 *men*¹ "door", 𐰚 *kuo*² "teeth" and 𐰚 *sə*¹ "family name" look like Chinese 門 (presumably the vulgar form for 門 "door"), 齒 (a vulgar form for 齒 "teeth") and 𐰚 *si* respectively. The Tangut radical 𐰚 "metal" also bears a striking resemblance to Chinese 金, a vulgar form for 金 "metal". Tangut graphs of this group are direct imitations of Chinese characters.

3.2. Graphs similar in construction between Tangut and Chinese

Another type of Chinese influence can be observed in the construction of some Tangut characters. The Tangut 𐰚 *liwu*² is a loanword from Chinese 呂. Since the Tangut graphic element 𐰚 "mouth" corresponds to the Chinese graph 口 "mouth", there is parallelism between Tangut and Chinese in piling up the same elements. The Tangut 𐰚 *dzin*² "single" and 𐰚 *xiow*² "a pair" are constructed on the model of Chinese 隻 "single" and 雙 "a pair" (vulgar forms of 隻 and 雙). Here the same elements are put side by side instead of being piled up.

3.3. Chinese phonetic elements in the Tangut script

In the Tangut script the graphs standing for Chinese loanwords are often formed by adding some radicals to Tangut graphs with the same meanings. For example:

Tangut
 𐰚 *keu*¹ family name
 𐰚 *seu*² family name

Chinese
 高 *kāu* high, family name
 小 *xiǎu* small

Tangut
 𐰚 *bin*² high
 𐰚 *tsi*¹ small

The most important thing here is that these graphs are read as in Chinese

according to the meanings of their right parts. Without a knowledge of Chinese it would be difficult to understand how these graphs are made up and how they are pronounced.

3.4. Chinese semantic elements in the Tangut script

In the Tangut script there are graphs, the composition of which is incomprehensible, unless Chinese semantic elements are assumed to be present. For instance, the graph swa^2 "grief, sad" has the graph su^2 "to be like" as its constituent. Since no semantic connection seems to exist between them, we are inclined to think that fsu^2 is the phonetic in swa^1 . As a matter of fact, these two graphs are used in translating Chinese words 猶 jiu "to be like" and 憂 yu "grief, sad", which are homophonous in the Chinese dialect known to the Tangut. This example shows that the two Tangut graphs are connected through the medium of two Chinese homophonous words semantically equivalent to each of them.

4. Concluding Remarks

In the foregoing I have tried to outline the scope of Chinese influence on the formation of the Tangut script. I have traced not only the principles of formation, but also the shape and structure of some individual Tangut graphs back to Chinese origins. The most conspicuous thing in the Tangut script is that Chinese phonetic and semantic elements are mingled together with Tangut elements in forming graphs.

Motivation and Methodology in Reforming Writing Systems On the Emergence of the Vocalization Systems of Hebrew

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1206
In this paper I propose to deal briefly with a chapter in the history of linguistic thought that is not represented in the way it deserves by historians of linguistics, namely with some aspects of the theory that underlies the reforming of the traditional writing systems of three Semitic languages, Syriac, Hebrew and Arabic. To a certain measure, my presentation will constitute a continuation of a number of points raised, some twenty years ago, in my monograph *The Vocalisation Systems of Arabic, Hebrew and Aramaic* (The Hague, 1962). Since its publication, significant work has been done in the field, and there is, therefore, room for a fresh look into the subject. In this paper I shall deal mainly with Hebrew.

The traditional orthographies of Hebrew, Syriac and Arabic denoted only some of the vowels, but even for these vowels the graphic representations—by the so-called vowel-letters—were multi-valent.

This was the situation before certain reforms in the orthography were introduced. In Syriac, beginning with the fifth century A.D., in Hebrew with the sixth or seventh, and in Arabic with the seventh, special signs were invented to denote the missing vowel phonemes. These signs are known as the Vocalization Systems (henceforth: *VS*). Although, because of the sacred nature of the texts, the signs were placed above and underneath the letters (occasionally also alongside a letter), so that the linear spelling of the words was not altered, what actually happened was a reform in the orthography, and one that has changed the dependence of the reader on memory and context.

These reforms, particularly in Syriac and Hebrew, were the outcome of exacting and continuing processes, lasting over several centuries. They comprised a number of stages, requiring an analysis of sequences of phonetic segments on the one hand, and the establishment of the notion of the phoneme on the other. Our sources for the reconstruction of the history of the *VS* vary from one language to the other: while Syriac provides ample manuscript evidence for the early stages, Hebrew has comparatively restricted information of this kind, though significant information is provided in the writings of the early biblical philologists (the Masoretes). Arabic has evidence of the two kinds.

An analytical principle of methodological significance, established early in the Syriac and Hebrew philological schools in which the *VS* were created, was that of *phonetic contrast* (henceforth: *PC*). A further step in the analytical process was, as we shall see, the establishment of the notion of *phonemic opposi-*

tion. We shall briefly describe the emergence of these concepts in one of the Hebrew philological schools. In this school, the Tiberian, the concept of *PC* became evident and was established by relating the ultra-short vowels to the "full" (or ordinary) ones.

In the reading tradition carried over by this school, this quantitative feature was considered phonetically relevant: its vowel system in its entirety consisted of seven phonemes, each of which had an ultra-short counterpart:¹⁾

ī	u		ī̇	ū̇
e	o		ē̇	ō̇
ε			ε̇	
a	ā		ā̇	ā̇

Although this feature was of no phonemic significance, the Tiberian philologists took great pains to represent it clearly. It was first indicated in minimal pairs of homographs differing merely by having a "full" vowel versus an ultra-short one. For the member of the homographic pair which has a full vowel (usually /a/) the term *milkel* ("above", "upper") was used, while its counterpart, having an ultra-short vowel, was denoted by the term *milra* ("below", "lower"). These terms quite probably referred to a pair of diacritical dots, one of which, placed above the word indicated a "full" vowel, while the other, underneath the word, denoted an ultra-short vowel.²⁾

Later on, a special sign—the *šewā*—was created, whose function was to denote this feature of ultra-short duration. The *šewā* sign was used also to denote zero, namely the non-existence of any vowel. Thus this sign became phonetically bivalent. This bivalence may be accounted for by the assumption that the Tiberians regarded zero and ultra-shortness (of vowels) to be two entities in complementary distribution, the occurrence of either of them depending on syllabic structure.

Alongside these linguistic considerations there was another reason for the use of a sign for zero by the Tiberians. In introducing their reform, these philologists established a principle that may be formulated as follows: no letter representing a consonant should appear without a vocalization sign (unless it comes at the end of a word). If a letter representing a consonant does not have a sign denoting a vowel, it must have a *šewā* sign. This function of the *šewā* can be defined preventive: if no sign denoting zero would appear under the letter, the reader might assume that a vowel had to be supplemented here, and an erroneous reading would result.

1) Most of the ultra-short vowels were of allophonic status. One, /ā̇/, had a phonemic status, but its functional load was exceedingly light. Cf. my *Voc. Syst.*, p. 22, footnote 17. For the structure of the vowel system of Tiberian Hebrew see now my paper, Some Notes on Šelomo Almoli's Contributions to the Linguistic Science of Hebrew, in *Interpreting the Hebrew Bible* (E.I.J. Rosenthal Festschrift, ed. by J. A. Emerton and S. C. Reif, Cambridge, 1982).

2) For another view see A. Dotan, Masorah, *Encyclopedia Judaica*, Vol. 16, cols. 1431-1432.

Let us now deal briefly with the concept of *phonemic opposition* (henceforth: *PO*) and its graphic notation. Once again the evidence is furnished primarily by the Tiberian school, in the history of which this concept emerged—apparently quite early—out of a phonological analysis of a prosodic feature. Hebrew possesses two kinds of stress: primary and secondary. Both are phonemic, although the functional load of the second is rather limited. The Tiberians noted the distinctive role of the secondary stress—which they contrasted with *zero* stress—and created for it a special sign, the *giʿyā* (the primary stress was denoted by the accents of cantillation). Their scale of stress thus consisted of three degrees: /Ø/ (zero stress), /V/ (secondary stress), and /V̇/ (primary stress). /Ø/ and /V/ contrast, as well as /Ø/ and /V̇/; /V/ and /V̇/ do not contrast.

Compare the following example:³⁾

⟨yirʔw⟩ “they will be afraid” = /yirʔu/ = [yirʔu]
 versus ⟨yrʔw⟩ “they will see” = /yrʔu/ = [yrʔu].

The principle of *PO* was also applied in the process of extracting the vowel-phonemes out of the phonetic segments and consequently creating the proper signs to denote these phonemes. In the initial phase of this process there is no notation of specific vowels. The focal problem then was how to distinguish *minimal pairs* of words, which differed only in one vowel-phoneme. The approach of the philologists to this problem may be termed “*logographic*”—since it is concerned with the whole word as a linguistic unit—and is marked by the notion of *relativity*.

The differentiation of the minimal pairs was achieved by establishing oppositions between individual vowels and groups of vowels, and by marking these oppositions. For the latter purpose the terms *milʿel* and *milraʿ* (see above) were once again applied, but their meanings in this capacity were different.

Milʿel denoted a word whose differing vowel-phoneme possessed a relatively back-articulatory feature, while *milraʿ* denoted a word whose differing vowel-phoneme had a relatively front feature (when two back vowels had to be differentiated, *milʿel* represented a word with a higher vowel while *milraʿ* stood for a word with a lower vowel).

Thus, these relationships were obtained:

$$\text{mil}^{\text{el}} \left\{ \begin{array}{l} /u/ \text{ and not } /ā/, /a/, /e/, /i/ \\ /o/ \text{ and not } /ā/, /a/, /e/ \\ /ā/ \text{ and not } /a/, /e/, /i/ \end{array} \right\} \text{mil}^{\text{ra}}$$

Cf. the following example:

⟨bnʿr⟩ *milʿel* = /banoʿar/ “in young age”
 ⟨bnʿr⟩ *milraʿ* = /banaʿar/ “in the young man”

(a note on the marking on *milʿel* and *milraʿ*: it is possible that these terms refer

³⁾ Angle brackets denote the orthography of the forms.

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to dots placed above and below the words; but while Syriac Mss. do reflect this practice, Hebrew Mss. do not provide substantial evidence for it).

By establishing these relationships and ascertaining the functional role of the vowels within the sound system, the Hebrew philologists have made the final step towards establishing the concept of the distinct vowel phonemes. The marking of the particular vowel phonemes by special signs then became a technical matter.

On the Linguistic Basis for the Use of Capital Letters

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1207
The point of departure in our examination of the linguistic basis for the use of capital letters in orthographic systems is the proposition that spelling is a relatively autonomous subsystem within the system of the written or standard language and that it fulfils specific functions in social communication, the most important of which are the notational function on the one hand and the comprehension function on the other. In the case of the notational function (i.e. the conversion of thoughts or speech into writing) it is the interests of the speller which are primarily at stake. For him the graphic norm is a guide to action which he must follow. In opposition to this, the comprehension function (i.e. the conversion of writing into thought and/or speech) primarily affects the reader's interests. For him the graphic norm represents a pattern of expectations and, apart from the fact that the written text should provide the opportunity of reproducing its sound form, the main consideration is that it should facilitate the rapid comprehension of meaning. This task consequently leads to the additional graphic marking of certain elements and relations in the written language which are relevant for the comprehension of meaning.

One of the specific graphic means which serve the comprehension function are capital letters. All modern European languages make use of this graphic means. Capital letters only took on their special functional value in the course of the second millenium A.D. In the ancient world there was no distinction between small and capital letters. A script was used, which from the present-day point of view consisted exclusively of capital letters. In connection with the development of cursive scripts for everyday use an increasing differentiation took place within the majuscule script between the seventh and ninth centuries and in this way scripts consisting exclusively of small letters emerged in several European countries. Letters from the old majuscule script were re-introduced into this minuscule script as capital letters in order to specially mark certain positions in written text. Capital letters thus acquired a special functional value against the background of small letters, which now formed the basic script, the unmarked form. By means of capital letters certain positions in text and also certain elements within sentences were additionally marked and thus emphasized for the reader.

In part this development proceeded differently in the various European languages both over time and as far as the result is concerned. Today, however, capital letters represent an inventory of characters parallel to that of the small letters, the elements of both inventories forming a set of paired variants. As

a grapheme the capital letter not only expresses a relation to the phoneme, however, but over and above this gives further additional information which does not refer to the phonological level but to features of other levels of the language system. Besides this, capitalization can on occasion also be used to express respect and deference for the thing denoted. Capital letters fulfil in European languages a number of tasks connected with the expression of relationships between the graphic level and the other levels of the language system and are in this connection polyfunctional. In contrast to the small letters they make clear the relationships of the graphic level

- to the textual level by marking headings, headlines and titles
- to the syntactic level by marking the beginning of sentences
- to the lexical level by marking certain words, word groups and word-classes or subclasses.

Particularly with regard to the expression of the last of the above-mentioned relationships the development of the European languages has led to two different results in the use of capitals: a minority type, represented today only by German; and a majority type, represented by all other European languages.

In the majority type the use of capital letters is essentially limited to the marking of proper names; either in the narrow or the wider sense. Obviously it is proper names which occupy such a special position in the lexical system that in all European languages their graphic emphasis by capital letters is regarded as necessary in the interests of the comprehension function. Notwithstanding this basic correspondence the orthographic rules governing such matters in various languages of the majority type do vary in detail, however. Thus the designations of members of a tribe or nation are regarded as proper names in a number of orthographic systems and are consequently spelled with an initial capital letter. e.g. in English, French and Czech; in other systems they are regarded as common nouns and therefore spelled with an initial small letter, e.g. in Russian, Spanish and Hungarian. Both the origin of such differences and the difficulty of arriving at exact rules in this sphere lie in the problems involved in defining and delimiting proper names. In the minority type the use of capital letters extends far beyond the sphere of proper names and includes the whole noun-class and those words which can be understood as nouns (nominalizations). The effectiveness of the principles governing the use of capitals in the majority type is in this way considerably reduced and the graphic prominence given to proper names only becomes apparent when these consist of word groups which do not begin with a noun (e.g. Schwarzes Meer, Deutsche Demokratische Republik). What is beyond all doubt, however, is the fact that the extension of capitalization to the whole noun-class and the consequent shifting of the basis for decision onto a higher, primarily grammatical, level has led to considerable difficulties, especially for the speller. Here, too, the reason for this lies in the absence of a clear boundary between the noun and other word-classes. Even a most comprehensive apparatus of orthographic

rules, which has attempted with increasingly detailed stipulations to delimit the fluid boundaries surrounding the noun, is in many cases incapable of ensuring a clear decision. All published analyses of mistakes in German orthography prove that such difficulties play an important role in actual written communication in German.

This situation inevitably leads to the question whether the rules of capitalization in German ensure a balanced fulfilment of the functions of writing, whether the considerable difficulties created for the writer appear to be compensated or justified by the efficiency of these arrangements for the reader. The relevant empirical investigations into question of reading and comprehension so far published arrive at different conclusions concerning the role of capitalization in this respect. A few investigations conclude that if general capitalization of nouns were abolished some kinds of reading would be made easier than under present arrangements. Others claim that this kind of change would entail certain disadvantages at first, especially in rapid silent reading. However, the negative effects predicted are in no case so significant that they could not be eliminated by the process of habituation.

Contrary to what is widely assumed, the findings of empirical reading research do not give any substantial support for the retention of general capitalization of nouns in German. They confirm from a linguistic point of view that the whole noun-class in German does not occupy such a special position either semantically or syntactically or intonationally as to justify giving it general prominence by graphic means. The categorial semantics of the noun is so broad that items from all other word-classes can be transferred to this categorial type. This hardly justifies giving special graphic marking to this class on semantic grounds. From the syntactical point of view the noun is strongly polyfunctional in German; and as far as intonation is concerned nouns are by no means the words which are always stressed in the sentence. We thus come to the conclusion that not only do the rules of capitalization in German cause considerable difficulties for the speller but they also do not really offer any benefit, or at least any substantial advantage, for the reader which would compensate for the above-mentioned difficulties. For this reason we consider that the orthographic rules governing capitalization in German should be brought into line with the majority type. This would bring a more balanced fulfilment of the two main functions of writing.

This kind of solution for the German language has been demanded for a long time but it is only now, after detailed research on this question and after agreement among linguists from the four German-speaking countries, that it has come within reach. The main linguistic problem is to achieve a orthographically practicable definition and delimitation of proper names. The different solutions of this problem arrived at in the other languages of the majority type are a clear indication of the difficulties involved. They result from the fluid boundaries of this lexical class—a problem, incidentally, which confronts any set of rules which attempts to mark a certain class or subclass of words by

capitals. For this reason no new arrangements can be expected to ensure the complete elimination of all difficulties, though they certainly can lead to an improvement by means of greater functional consonance.

It is not the intention of this paper, to present our detailed proposals for a new set of capitalization rules in German. The principles of our proposals derive from the theory of centre and periphery of linguistic phenomena elaborated in Czechoslovak linguistics. We attempt to determine an orthographically practicable concept of proper names by the extensional method of listing its subclasses. In this way we attempt to arrive at a solution which combines relative unequivocality, clarity and stability with practical ease of application. Only the future will show to what extent we have succeeded.

Proper Characters and Vowel System of the Old Japanese

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1208
1 The conjecture that there were proper characters, so-called Sindai or Kamiyomo(n)ji (prehistoric scripts) before the transportation from China through Korea to Japan in the sixth century has been transmitted by the Japanese scholars in the fourteenth and nineteenth centuries. Franz Philipp von Sieboldt, German physician noticed in his essay in 1840 the fragments of signs like the Occidental rune script and Oriental hieroglyphs carved everywhere in caves quoting the passages of an unknown book by also anonymous theologist Yukitomo Araki: "There must have been proper characters in legendary era in Japan". Recently several books abundant in examples were issued in various apocryphal "Uetsufumi", "Hotsumatsutae", "Mikasafumi" and so on against canonical "Kojiki" and "Nihonsyoki".

These characters, syllabic and sometimes alphabetic phonograms, of which documents were copied, to our regret, in much later eras than Nara as Kojiki itself, totally consist of five vowel system as nowadays. In some localities, however, in San-in district six vowels with ordinary "i" and other different one as phonemes are used. The above five vowel system relates to the following affair.

2 In the eighteenth century Norinaga Motoori remarked in his arduous life work "Kojikiden" that twenty syllabaries "ki, gi, fi, mi, e, ke, ge, fe, be, me, ko, go, so, zo, to, do, no, mo, yo, ro" among about one thousand of Chinese origin "Man-yôgana" have respectively two sorts and are strictly distinguished to meaning in many various characters. One of his disciples Tatsumaro Isizuka left a manuscript of all these syllabaries systematically arranged, which was handed down to Hasimoto, graduate of linguistics and afterwards professor of Japanese language of Tokyo University, and his two essays on periodicals in 1917 and 1931 gave vivid colour with cubic effect to the society of Japanese language monotonous till then.

The theory of Hasimoto was developed to the ambitious hypothesis of vocalic harmony indispensable to the Altaic languages and wanting in the Japanese by Ikegami, graduate of Japanese language of Kyoto Univ. and Arisaka, graduate of linguistics of Tokyo Univ., whose essays appeared in 1932 and 1934. Thus, for instance, the homonym "kami" meaning "top" and "god" must be hereafter discriminated one from another no matter how the original etymology is.

On the one hand, Yasuda, graduate of the Kokugakuin Univ. enthusiastic in Japanese language from boyhood published in 1928 the compact Japanese

grammar acknowledged by himself as the most popular abroad, in which he showed independantly the sounds and flexion of the old Japanese before the Nara era, and further played the role to cut the Gordian knot, i.e. explanation of nature of each sound founded on the antique phonology of Chinese language "Inkyô", which was kept at a distance by Japanese scholars in general way for fear of labyrinth.

3 Some opponents are inevitable in this field too. Y. Yamada, self-taught and authoritative of Japanese language revealed his sceptic opinion early in 1937, and Yosizawa, graduate of Tokyo Univ. and prof. of Japanese language of Kyoto Univ. asserted positively the opposite view in 1968. Madake, graduate of Chinese language of Tokyo Educational Univ. and prof. of Fukuoka Ed. Univ. stated in his essay of 1960 and voluminous book of 1969 that the eight vowels of the old Chinese would not always have reflected on the Japanese syllabaries of Chinese origin at that time.

The charisma during half a century began to be shaken at last. Two essayists showed up. K. Matsumoto, prof. of linguistics of Kanazawa Univ. claimed in 1974 why the vowels themselves without precedent consonants were only five instead of eight, and Morisige, prof. of Japanese language of Nara Women Univ. doubted the usage of only one syllabary "ko" in the Heian era, which must have remained with another anomalous in the earlier Nara era as survival. Both revolutionary essays were introduced arranged five times on one of the central authentic newspapers in 1975-6 with the interposed protest by S. Ono, graduate of Japanese language of Tokyo Univ. and now prof. of the Gakusyûin Univ. how so many complicated Man-yô syllabaries were understood perfectly without cognition of quality of sounds by ear and that the number of sounds changes sometimes in the long history of languages. Besides Tai in Sîkoku island issued in 1979 an etymological dictionary of creative content, where the author recognizes the syllabaries in question as "words without sounds" and regards the abnormal vowels as impossible to pronounce in reality.

Three fanatic believers of archaic Japanese scripts, Agô in 1976, Saji in 1979 and Y. Matsumoto in 1980 edited respectively their books, of which the latter two praise the above insisters on five vowels as grateful approvers giving negation to the virtual image of eight vowels. Thus the name of place "Yamato" in Kyûshû and Kansai becomes quite the same because both letters for "to" are to be read equally and not at all decisive of the relational districts.

As for me, now, I would like to conclude the problem with the Egg of Columbus. In Japan at that time the population was perhaps five or six millions and the inhabitants of central region around Yamato in Kansai consisted of many foreigners from Korea and China whose sound systems were different from aboriginal, therefore, an abnormal language occurred like Norman-French or Yiddish. Those who understood a lot of complicated syllabaries of Chinese characters were no more than one hundred people, literary and religious, of high rank who were almost men. As many people often do nowadays too, they put up with the interpretation of words and phrases with silent reading instead

of reading aloud, as, e.g. "son" and "sun" or "brake" and "break" are understood by the spelling of letters. Thus grew the compromising style "Kambun", the Japanized recitation "Rôei" and calligraphy "Syodô". The average change of speech is like stature of nation. The multiple orthography is unstable at first, but fixed gradually for the general communication through hands and for eyes, the pronunciation being in this case secondary. The unique example, by the way, "kekere" with "Hiragana", Japanised cursive syllabaries in the anthology "Kokinsyû" in the Heian era should not be the regular rudiment of "kokôrô" in the Nara era corresponding to the modern standard "kokoro" (heart, mind, spirit), but exceptional and obsolete.

4 The five vowel theory extends to the discussion of the genealogy of the Japanese language. The illiterate Ainu language with the same word order and similar vowel system with the Japanese was clearly negated, however, owing to the incorporative and holophrastic structure by K. Kindaiti, graduate of linguistics and prof. of Tokyo and the Kokugakuin Univ. Nevertheless Leo Sternberg, ethnologist of USSR regarded the Ainu language as one of the so-called Oceanic languages corresponding to the Austronesian consisting of Melanesian, Polynesian and Indonesian branches by W. Schmidt, Austrian ethnologist and linguist. S. Hattori, graduate and prof. of linguistics of Tokyo Univ. decided by the lexico-statistic measurement that the Ainu language makes an intimate group with the Japanese and also with the Korean despite of the sound system with many vowels. Also Yasumoto, graduate of Kyoto Univ. proved the same result by the "shift" method combined with the computer. Umehara, graduate of Kyoto Univ. and now critic of the old civilization of Japan is in this vogue.

5 Finally I must here announce distinctly that the idea of the three vowel system of the Loochooan which is unquestionable dialect than sister language of the Japanese was completely confuted with the linguistic judgement "palatal law" by E. Polivanov, linguist of USSR in 1914 and Iha, graduate of linguistics of Tokyo Univ. in 1930. The Loochooan sound system of the mainland originally consisted of five vowels as many other present dialects of the adjacent small islands. I emphasize that we need not discuss here now the vowel system of human language, which is quite another matter.

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The Acquisition of Semantic Knowledge: The Connectives

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1209
1. During the last several years I have been involved in a series of studies on children's acquisition of lexical knowledge of *connectives*, used to join a subordinate clause to a main clause or to another subordinate clause. Function words such as these conjunctions have a typical relational character. Their meaning is difficult to define even for the literate adult, and they offer particular great difficulties for children and for aphasic patients. Knowledge of these words' meaning is part of the lexical-semantic competence of a speaker, and it is an important question to ask how it has been acquired. This knowledge is rather complex. It includes a notion about the phonological representation of the individual words, of the syntactic class to which they belong, of the syntactic constraints which restrict their use in a sentence, several kinds of information about the relation to other words (to which concepts they apply, with which words they are synonym) and different pragmatic notions (in which context a word can be used, when it is appropriate, etc.). This lexical competence also includes the knowledge that a word belongs to a certain semantic domain. Part of this complex knowledge is of a metalinguistic character, concerning notions such as the appropriateness of the use in a given context, or the alternative of selecting another more appropriate synonym to fit a particular context.

The studies I have carried out have tried to explore the development of this lexical-semantic competence about words as individual entries in the mental lexicon. The main questions asked can be reduced to the followings: How does acquisition of the meaning of the connectives take place? How are different connectives differentiated? Which are the semantic distinctions which the child learns to make first, and which ones come later? What kind of notions does the child have about the function and the meaning of the connectives when they are presented in isolation? What kind of metalinguistic knowledge does the child have about these words, and how is the course of acquisition of this knowledge compared, for example, with other word classes? To answer these questions, a series of studies has been performed with a variety of tasks. These included, for example, the following types of experiments: judgments of equivalence in meaning of connectives in Italian and Dutch children; preference for a sentence containing a connective as a description of a picture or a paragraph of a sentence; choice of a connective among appropriate and inappropriate ones, as the right word to join two clauses as description of a picture.

or out of two inappropriate ones; judgments of similarity of meaning of connectives by sorting, etc. The results of these studies, rather consistent with each other, gave a nice picture of the process of acquisition of the meaning of the connectives. This picture includes some evidence on the progressive differentiation of meaning of the connectives, and on the strategies the child uses in dealing with these words in different linguistic tasks.

The experiments designed to explore the way in which the different connectives become differentiated in meaning have given the following result. *Causal* and *temporal* connectives seem the first to be differentiated, *final* and *consecutive* come last.

The results of another study have shown that when the child does not know the meaning of a connective and is requested to perform in a task in which the "unknown" words have to be used, he does not operate at random, but relies on several alternative strategies. At a first stage, for example, he chooses for a given sentence the words he knows better, or those which have some physical similarity with a known word. At a later stage, he may select a connective on the basis of partial overlap in meaning with the appropriate one, this strategy suggesting a gradual differentiation of the specific meaning of each connective.

Two studies have shown that at an age at which children are capable of grouping nouns and adjectives on the basis of their meaning, they group connectives only on the basis of physical similarity, showing that these words, which are correctly used in a context at that age, still lack a status of independent lexical items with a specific meaning.

2. Another problem I have studied concerns the development of a more meta-linguistic kind of lexical knowledge, namely the notion that a connective is a word of the language, and that it belongs to a given semantic domain, to which other words belong. This problem has been studied with lexical and semantic decision tasks, and the experiments are briefly reported here.

The questions asked in this study can be reduced to the followings: when does the child "make" a connective a word? How does this awareness about the structure of a semantic class develop?

The first two experiments reported here consisted of a lexical decision study with connectives, nouns and adjectives as target words. The third and the fourth involved semantic category decisions, namely the task to decide whether a word belongs to a given lexical domain or not.

3. Experiments 1 and 2—Lexical decision with connectives

This study consisted of two experiments with different subjects but with the same material. The stimulus words were 48 strings of letters as follows: 8 nouns, 8 adjectives and 8 connectives, 16 "legal" non-words (possible but non existing words in the Dutch language) and 8 illegal non-words. The 8 connectives were chosen among the most frequent 20 connectives in Dutch. The

nouns and adjectives were matched to the connectives in frequency, length and contour. In the first experiment the words were printed on cards and the subjects, children of 2 grade (20 Ss), 4 grade (19 Ss) and 6 grade (20 Ss) had to tell the experimenter whether the string of letters was a word or not. The dependent variable was the number of errors. In the second experiment the same strings were given tachistoscopically on a visual display to the subjects (different from the first experiment): 19 children of 2 grade, 23 of 4 grade, and 20 adults. The task was to decide, as quickly as possible, whether the string of letters was a word of Dutch or not. The dependent variables were the number of errors and the latencies.

Both experiments gave the same results. Younger children make more errors and their latencies are much longer. Connectives elicit significantly more errors and longer latencies than adjectives and nouns: in fact, the error data show that they are treated by many of the 2 and 4 grade exactly as legal non-words. In both experiments children had almost no problems with nouns and adjectives. For relational words as connectives, then, the kind of knowledge which is necessary to perform in a lexical decision task seems to develop much more slowly than for content words.

4. Experiment 3—Semantic category decision task

In this experiment the subjects, 20 children in each group of 2, 4 and 6 graders, and 20 adults, were tachistoscopically presented with 60 words: 25 words of *time*, and 35 from other lexical semantic domains. The 25 words of time were nouns, adjectives, adverbs and connectives. Of the 35 remaining words, 25 were matched approximately to the experimental words in class, frequency, length and contour. The experimental words of the different categories were crossmatched in frequency.

The dependent variables were the proportion of errors and the latencies. Both sets of data showed the same trend. The results can be summarized as follows. First, errors and decision latencies decrease regularly from the 2 grade to the 6 grade and further to adulthood. Nouns are at all ages the easiest, followed by adverbs, and the connectives by far the most difficult. As in Experiments 1 and 2, the connectives are again the most difficult words: here, the temporal connectives require the longest semantic decision time. An interesting result was the relative ease of adverbs as compared to connectives, which can be interpreted by considering roughly that connectives are relational words, while adverbs are semantically "richer", with an own meaning.

5. Experiment 4—Semantic decisions on words of time and space

It has been argued by several linguists and psychologists that spatial and temporal terms present a considerable overlap. The expressions of time would be based on a spatial metaphor. There is some developmental evidence in favor of this notion, such as confusion errors in the use of temporal and spatial words. Time expressions are often at first misinterpreted as spatial expressions.

Spatial expressions also seem to be used by children before temporal terms. This evidence, is, however, only episodic. In this experiment I have tried to test the notion that temporal function words might develop out of spatial terms. The children, again 2, 4 and 6 graders (15, 13 and 16 Ss respectively) were given words printed onto cards with the request to decide whether they were words of *space*, of *time*, or neither. The 46 words were nouns, adjectives, adverbs, connectives and prepositions, approximately 40% temporal words, 40% spatial terms, and 20% miscellaneous. At all age levels the children made few errors for content words and many for function words. Temporal terms were significantly more often assigned to the category of spatial words than viceversa. The results are only preliminary, but they support the notion of expressions of time as developing out of the notion of space, or from a common system which gradually develops into two distinct categories.

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Remarks on Children Bilingualism

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Referring to my research experience, I believe that, in the framework of research on bilingualism, insufficient attention has been devoted to different types of bilingualism. As a rule, the term 'bilingualism' has been employed to cover a number of different situations, all characterized by the fact that two or more languages are used by the same speaker addressing different listeners. On the other hand, bilingualism is often intended as the typical linguistic condition of coherent groups of speakers who, for historical or political reasons, share the same land with the speakers of a different group, and consequently learn this second group's language in addition to their own. In group bilingualism—for political, 'economic or cultural reasons—one of the two languages is usually prevailing. The speakers of the other language belonging to the group are said to constitute a 'minority' (linguistic minority). As a rule the members of the 'minority' are the ones who support most of the weight of bilingualism, because only they will be forced to learn both languages, thus becoming bilingual. In general, the instances of members of the majority learning the language of the minority are limited in number and explained by special circumstances¹).

So far, the current interpretation of the term 'bilingualism' and of the implications thereof in sociolinguistic perspective are in agreement. There are, however, some fundamental differences between this conception of bilingualism and bilingualism seen from a different angle. No one will deny, of course, the importance of the problems raised by bilingualism in groups or communities. One must not forget, however, that bilingualism is to be looked for (in E. Haugen's words) "in the head of the single speaker"²). In other words, bilingualism is a state of affairs strictly related with individual conditions and problems. Group bilingualism is only the result of the fact that the members of a certain group socially accept (and recognize as a mark of their peculiar condition) their being bilingual. Thus bilingualism is socially imposed, but it impinges upon the individual's behaviour. Bilingual speakers are bilingual only if they accept to be bilingual, that is if they accept (consciously or not) the obligation to learn a second language, pressed upon them by the circum-

1) Cf. the problems hinted at in G. Francescato, *Analisi di una collettività bilingue: le condizioni del bilinguismo in Alto Adige*, in 'Quaderni per la promozione del bilinguismo', 7/8, 1975, pp. 1-37.

2) E. Haugen, *The bilingual individual*, in *Bilingualism in the Americas*, Univ. of Alabama Public., n. 26, 1956, p. 69.

stances they live in.

This opens the possibility—so far insufficiently investigated—of pointing at bilingual speakers who do not belong to any “group” of bilingual speakers. This is what I try to convey by use of the term ‘isolated bilingual’. There are a number of features, which can be used to set up a definition of the ‘isolated bilingual’:

- 1) we can distinguish two main categories of ‘isolated bilingual’:
 - a) those speakers who have acquired the knowledge of two different languages because they have been born in so-called ‘mixed’ families, that is in families where the parents themselves speak different mother-tongues;
 - b) those speakers whose families have moved to a country where a different language is spoken (e.g. migrant families, etc.)³⁾
- 2) in both cases these speakers, having been exposed since their early infancy to two different languages, meet the conditions for becoming bilingual: their bilingualism, however, is dependent upon the fact that the community they live in is *not* a bilingual community⁴⁾
- 3) they are ‘isolated’ because their bilingualism is not the result of a situation of group-bilingualism;
- 4) their bilingualism is to be viewed against the situation obtaining in the families, and it can not be dissociated from that situation;
- 5) bilingualism for them is a ‘dynamic’ situation, that is a situation which will eventually change, if and when the family situation changes⁵⁾.

To support this list of features, marking the condition of the ‘isolated bilingual’, I refer to a study of over 100 instances of children, whose biographies are in agreement with the features stated above; this study has been published in form of a book⁶⁾. Of course, this is not the first work to summarize tentatively the experiences of bilingual children as expressed in their biographies. It is the first study, however, to take care of two main features: the strict relationship between the individual’s experience and the situation in the families and the dynamic aspect of bilingualism. Within the frame of the two main categories stated above (mixed families—migrant families) there are in fact almost as many different situations as there are biographies. This is especially true when we consider these ‘bilingual’ speakers at different moments of their lives. My research contrived to handle only instances of bilingualism acquired

3) There are a number of families where a third language is spoken, either as a common language by parents of different mother tongue, or as a consequence of repeated changes of residence.

4) This point underlines the difference between the children raised in similar situations, and the children raised in a community sharing their bilingualism.

5) The changes reflected in the children’s bilingualism are often extra-linguistic changes (e.g. changes of residence) and depend heavily upon the local linguistic pressure.

6) Cf. G. Francescato, *Il bilingue isolato*, Minerva Italica: Bergamo, 1981.

in the first years of life, at any rate not later than the tenth year of age⁷). My subjects, however, having been bilingual since their childhood, may (or may not) have maintained such condition of bilingualism in their adult age. From my study it appears with evidence that there are cases in which bilingualism is lost in later age, as there are cases in which bilingualism is maintained, and often becomes a dominant feature in the life of the subjects.

Loss or maintenance of bilingualism depends thus upon such different factors as the return of the family to the country where its original (first) language is spoken, or else the building of a "carrière" (scientific, of employment, etc.) based on or favoured by the knowledge of two languages⁸). Bilingualism, thus, has to be considered as a 'status' which can be acquired and bettered as well as lost, changing often with the change of the circumstances. There are times of 'development' of bilingualism, as well as times of 'latency' of one of the languages, much in agreement with the different conditions of the individual life and situation. Bilingualism appears to be a dynamic process, with not infrequent and relevant changes of 'domination', transferring the prevailing role from one language to the other.

If in group bilingualism the constant pressure of the group (and of its official, as well as its cultural rules) is principally responsible for the maintenance of a reasonable degree of bilingualism in the entire community, in the situation of the 'isolated' bilingual many other factors are present, to determine different types of linguistic behaviour by the speakers faced with the requirements of a changing situation for bilingualism. As we have seen, one of the preminent factors, which have their impact upon the conditions for spontaneous bilingualism, is the family of the bilingual speaker. By 'family' I mean the complex and total net of relations which operate on the linguistic behaviour of the members of the family itself. Apparently, the choice of the language to be spoken by father or mother to a newborn baby is in first instance 'free', in other words it admits the use of whichever language is their respective mother-tongue. Shortly after the birth of the first child (and even more after the birth of other children) it may however appear that the maintenance of one of the languages spoken to the child becomes difficult, sometimes superfluous, often frankly inconvenient. It does not need to be underlined that other members of the family, outside the parents, will exercise upon the children a strong influence, especially when they represent the model of the speakers in the ambiance. Such an influence may favour (or, in other cases, hinder) the use of one of the two languages. Still a stronger influence must be attributed to the language of the school.

Thus different 'dominance configurations' will characterize the description of the linguistic behaviour not only of the single bilingual speakers, but of

7) That is, before the age of adolescence, usually considered the limit age after which spontaneous bilingualism is no more possible.

8) Many bilingual children become later interpreters or translators, or teachers of their second language.

their families as a whole. To give only one example, bilingualism forced upon a child of a migrant family by the school, may later make way to 'monolingualism', if the 'second' language becomes for him the dominant language; not only, but his behaviour, accompanied and supported by the pressure of the ambience, may finally result in monolingualism (adoption of the 'second' language by the whole family). In my research I have met a number of cases of this type, but also, to be sure, as many contrary cases, that is instances of families where the original language of the family still has succeeded to remain the dominant one within the family, but with various degrees of diglossia for those members of the family who are obliged to use the local language (for them, a 'second' language). Not infrequently, this results in a considerable impairment for the use of either one of the languages, or even of both. In fact, even a quick inspection of the biographies I collected shows that the theoretical relationship between bilingualism and diglossia needs to be revisited. The theoretical frame suggested by Fishman⁹⁾ is certainly insufficient to adequately taking account of the situations, as they appear in my analyses of the actual biographies of bilingual children.

When the results of my research on the 'isolated' bilingual children are compared with the results of similar researches conducted with bilingual children belonging to a minority group (in my experience, in particular, children of monolingual or mixed families of the slovenian speaking minority in Trieste)¹⁰⁾, it appears that between the bilingual children belonging to a group, and the bilingual 'isolated' children the psychological differences are not so significant, but the sociolinguistic differences are very relevant. Of course, the linguistic problems to be considered in each of these two groups depend heavily upon the social situation: but for the minority bilinguals the linguistic behaviour is strongly dominated by the schemes provided by the group and commonly shared by all speakers. For the isolated bilinguals, on the contrary, the linguistic behaviour is a consequence of many changing factors, which are strictly individual in nature, and which continuously put the subject in front of overt or unconscious linguistic choices.

To conclude: further study of these problems and a closer examination of the social and linguistic factors and processes seems to me of considerable importance for a better comprehension of the problems of bilingualism, a linguistic situation which is receiving a growing share of interest in the modern world.

9) Cf. J. Fishman, *Bilingualism with and without Diglossia; Diglossia with and without Bilingualism*, in 'Problems of Bilingualism', *The Journal of Social Issues*, XXIII n. 2, 1967, pp. 29-38.

10) Cf. as a first introduction G. Francescato, M. Ivsič, *La comunità slovena in Italia: aspetti di una situazione bilingue*, in 'Quaderni per la promozione del bilinguismo', 21/22, 1978, pp. 1-38.

Learning the Standard Variety of a Mother Tongue in School*

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Results of research in linguistics, sociolinguistics and psycholinguistics can offer some facts and ideas about language and language use that can be useful to answer important problems facing first language teachers such as: which geographical variety is to be taught? can we accept any language register in the classroom? is it possible to "rectify" children's speech and how can it be done? This paper intends to answer the basic question of which "language" should be taught in primary and secondary schools.

This question arises because of the existence of variations in language and of the socio-cultural preference for one of the different varieties. In French, for example, there are first differences between written and spoken language. There seem to be few normative problems in the learning of written French, precisely coded in accepted grammars and dictionaries. In oral French, as in many Western languages, the situation is different. There are many different linguistic forms conveying the same meaning, specially at the lexical and phonological levels. These variations have been related to (1) geographical variables (these are usually called dialects), (2) social variables (economical and cultural classes, ethnic groups, etc.) influencing the proportion of utilisation of the variants along a continuum instead of in a dichotomised way and (3) for the same speaker, situational variables such as topic, context, physical situation, type of listener, etc. Rather grossly and arbitrarily, "registers" have been identified and oral variants grouped in "popular", casual and formal speech. So in oral language, many anthropological norms, so to speak, exist, the speaker using the variants known and socially accepted in such and such a situation.

As revealed by researches on attitudes and variations, there is also a constructed and often idealised norm, that is a set of variants socioculturally considered as better than the others, particularly in formal situations of communication. These variants are generally closer to the written language and are used more often by privileged socio-cultural classes.

In spite of the variations, intercomprehension exists more or less largely and is probably based on a large amount of linguistic elements common to many speakers, on some kind of a basic nucleus, on the richness of the passive linguistic repertory of the speakers and on the use of context.

* An expanded French version of this text has appeared in *La norme linguistique*, Québec, Conseil de la langue française et Paris, les Editions Robert, 1983, pp. 463-509.

Language is a tool, a privileged means to realise certain function of an individual and communal nature. This functional perspective relativises the linguistic code, which is then properly considered in another way than an object of description and a goal in itself. A speech act then which does not utilise the standard variety will be nevertheless valued if it realizes the functions it is supposed to achieve.

It is agreed today that the natural development of language in children depends largely on the functional utilisation of speech and on the verbal interactions with the environment. Children's language development is such that at the age of five, they have acquired most of the oral language of their environment and seem to be able, on a limited basis, to make linguistic choices. There are differences among children in their development of linguistic and functional aspects, and the main variables related would seem more to be age and intelligence than for example sex or social class when linguistic performances are compared between themselves rather than to the standard language. It appears also that oral language development goes on after the age of five and that therefore school has a role to play in such development.

Verbal interactions are also important for school learning. Language is not only an academic subject but also the most important medium of teaching and learning and the principal means of inter-personal relations between children and teachers, a crucial element of success or failure for elementary school children.

Because of the importance of authentic verbal interactions in class, the reality and necessity of intercomprehension of coexisting linguistic variants, and the important functions assumed by dialects and informal registers, in short because of the legitimacy and inevitability of linguistic variation, school must accept and also utilise the casual and non-standard speech of children. That acceptance need not prevent school from proposing written and oral linguistic development objectives.

The rejection of the child's spontaneous language by the normative, code-oriented, traditional pedagogy, is thought to have negative consequences. Such a pedagogy has a tendency (1) to put the focus on a mythical standard oral language often to the detriment of the development of meaning and functions, (2) to neglect the development of oral language, (3) to augment the difficulties in the task of learning to read, (4) to underestimate the children's linguistic competence, (5) to discriminate against children of lower socioeconomic backgrounds, (6) to discourage authentic verbal interactions in class, (7) to create, when it succeeds, some kind of social uprooting or to contribute, particularly in teenagers, to the possible rejection of school and of its values.

Another pedagogical orientation, the communication-oriented pedagogy, takes into account the sociolinguistic and functional perspectives outlined above and is centered more on the production of meaningful discourse than on the code itself which is generally accepted in its many variations. However this orientation is not easily put into practice and has a tendency to encourage im-

provision in teaching, to limit language activities to current or self-expressive communication, to ignore linguistic objectives at the oral level and to neglect or integrate with difficulty the formal aspects of code learning.

Since communication is constituted for an important part of linguistic elements and since linguistic competence is part of communicative competence, it is felt that both aspects should be present in learning a mother tongue in school. Considering the linguistic code as a means instead of an object or a goal does not prevent the necessity for the school of determining which linguistic variety and register will be privileged as a learning objective. Otherwise, school will not fulfil its obligations towards both society and individuals and will contribute more to the accentuation of social inequalities.

In the perspective of integrating the formal, situational and functional aspects of language, the goals of school should be to develop the linguistic competence of children in order to assure as best as possible the communal functions of language (develop and protect the national language; transmit values, knowledge and literacy; assume the institutionalised communications; etc.) as well as the personal functions (thinking, imagining, expressing, etc.). At the linguistic level, the general objective should then be to enlarge the linguistic repertoire of each child so that he will be able to use the linguistic elements of the code, dialect and register appropriate to his intentions and to the communicative situations he will be in. School should also aim to give a positive attitude towards linguistic variation. What then could be the language content of teaching?

As far as receptive competence is concerned, the objectives should be to develop listening and reading skills. Such development is possible not only by learning new linguistic elements, more formal ones, but also by using the linguistic variety the child already knows. The assumption here is that comprehension skills are cognitively the same whatever variety is used and therefore the improvement gained in listening to casual speech familiar to the child could be transferred to more formal speech and to reading. The acceptance of the children's language also contributes to the linguistic richness of the classroom and to the possible acceptance and study by the children of linguistic variations. Concerning reading, the study of dialect literature should be encouraged. At the elementary level, it has not been adequately demonstrated that the use of dialect and non-formal elements in beginning reading helps children, even slow learners.

The question of the choice of linguistic elements to be learned is more complex regarding the production of speech. Three principles of choice can be useful. The economy principle suggests that one variety (dialect and/or register) be taught at a time. The utility principle implies that the most frequent and the most socially accepted variety should be learned. The productivity principle indicates that structural elements are more important than lexical and phonetic ones.

Applied to the acquisition of the written language, these principles mean

that what is called "le français écrit correct" is taught at the primary and secondary levels. It excludes non-formal or dialectal as well as literary French, but accepts that the day to day dialect lexical items should be learned in their written form. In oral speech, the general objectives should not be to replace one variety by another, but to enlarge the active repertoire of the children and to promote the voluntary and spontaneous use of linguistic elements appropriate to intent, topic and situation. But there is a need to be more precise about these elements and we suggest therefore some criteria which may help the teacher to choose which oral elements are to be actively mastered by the children.

The first criterion would be to "teach" linguistic elements that certain children have not yet mastered because they still use some baby talk, such as /kɔʃɔb/ for /ʃɔkɔb/. Secondly, there should be an active mastery of what is called socially unmarked variants. For example, in French Quebec, the affrication of /t/ and /d/ would be considered only as an indicator of register but a change of vowels such as [pe:ʀ] instead of [pɛ:ʀ] is socially marked. The second criterion says this: teach the standard variants corresponding to such markers rather than the variants corresponding to indicators. The last two criteria are: teach variants that are already relatively frequently used in the community and the ones that are accepted as correct by that community, which in Quebec, for example, would be some kind of a Quebec standard French.

In conclusion, may we say that the problem of learning or teaching the standard variety of a mother tongue in school has been seen in this case in the perspective of the teaching of French in Quebec. It may appear that the basic problem is rather similar in other communities and that the principles and criteria suggested could be applied in these communities.

The Developmental Theory of Language Learning: Evidence from What Learners Fail to Say

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The first thing I should point out is that the title of the paper is as given in the volume of abstracts not as given in the programme. Those of you who came along hoping to find out what the developmental theory of language teaching was are going to be disappointed.

This paper had its origins in a project commissioned by the University of Malaya in Kuala Lumpur, who were dismayed by reports coming in from employers that University of Malaya graduates could not cope with the English they needed in their jobs. This discrepancy between the standard of English required in industry, commerce and administration and the standard of English needed to get through the school and university system was in fact completely predictable as the entire educational system had just switched over to being exclusively Malay-medium. The question of what we proposed to do about narrowing the gap is not the concern of this paper¹⁾, but the obvious starting point was to do some sort of analysis both of students' language and of the target language—that is, the language used in the various jobs where English was needed. We were very fortunate in being allowed to observe and sometimes record data such as departmental meetings, production meetings, job interviews, even credit interviews, although there was always the proviso that the actual content remain confidential. This puts me in the happy but dangerous position of being able to say anything I like without you being able to check the date.

The most striking initial result, using conventional error analysis techniques, was not unnaturally related to the density of errors made—the students made something like 160 identifiable errors per 1000 words, while the English-medium speakers made around 20 errors per thousand words. However, closer inspection of the errors showed that the difference between the two groups was definable only in terms of quantity of errors; the types of error made were the same in both cases.

The next step was to consult the various studies that have been made of the variety (or varieties) of English spoken in the Malay peninsula. Of the 3 or 4 studies published in book form, all were actually written in Singapore and all are somewhat impressionistic. Eventually I decided to use as a basic reference work the most recently-published study, that by Platt and Weber.²⁾ Com-

1) A report on this will undoubtedly be published in late 1983.

2) Platt, Weber 1980.

paring the list of student errors with the Platt and Weber list, I found that over 70% of supposed errors were directly and unmistakably identified as regular features of Malaysian and Singapore English- features such as copula omission, plural and third person singular marker omission, auxiliary omission, article omission, reduced tense system, incorrect word order in questions, and so on. (The very familiarity of the members of such a list to those of you who have been involved in error analysis must in itself constitute a powerful argument for a developmental, as opposed to a transfer, theory of language learning). Now, Platt and Weber divide Singapore into four linguistic levels largely on the basis of the *frequency* of occurrence of such features, and it was very tempting for us to agree with them and say that all the students needed was some sort of remedial course designed to cut down the number of errors made and there would be no further problem.

However, it seemed to me that there was something else going on here. "Correcting" the errors in the transcripts still left a stilted, halting text. I began, therefore, to look at the different kinds of cohesive device in the data.

The technique of comparing a text with a check-list of structures had already been tried in a small way by Greenall³⁾ of Newcastle University, when he found significant differences in the frequency of usage of six syntactic features (which he characterised as "devices employed in information theme structuring") between native speakers of English and fluent non-native speakers of English. An initial analysis of my data using these features produced quite spectacular results, with the English-medium speakers being roughly comparable to the fluent non-native speakers of the Newcastle survey, and the Malay-medium students using virtually *none* of the features.

Extending the check-list to include inter-sentential cohesive devices (in the strict Halliday/Hasan⁴⁾ sense) as well as intra-sentential features such as subordinators, co-ordinators, kinds of noun phrase, clefting, modality, and so on produced similarly spectacular results. An analysis of lexis used quantified the amount of repetition necessitated by the lack of access to the above syntactic devices. The percentage of different lexical words to the total number of uses of lexical words was 17% in the case of Malay-medium and 66% in the case of English-medium.

Although spectacular, the results were, I reluctantly decided, a little unfair on the students. The sort of tasks they were doing were by nature repetitive, and the lexis if not the syntax was severely limited by the constraints of the task, whereas the English-medium data was from situations where, even if the lexis was limited, the kind of contribution that participants could make was much more open. I therefore limited myself from this point on to a comparison of English-medium students (henceforth E) and Malay-medium students (Henceforth M) performing the same task, namely telling a

3) Greenall 1980.

4) Halliday/Hasan 1976.

story using a series of 7 initially unordered pictures, initially distributed to and described separately by different members of the group. Obviously, if the technique yielded similar results in this extremely restricted test, then it could safely be assumed to be a valuable device for characterising language performance in a whole range of different situations and for different purposes.

I also set myself the task of producing a standards check-list of items which could be used by other researchers in the field. For basic reference I used, fairly uncontroversially I hope, Quirk and Greenbaum's "University Grammar of English" and Halliday and Hasan's "Cohesion in English". The various categories have over a period of time been refined, modified and sometimes abandoned, and the result I admit is a rather strange-looking list of features which are obviously not all at the same linguistic level (or even from the same linguistic theory) and in which there is an inevitable amount of overlap.

The list and results are given in the accompanying table. A few comments may be made about this. Results are given to the nearest whole number, which hides some relatively insignificant differences at the one level. Items 1—9 are mostly intra-sentence, items 10—14 mostly inter-sentence. For item 3, M has a question-mark after "10" because it is difficult to determine whether the expression "I think" is parenthetical or genuinely introducing indirect speech in most of the examples. The large number of personal pronoun occurrences under item 11 disguises the fact that *all* uses for M involved "he", whereas E used he, it, they and the object forms. (There was no occasion to use "she" in this particular task.) The large number of uses of "and" between sentences and without ellipsis reflects what is subjectively an almost dominant feature of M speech; a not untypical example is: "and he's very clever and he close the hole and he change the sign and he say no gold has been stolen and he going back." Item 16 needs a word of explanation. While it is obviously simplistic to use word-length as a measure of linguistic complexity, the contrast in the earlier comparisons had been startling, and there does remain a difference even in this very controlled activity. It might also be remarked that another difference obvious to anyone who listens to the tapes lies in the speed of delivery. Ignoring lengthy pauses within turns as well as pauses between turns (in other words, measuring only the length of time taken to say the words), M averaged between 60 and 80 words a minute, while E averaged from 140 to 180.

It is immediately clear from the table that there is a significant and consistent difference between the two groups. If it can subsequently be shown that improving linguistic performance is not so much a matter of correcting existing errors but rather of expanding the range of linguistic devices available to the learner, (in other words, actually providing the opportunity for more errors) then we will indeed have gone some way towards showing that you can't learn without goofing".⁵⁾ The problem with much of the work done on second language learning is that some of the evidence offered is very flimsy

5) Dulay/Burt 1974.

	Features	Uses per 1000 words	
		M	E
1.	Relative clauses	—	15
	of which, reduced relatives	—	7
2.	NPs with participle	—	3
	with infinitive	—	2
	with wh-	4	6
	with noun premodifier	—	3
3.	Indirect speech	10(?)	4
4.	Subordinate clauses of time	—	—
	place	—	2
	condition	—	—
	concession	—	—
	cause	1	1
	effect	—	1
5.	Comparatives (-er, so, as, too, enough)	—	4
6.	Cleft sentences	—	4
7.	Passives	—	4
8.	Modals	2	10
	of which, modals+perfect	—	2
9.	Compound verbs	—	19
10.	Anaphoric reference	—	—
	it, this, that	4	14
	such, so, as	—	1
	here, there	—	—
11.	Substitution	—	—
	he, she, it, they	47	51
	one, ones, same	6	8
	do, did	—	—
	so, not	—	—
12.	Co-ordinators	—	—
	and (inter-sentence)	—	—
	without ellipsis	42	35
	with ellipsis	4	13
	and (intra-sentence)	1	5
	but	2	5
	or	1	1
	other	4	11
13.	Lexical cohesion—general or	—	—
	summarising nouns as anaphoric device	—	12
14.	Hesitation-markers (well, anyway, sort of, you know, etc.)	4	22
15.	(a) Lexical words	357	300
	(b) Different lexical words	62	99
	(c) b/a %	17%	33%
16.	Words (minus inflection) of 3 syllables	4	5
	4 syllables	—	5
	5 syllables	—	—

indeed, Abbott⁶⁾ criticises a lot of error analysis work on the grounds of lack of rigour, and illustrates a way of making analysis more systematic. What I suggest here is that the check-list approach enables one to make a far more complete—and potentially more useful—profile of the learner's performance than conventional analysis could.

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Bilingual Children's Acquisition of Five English Derivational Processes*

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1. Introduction

In spite of the increased attention that the study of inflectional operations has received in the recent linguistic literature (Ferguson and Slobin 1973, Christie 1977, and Pacesova 1977), there has been a paucity of systematic investigation aimed at discovering the productivity of English derivational processes in both formal as well as psychological terms, except in the original work of Berko (cf. Derwing 1976). The present study represents continued research into this much desired area in the vein of Derwing (1976) and Marckworth (1978) and in the sense of Christie (1977). The five derivational processes investigated are: (1) one type of noun compounding (CPD) as in *doghouse*, (2) agentive suffix (AGENT) *-er* as in *singer*, (3) instrument suffix (INST) *-er* as in *pointer*, (4) adjective forming suffix (ADJ) *-y* as in *muddy*, and (5) adverb forming suffix (ADV) *-ly* as in *slowly*.

This study was conceived as an experimental investigation of emerging morphology, addressed to the question whether these five English derivational processes are psychologically productive in the bilingual child and if so, whether his acquisition is equivalent in pattern, rate, and content to that of his monolingual counterpart reported in Derwing and Marckworth and, if there is any discrepancy, on which linguistic grounds can it be accounted for.

2. Method

The Derwing test (1976) was modified and adopted in the current study. Using the Berko technique (1958), Derwing devised a derivational morphology test in which nonsense stems are created, illustrated, and framed in a dialogue aimed at inducing the subject to produce forms of the nonsense stems if the morphological processes are internalized. In the present study, the five morphological processes are tested with one real English stem and two nonsense forms: *doghouse*, *zabehouse*, *oogcatcher* (CPD); *singer*, *doshier*, *yurser* (AGENT); *pointer*, *neaver*, *cumer* (INST); *muddy*, *teeby*, *glurky* (ADJ); and *slowly*, *blighly*, *heesply* (ADV). The subjects were twenty-four 6-12 year old Chinese-English bilinguals who are from Mandarin/Taiwanese-speaking families of upper-

* The authors wish to express their deep appreciation to Prof. S. R. Munro for his many valuable comments and suggestions.

middle class socio-economic status and together with their foreign-born Chinese parents have lived in Edmonton, Alberta, for at least one year. The items were randomized and presented to the subject individually. Responses were scored as correct (+1) or error and null (0).

3. Results

Data are analyzed separately for nonsense and real stems and for two age groups: 6-7 year olds and 8-12 year olds. Table 1 summarizes the results: *productive* indicates that at least 50% of subjects produced the form on a nonsense stem; *onset* means one subject did so. Note that the younger Chinese-English bilingual children exhibit fairly limited productive control of the derivational processes and score lower than their comparable age monolinguals, whereas the older ones have acquired virtually all but one process in their morphological repertoire and outperform their monolingual counterparts. For real stem forms, the younger children showed equivalent control of three processes (*CPD*, *AGENT*, and *INST*), while the older ones have mastered all five processes and again outperformed their monolingual counterparts.

Table 1 Productivity of Nonsense Stems

	English Age 6, 7	Chinese 6	Chinese 7	English 8-12	Chinese 8-12
CPD	productive	productive	productive	productive	productive
AGENT	productive	pre-onset	onset	productive	productive
INST	productive	pre-onset	pre-onset	onset	productive
ADJ	onset	pre-onset	pre-onset	productive	onset
ADV	onset	pre-onset	productive	onset	productive

4. Discussion

As noted above, the bilingual child's acquisition of these derivational processes is different in rate and pattern from his monolingual counterpart. Why is this so? It has been suggested that a child would need to be properly exposed to the primary linguistic data for at least two to three years before he could start to discern regularities of inflectional elements and to induce rules to account for his generalizations (cf. Bogoyavlenskiy 1957, Ruġe-Draviņa 1959, Cazden 1968). This suggestion appropriately explains the marked developmental lag in the younger bilingual children who had been exposed to English for less than three years. Furthermore, since all pertinent linguistic information might not be readily accessible to the bilingual children immersed in their fundamentally divergent linguistic environment, the acquisition pattern in the bilingual group is justifiably different from that of the monolingual children and the adult community (cf. Anisfeld and Tucker 1968).

One of the most striking features of the bilingual child's acquisition of derivational processes is the remarkable productive control of noun compounding exhibited even in the younger group. Pacesova (1977) observed that in language

acquisition his Czech child started with the mastery of unmarked classes such as word order rather than inflectional elements of high frequency of occurrence. Of the five derivational processes in this study, noun compounding is not only the most transparently unmarked but also the most extensively-applied construction whose surface structure is identical as well with its Chinese counterpart. This structural and pragmatic comparability plausibly facilitates the bilingual child's language acquisition.

The developmental lag in *ADJ*, which continues even into the later stages of morphological acquisition, may baffle any scrupulous mind. In fact, it was the only process to reach 100% correct in both the Berko (1958) and Derwing (1976) adult samples. This fact alone should invariably substantiate its formal as well as psychological status in English derivational morphology. One possible explanation for the delay is based on its phonetic quality. Zakharova (1958) indicates that in the initial analytic-synthetic stage of language acquisition the child's generalization is conditioned by the marked phonetic shape. In rapid speech, *ADJ* -y realizes as a high front lax [ɪ] which is comparatively more obscure auditorily (Marckworth 1978) and hence it is more difficult to perceive. This auditory unobtrusiveness is further complicated by competing derivational patterns. There are at least twenty varieties of adjective formation available to the English-speaking monolinguals, ranging from viable competing patterns in null such as (*good*) to -ish (*whitish*), and -y (*muddy*). Each derivation is incontrovertibly governed by semantic-pragmatic restrictions. Notice the following pairs: *grimy teeth/covered with grime*, **plaquy teeth/covered with plaque*, and **yellow teeth/*covered with yellow*. Unlike the monolingual child, the bilingual child faces a new mounting body of all sorts of regularities to discern as soon as he is plunged into the English-speaking milieu. Consequently, it is plausible that a more delicate analysis is delayed to the later stages of learning (cf. Zakharova 1958).

One unexpected finding in the study was the high production rate of real stem forms for the bilingual children. These derived real forms may be processed as separate lexical items with no derivational meanings internalized initially (cf. Cazden 1968). Then, it appears as though our findings agree with Miller and Ervin-Tripp (1964) who noted that "ability to apply an inflection to a nonsense word typically emerges... [soon]... after application of the inflection to real words."

5. Conclusion

Four of five English derivational processes investigated are formally as well as psychologically productive in the older bilingual children. The bilingual subject seems to be specially aided by the structural comparability in the two languages in accelerating acquisition of that particular feature; however, the subject may be saddled with limited pertinent linguistic information, particularly in his initial stages of language acquisition, compared to his monolingual counterparts. Consequently, lexically-based acquisition may precede rule-

based acquisition in bilingual children by some time.

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First Language Acquisition of Mandarin Chinese: Constraints on Free and Bound Null Anaphora*

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Anaphora has been recognized as central to the syntax and semantics of complex sentence formation not only in English but also in other languages, and has been recognized as one of the central features of UG.

We define an anaphoric relation generally as "a relation in a sentence where an anaphor is given its interpretation by reference to another constituent within the sentence."

Various contextual factors are known to influence the assignment of anaphoric relations. However, it has been discovered that specifically linguistic principles constrain the range of possible anaphoric relations. Moreover, there is a subset of anaphoric relations which appears to be uniquely determined by linguistic principles. If the interpretation of an anaphor is exclusively determined by linguistic principles, that structural relation is termed *bound anaphora*. Other cases are termed *free anaphora*.

Current work in linguistic theory is concerned with determining the full set of specifically linguistic principles which constrain the type of anaphoric relations possible in natural language. This study contributes empirical support for including one specifically linguistic principle in this set.

In Chinese, there is a wide range of structures in which phonologically null anaphora occurs. The grammars of Chinese and English both make a distinction between free and bound anaphora. In English, null anaphors are generally bound, while pronominal anaphors are generally free. In Chinese null anaphors may be either bound or free. In Chinese the syntactic configuration not the phonological content of the anaphor exclusively determines whether an anaphor is free or bound.

With both bound and free anaphora in Chinese there are grammatical structures in which the constituent which controls a null anaphor occurs before that anaphor. This is referred to as forward control. There are also structures in which the constituent which controls the anaphor occurs after it. This is referred to as backward control.

Critically, configurational properties of headedness lead to the syntactic binding of anaphors by the head. In Chinese, which is left branching, this will involve backward control. This contrasts with free anaphora found in subordinate clauses where there is nothing inherent in the syntactic configurations

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to determine one direction of control over another. The direction of control of free anaphora can be altered without requiring any structural changes in the sentence other than reversing the positions of the anaphor and the constituent which controls it.

These facts regarding anaphora in Chinese raise the question of what role specifically linguistic principles may play in the first language acquisition of anaphora in Chinese. Since anaphors in Chinese are not distinguished phonologically, but only by specifically linguistic factors in their abstract syntactic configurations, if we can show that young Chinese children distinguish bound and free anaphora, this would be evidence that they are sensitive to the specifically linguistic principles in their syntactic configurations.

In this study we hypothesize that Chinese children are sensitive to the syntactic distinction between *free* and *bound* null anaphora. We predict that if Chinese children distinguish free and bound anaphora, then they should recognize free anaphora as compatible with the discourse principle of forward directionality of control, but bound anaphora should resist this principle. However, bound anaphora should be especially sensitive to the principle of headedness, which in Chinese leads to the backward control of grammatical anaphora.

With regard to pragmatic context, it is generally acknowledged that discourses are structured sequentially, leading to a forward control of anaphora. If children are sensitive to the distinction between bound and free anaphora, then the pragmatic context established in a discourse should promote the use of forward anaphora in the free but not in the bound.

In our study 100 children in Taiwan from 3;0 to 5;6 (years; months) were tested by an elicited imitation task. We summarize here.

Results showed that there was a significant difference between children's success at imitating free and bound anaphora, with overall significantly more success on free than on bound. However, analyses showed that *both* the factors of Directionality and Pragmatic Lead interacted to distinguish free and bound anaphora types.

Free anaphora is significantly easier to imitate with forward control than with backward. For bound anaphora the opposite is true. Backward bound anaphora is significantly easier than forward. In fact, bound and free anaphora are treated equivalently in the backward control forms; it is the forward control form which principally distinguishes them.

This is consistent with our characterization of the differences between free and bound anaphora. Forward control significantly increases imitation success of the free anaphora. Forward control, however, conflicts with the grammatically determined direction of bound anaphora, and therefore significantly depresses success on this type. On the other hand, backward control is dependent on grammatical structures, not on discourse principles. Thus both bound and free are not distinguished here.

Pragmatic lead also differentially affected free and bound anaphora as hypothesized. Adding pragmatic lead significantly depresses the amount of cor-

correct imitation of backward anaphora whether free or bound. For free anaphora, +PL significantly depressed correct imitation of backward anaphora but did not affect success on forward control. These different patterns of the effects of PL on free and bound anaphora, are explained by the fact that PL establishes a discourse setting which sanctions free forward control. This is consistent with forward free anaphora but conflicts with the grammatical principles which govern bound anaphora and the backward control of anaphora in general.

Analyses of children's errors in imitation also confirmed that children were differentiating free and bound anaphora types and that these types were differentially sensitive to the factors of pragmatic context and directionality of control.

Although there were more errors on the imitation of bound types than on free, there were significantly more anaphora errors on the free type than on the bound. In particular there were significantly more permutations of anaphoric direction in the free than in the bound cases. In the bound anaphora, the pragmatic lead had only a small effect on anaphora in general and near zero effect on anaphora direction. These data clearly confirm that children were differentiating free and bound anaphora both with regard to pragmatic lead and with regard to the directionality of control.

In conclusion, our results confirm that in spite of the fact that free and bound anaphora are phonologically identical, both being null, young Chinese children differentiate them. They differentiate them in a way which is consistent with our general grammatical theory of the distinction between free and bound null anaphora. Directionality of the control of anaphora was found to have systematically different effects on children's production of the two types, reflecting in free anaphora the interaction of forward direction and discourse, and in bound anaphora an interaction between backward direction and grammatical control. Pragmatic lead was also found to have systematically different effects on the children's production of the two types, depressing both bound anaphora and backward anaphora with which bound anaphora is consistent. Therefore, children did distinguish bound and free null anaphora. The question then becomes what distinguished them for children.

The primary distinction between the two types of anaphora was syntactic: the bound case occurs in a configuration with a head and a left branching embedded clause (a relative clause); the free case simply involves a clause subordinated to another clause. In order to account for our results it must be the case that children are sensitive to these grammatical factors.

In conclusion, besides supporting our general point in this paper, we think our study contributes to 2 general issues, one in the field of linguistics and one in the field of cognitive psychology. In linguistics it is an issue whether anaphors can be characterized in terms of their phonological content, eg. in terms of a pronoun class. Our study clearly supports analyses which posit general structure-dependent principles which are independent of the phonological content of an anaphor.

In cognitive psychology, it is an issue whether children at the earliest stages of first language acquisition are guided by general cognitive principles alone or whether they bring specifically linguistic principles to bear on the language learning task. For example, it has been proposed that children may bring only discourse principles, which are consistent with forward linear order, to bear on the early acquisition of anaphora. Our data show children to be sensitive to grammatical structure which is incompatible with forward linearity as the only principle accessible to children. Only a model which attributes to children specifically linguistic principles as well as general cognitive principles will be sufficient to account for these data.

Comparative Studies on the First Language Acquisition of Japanese and English Language Universal and Language-Specific Constraints*

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In this paper we summarize several results from a series of comparative studies we have completed on first language acquisition of Japanese and English. These studies are part of a large cross-linguistic project which is studying acquisition of complex syntax in six languages. The research paradigm underlying this project studies certain critical ways in which languages vary, e.g. word order or branching direction, which may be termed "parameters" of language variation. The basic assumption of the research paradigm is that if certain acquisition commonalities and/or differences can be identified with certain of these parametric variations, then this will provide evidence that children are sensitive to these parameters. Identifying these sensitivities can provide the basis for a theory of how children begin to organize the large amount of variant language-specific data to which they are exposed, and to construct the grammar of their language.

A comparison of Japanese and English first language acquisition is especially important to this study. Japanese and English differ in both the parameters of word order and branching direction, as well as in a number of other properties. As is well known, Japanese basic word order is subject-object-verb while English is subject-verb-object. Japanese is also left-branching, in that relative clauses, subordinate clauses, and other forms of sentential complementation are generally placed before their head. English is right-branching, in that such complements are generally placed after their head. These parameters of variation between Japanese and English are profound differences which account for extensive variation across these languages.

If there are similar patterns in the acquisition of complex syntax in Japanese and English, in spite of these and other profound differences across the two languages, then these commonalities must represent deep properties of the first language acquisition process. If there are specific differences in first language acquisition of Japanese and English, we may then ask whether these differences can be explained by one or other of the parameters of variation across the language.

In this paper we identify a number of commonalities and a number of speci-

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fic differences between the first language acquisition of complex sentence formation in Japanese and English. We argue that both the commonalities and the differences reveal early sensitivity to the grammatical structure of natural language and to the specific parameters which distinguish Japanese and English.

Our data result from a series of comparative experimental studies and studies of natural speech of young children acquiring Japanese as their first language in the Tokyo-Yokohama area of Japan, and of young children acquiring English as their first language in the United States. The data are provided by about 200 children in each language from 2-5 years of age, which is the period of early syntax acquisition in each language. Most of the data in these studies involve early coordinative and/or subordinative complex sentence formation.

RESULTS

Our experimental study is based on Japanese and English speaking children's elicited imitation of simple coordinative sentences. In an elicited imitation task a child is asked to repeat a sentence "just the way" the adult says it. The assumptions underlying the task are that in order to repeat, the child first reconstructs the sentence, processing it through his own comprehension and production systems, and thereby revealing properties of these systems.

The experimental sentences varied according to two factors. First they varied according to coordination structure. Some of the sentences were full coordinations, i.e., coordination of two full propositions, each proposition containing a noun and a verb, as in, for English, "Babies laugh and babies cry," or for Japanese, "Hato mo tobu shi suzume mo tobu." The sentences also included coordinations which were reduced, and thus involved the coordination of non-propositional elements, i.e. nouns or verbs. An example of an English reduced sentence is "The teddy bear walks and sleeps;" this can be related to a sentence coordinating full propositions like, "The teddy bear walks and the teddy-bear sleeps." An example of reduced coordination for Japanese is "Otona mo kodomo mo oyogu." The experimental sentences also varied according to directionality of reduction, i.e. forward or backward. We will return to this factor later, but generally it had to do with the position of the redundant term or of the site of reduction.

In Japanese, three different tests which used sentences varying in these ways were administered. Japanese I involved the connectives *shi* and *to*. Japanese II involved the connectives *shi* and *mo*. Finally, Japanese III involved the gerund *-te* and the particle *to*. Our natural speech analysis is based on speech samples collected from the children in their homes in both languages.

We first review a basic set of 4 commonalities between acquisition of Japanese and English we have found in these data. We then review a set of 3 differences. Both the commonalities, and the differences replicate over elicited imitation and natural speech results.

COMMONALITIES

Commonality 1

In each language productive competence for complex sentence structures is acquired at a similar rate at similar ages between 2;0 and about 3½ years. Overall, both Japanese and English groups acquired the ability to successfully imitate simple coordinate sentences with a 75% success rate by 3½ years of age. Correspondingly, both young Japanese and English speaking children use a similar proportion of predicative recursive structures in their natural speech, in spite of the fact that the devices for this predicative recursion differed across languages.

Commonality 2

There was also significant similarity in the acquisition order of the general types of complex sentences acquired in Japanese and English. In both languages, full propositional coordinations were among the earliest forms of complex sentence formation acquired, in spite of the fact that in Japanese there is no connective which is exactly equivalent to English *and*. In both languages children were able to imitate full propositional coordinations as well as or better than reduced coordinations.

Commonality 3

In both languages, redundancy was frequently retained.

Commonality 4

Grammatically reduced forms were delayed in acquisition in both languages.

DIFFERENCES

We have also identified three differences in the acquisition of complex sentence formation in Japanese and English. These differences indicate some ways in which the child is acquiring the language-specific of grammatical reduction.

Difference 1

In English, most of the redundancy retained in full-formed propositional coordination corresponded to sentence initial, in particular, subject position. For example, in "*I* got two bears and *I* two and a half," the redundant pronoun *I* is retained in sentence initial subject position. In contrast, in Japanese natural speech, most of the redundancy which was retained in full propositional coordination was in the predicate; for example, "*Ue ni ageru no shita ni ageru no*," it is the redundant verb *ageru* which is retained.

Difference 2

In English, reduced coordinations in which the gap preceded its antecedent

(which may be called backward reductions) "The kitties \emptyset and the dogs hide," were acquired late, while reduced coordinations in which the gap followed its antecedent (which may be called forward reductions), "The teddy-bear walks and \emptyset sleeps," were acquired relatively early.

Japanese children evidenced late acquisition of grammatically reduced forms with forward gaps e.g., "Inu wa hoeru shi \emptyset kamitsuku" and relatively early acquisition of grammatically reduced forms with backward gaps, e.g., "Otona mo \emptyset kodomo mo oyogu."

Difference 3

Japanese children were highly sensitive to the unmarked subject-object-verb order and sentence structure in Japanese which distinguishes this language from English; they found sentences containing right dislocation significantly more difficult than standard subject-object-verb sentences.

SUMMARY OF DIFFERENCES

In summary, we not only found a set of basic commonalities between Japanese and English acquisition, but also a set of specific differences in early stages of acquisition of these two languages. The differences lie in redundancy patterns and in the location of gaps in early complex sentence formation. These four commonalities show that in spite of the significant differences across languages, both Japanese and English speaking children are following similar general principles in the acquisition of complex sentence formation. The differences reflect sensitivity to parameters of language variation.

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Self-Sustaining Dialect: A Model for Second Language Training

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0. INTRODUCTION

After some period of training, a foreign language student begins to acquire new knowledge about the target language based solely on his previous experience with that language. We propose that this kind of learning in the target language should play a central role in foreign language training. At some point in his first few years of training, the student should achieve the ability to sustain and increase his knowledge about the target language using only his second language skills. Once the student reaches this level of competency, he has acquired what we term a "Self-Sustaining Dialect" (SSD) of the target language. In this paper we will characterize the SSD and illustrate its importance in foreign language pedagogy.

1. SELF-SUSTAINING DIALECT

SSD is a pedagogical concept drawn from observing successful non-native users who started their training in the classroom. We recognize that the linguistic behavior of an experienced non-native user is often very different from that of the native. A non-native's use of a language approximates rather than replicates the skills of the native user. A non-native who has achieved fluency in some areas of a language is often incapable of functioning where natives are completely at ease: the lexicon of whole semantic fields may be beyond the experience of a non-native; dialectical variation to which a native adjusts without effort may cost a non-native considerable energy before he is able to cope; speech acts such as insulting, threatening, or joking which create an instantaneous reaction in a native may be grossly misinterpreted by non-natives. The non-native user will continually encounter unknown areas of the language. The *successful* non-native user is someone who can efficiently deal with this unknown.

Non-natives manage successful encounters with linguistic unknowns by employing an impressive array of acquired knowledge and skills. We can observe that a major part of these capabilities involve the ways the redundancy of the language is exploited. Even native speakers do not hear or read or understand every bit of information directed at them in their own language. Redundancy in their language, from the phonology to the discourse, allows them to combine what they perceive into a message. This is more pronounced when a non-native

interprets the information: less information is received, and the process of combination is not so automatic. Whereas the native is often blithely unaware of the processes of interpretation, the non-native is almost constantly conscious of the possibility of misinterpretation. Therefore, a non-native will have explicit, often practiced strategies based on redundancy to check his interpretations or elicit additional information. SSD, then, is a description of the requisite language skills for implementing these strategies.

Prominent among the strategies is what we broadly term "paraphrasing." A language will provide more than one appropriate response to nearly every instance of social intercourse requiring a linguistic response. The two utterances, "He's a doctor" and "He practices medicine," may be equally appropriate responses to the question "What does he do?" The potential of multiple responses to a single social situation results from the redundancy of a language, and the manipulation of this redundancy is an act of paraphrasing.

2. SSD MATERIALS AND TESTS

We propose to orient the first two years of university training in Japanese and Chinese to acquisition of SSD. This orientation in turn affects the materials, tests, and program design of the instruction. The materials developed for such instruction, while not being radically different from current textbooks, are characterized by at least two features: greater intensity in the areas of the lexicon presented by the selected vocabulary and paraphrasing exercises (or "shift" drills). When a lexical field such as "occupations" is identified, the vocabulary presented has a greater intensity than one usually finds in textbooks. Instead of presenting twenty names of occupations, for example, SSD materials might present only five, but soon after present the terms which refer to actions and places characteristic of these occupations. Secondly, the focus of the dialog and exercises is not solely on the native speaker model. Current textbooks present dialogs and exchanges which are purported to occur between speakers of equal competence in the language. Even when non-native roles are written into the dialogs, their linguistic behavior is not usually distinguishable from that of native speakers. Along with the native speaker models in dialogs and exercises, SSD materials present the dialog model of a successful non-native speaker and paraphrasing exercises which are specifically designed to inculcate the observed strategies of successful non-native speakers.

Testing in SSD oriented instruction has features not found—or at least not stressed—in other language training. Teachers test their students by observing how they handle linguistic situations which have been presented in the course of study. That is to say that the learner's performance in the early years of study is evaluated by achievement testing. As the learner's experience in the language increases toward some vaguely defined critical mass, proficiency tests are administered which are supposed to predict how well the learner will perform in the "real world" beyond the pedagogical havens of a particular program of courses. Since SSD instruction is specifically designed to train the

learner to cope with language and situations beyond that presented in dialogs and exercises, from early in the course, a significant portion of the testing procedure will be a kind of proficiency evaluation.

3. SSD PROGRAM

A three or four year university program of SSD instruction is divided into two main parts: pre-SSD and post-SSD. We estimate the pre-SSD period to be from 300 to 400 classroom hours. During this time, instruction is designed to teach the learner to use the language. Once that is achieved to a degree that the learner's dialect of the target language is not dependent on classroom or textbook, the learner has achieved an SSD. During the post-SSD period, instruction is designed to introduce the post-SSD entry into the confusing abundance of native linguistic artefacts by identifying "traditions" of the spoken and written language and focusing the learner's attention on the conventions of each tradition. Traditions of the spoken language might include such things as movies, television drama, radio shows, business conferences, or history lectures. Written traditions might include magazines, newspapers, or modern fiction. Each tradition can be a discreet course of study in the post-SSD instruction. When a student completes his university training in Japanese, for example, he will possess an SSD of Japanese, and will have had the experience of applying his SSD to a variety of Japanese language traditions. The combination of traditions should allow him to enter other modern traditions and thereby increase his knowledge of the "real world" of the Japanese language, and, at the same time, sustain and increase his skills in that language.

Development of Japanese Vocabulary in Bilingual Children

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A large number of studies related to bilingualism have been published in the past decade. Most of these studies deal with the process of becoming bilingual rather than the state of being bilingual and they tend to focus on the learning of the second language (L_2), which is very often the primary or dominant language of the sociopolitical society in which the learner is to function. Relatively small number of studies take into account the second language learner's first language (L_1) and test his proficiency in L_1 , and they tend to treat bilingualism in two related languages within the Indo-European language family.

The study reported in the present paper was designed to investigate the maintenance and/or continued development of L_1 (Japanese) with a focus on children who were exposed to L_2 (English or French: structurally unrelated language to L_1) as a result of change in the country of residence.

METHOD

The subjects were 651 Japanese children of a high SES family living in Chicago, Seattle, Washington D.C., Vancouver, Melbourne and Geneva for variable periods (Table 1). They attended, in addition to an English or French speaking school, grades 1 through 9 of a school of Japanese supplementary studies which held classes usually once a week on Saturdays. The major aim of the school of Japanese supplementary studies is to develop the Japanese

Table 1. Subjects

grade	(age)	N	length of residence (years)			
			(male	female)	mean	(s.d.)
1	(6-7)	137	(68	69)	3.9	(2.1)
2	(7-8)	108	(59	49)	4.1	(2.4)
3	(8-9)	106	(59	47)	4.1	(2.4)
4	(9-10)	77	(37	40)	4.6	(2.9)
5	(10-11)	80	(35	45)	5.1	(3.0)
6	(11-12)	57	(17	40)	4.5	(2.7)
7	(12-13)	35	(17	18)	5.5	(2.6)
8	(13-14)	26	(11	15)	5.0	(3.3)
9	(14-15)	25	(5	20)	5.4	(3.1)
651			(308	343)	4.4	(2.6)

academic skills of expatriate children to a level commensurate with scholastic expectations in Japan. Almost all parents were "temporary residents" for job-related reasons, and who intended to eventually return to Japan. Thus, there was high motivation to maintain children's L_1 (Japanese) proficiency as well as to develop children's L_2 (English or French) skills.

To these 651 subjects, a semi-adaptive test of Japanese vocabulary (Shiba, 1978) constructed on the basis of the latent trait theory (Lord et al., 1968) was administered. Testing procedure consisted of two steps: a program test for preliminary measurement and a conventional, group test for accurate measurement (Shiba et al., 1980, 1981). The program test is a short form of the stratified adaptive test of five pages, each of which contains seven vocabulary test items of diverse difficulty. Each subject was to be tested individually, at first, by the program test, and then, to be given an appropriate form of conventional test chosen among ten forms from the easier to the more difficult one, according to the obtained score of the program test. The final scores were estimated by maximum likelihood method, and then converted to T-scores with a mean of 50 and S.D. of 10 based on norms of Japanese children living in Tokyo.

To the same subjects, a questionnaire was administered in order to obtain children's subjective reports on their use of L_1 and L_2 as well as on their oral and written proficiency in L_1 and L_2 . In the questionnaire, the rating scale method was employed.

RESULTS AND DISCUSSION

The distributions of Japanese Vocabulary Scores were lower and wider in their ranges, in general, comparing to the distributions of the Scores obtained from the standard group of children living in Tokyo (Table 2).

Correlational analysis between length of residence outside Japan and Japanese Vocabulary T-score indicated that the children who had been in L_2 speaking country for longer years showed more retardation in their Japanese vocabulary development ($r = -.45$ $n = 651$). Further analysis revealed that the strength of correlation was not the same in all grade groups (grades 1 and 2: $r = -.50$ /grades 3 and 4: $r = -.54$ /grades 5 and 6: $r = -.55$ /grades 7, 8 and 9: $r = -.34$). This suggested, as there was only a slight difference in mean and S.D. of length of residence among the grade groups (Table 1), that the age of entry into a L_2 speaking environment might be a factor which determined the later maintenance and

Table 2. Distributions of Japanese vocabulary Scores (T-score)

grade	mean	s.d.	
1 and 2	37.3	16.3	($n = 245$)
3 and 4	37.8	16.0	($n = 183$)
5 and 6	41.8	15.4	($n = 137$)
7, 8 and 9	44.5	17.0	($n = 86$)
	39.4	16.3	($n = 651$)

development of L_1 . In order to examine the effects of group differences in age of entry, Japanese Vocabulary T-scores were analysed in relation to age of entry and length of residence outside Japan (Table 3). In this analysis, 135 subjects who had resided in Japan between the first and the second residence outside Japan were omitted.

The results showed that the age of entry made a considerable difference in the rapidity with which L_1 proficiency was developed. Those who entered a L_2 speaking environment for the first time after the age nine or ten continued to maintain grade norms in L_1 skills. Besides, the analysis of the questionnaire suggested that these older immigrant children approached grade norms in L_2 academic skills, even if their dominant or comfortable language was L_1 rather than L_2 for longer years in comparison with younger immigrant children. On the other hand, those who entered a L_2 speaking environment at younger ages, especially before the age of five or six that is without L_1 schooling, did not maintain grade norms in L_1 skills.

In summary, the present findings agree with those of other studies in showing that older immigrant children whose L_1 proficiency is better established at the time of intensive exposure to L_2 maintain and develop their L_1 to a greater extent than children who immigrate at younger ages. This suggests that the critical period for the acquisition of maternal language might exist by the age of ten.

Table 3. Age of entry, Length of residence and Japanese Vocabulary Scores (mean T-score)

Age of entry	Length of residence (years)							
	0~1	1~2	2~3	3~5	5~7	7~10	10~13	
0					19.4 n=4	20.4 n=25	13.1 n=9	18.5 n=38
1, 2					31.1 n=20	22.3 n=12	36.7 n=4	28.8 n=36
3, 4			49.2 n=4	34.3 n=41	24.5 n=15	41.5 n=4	16.1 n=1	32.4 n=65
5, 6	48.7 n=4	49.0 n=23	36.9 n=44	31.8 n=25	28.1 n=9	41.5 n=8	22.2 n=4	37.8 n=117
7, 8	51.3 n=29	42.6 n=40	41.0 n=29	36.4 n=28	43.2 n=8			42.9 n=134
9, 10	47.9 n=25	46.3 n=17	42.8 n=20	48.7 n=9	47.7 n=5			46.2 n=76
11, 12	59.1 n=13	54.1 n=7	48.5 n=11	46.9 n=5				51.4 n=36
13, 14, 15	41.5 n=5	52.8 n=2	49.8 n=7					47.3 n=14
	49.9 n=76	46.1 n=89	41.3 n=115	36.4 n=108	31.2 n=61	20.5 n=49	20.5 n=18	38.8 n=516

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RESULTS AND DISCUSSION

Table 1. The mean scores of Japanese and Japanese-Vietnamese children on the Vocabulary Test.

Table 2. The mean scores of Japanese and Japanese-Vietnamese children on the Vocabulary Test.

Table 3. The mean scores of Japanese and Japanese-Vietnamese children on the Vocabulary Test.

Table 4. The mean scores of Japanese and Japanese-Vietnamese children on the Vocabulary Test.

Table 5. The mean scores of Japanese and Japanese-Vietnamese children on the Vocabulary Test.

Table 6. The mean scores of Japanese and Japanese-Vietnamese children on the Vocabulary Test.

Table 7. The mean scores of Japanese and Japanese-Vietnamese children on the Vocabulary Test.

Table 8. The mean scores of Japanese and Japanese-Vietnamese children on the Vocabulary Test.

Table 9. The mean scores of Japanese and Japanese-Vietnamese children on the Vocabulary Test.

Table 10. The mean scores of Japanese and Japanese-Vietnamese children on the Vocabulary Test.

Table 11. The mean scores of Japanese and Japanese-Vietnamese children on the Vocabulary Test.

Table 12. The mean scores of Japanese and Japanese-Vietnamese children on the Vocabulary Test.

Table 13. The mean scores of Japanese and Japanese-Vietnamese children on the Vocabulary Test.

Table 14. The mean scores of Japanese and Japanese-Vietnamese children on the Vocabulary Test.

The Mental Lexicon in Second Language Learning

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Despite various assumptions, hypotheses, and several recent experimental studies, very little is known about the composition, structure, functioning, and growth of the mental lexicon (ML) even in monolingual persons. The adjective *mental* (or subjective, internal) has been used to distinguish this type of unique word store in the language user's long-term memory from printed word lists, glossaries, dictionaries or thesauri in which lexicographers have attempted to capture the vocabulary (or "objective" lexicon) of a given language.

The major approaches mentioned here fall into several categories. Most psycholinguistic studies have used lexical decision tasks with adult native speakers of English (college students) as subjects. In their study of homographs, Rubenstein, Garfield and Millikan (1970) have shown that high frequency entries are recognized sooner in the ML. Glanzer and Ehrenreich (1979) have confirmed the effect of word frequency on lexical search questions. In meaning-oriented approaches, Fillenbaum and Rapoport (1971) have reported results of their extensive study of nine semantic domains to show how semantic networks might be organized in the ML. Similarly, Blaubergs (1980) has recently proposed a model of the ML based on an interesting combination of the notion of semantic fields with Fillmore's case grammar. In a different approach to semantic loads of lexical units stored in the ML, Conrad (1974) has shown that in cases of lexical ambiguity both meanings of a word are activated in memory when that word is heard in a sentence, despite the fact that the sentence may contain enough contextual information that points to only one and not to the other meaning. Conrad's claim that the lexical look-up phase of the decoding process seems to be independent of the linguistic (and extralinguistic) context is in direct contradiction to Coker and Crain's (1978) hypothesis that the ML allows direct access of a specific meaning (of an ambiguous word) if sufficient context is provided and that several meanings of one lexical unit will be looked up only when the context fails to provide sufficient clues.

Studies of malapropisms have provided additional dimensions in approaches to the ML. Fay and Cutler (1977:510) have posed a question which will be of great importance in further studies of the development of the ML in a second language: "... whether there are separate dictionaries for production and comprehension, or simply a single listing that is used in both operations. It might be argued that there should be two listings, since the optimal arrangement of the entries for the purpose of comprehension is surely not the optimal arrangement for production." In his recent study of malapropisms, Cowart (1979)

dismisses several previous assumptions concerning the functioning of the ML (such as the single access, the passive memory, the heterogeneous memory) and proposes instead a model consisting of three separate active lexicons (phonemic, syntactic, semantic) interlinked with each other by a system of pointers. Zwicky (1979) explains the mechanism of malapropisms by introducing the notion of insecure, defective or incomplete storage of the target unit in the ML of native speakers. For foreign learners of a language, such qualitative aspects of word storage apparently affect considerable areas of their word knowledge and hence will deserve intensive study.

A series of useful experimental studies has focused on the problem whether the morphological make-up of lexical units affects their storage and look-up in the ML. This, again, is a problem with important implications for the methodology of vocabulary instruction in foreign-language learning. While Manelis and Tharp's (1977) experiments on suffixation appear to support the independent storage hypothesis (that each affixed word is stored as a single entry), several studies by MacKay (1976, 1978, 1979) have shown the feasibility of the derivative hypothesis according to which stems and affixes of complex words are stored separately in the ML. Similar results in favor of lexical decomposition were reported for prefixed words by Taft and Forster (1975) and Taft (1979b). For English as the target language, these findings support the importance of "feeding" the ML of foreign learners with lexical units surrounded by their derivational as well as inflectional paradigms.

Several other categories of approaches that will be of importance for understanding the developmental aspects of the ML in a second language include a series of studies examining the order of acquisition (or order of difficulty) of grammatical morphemes in children and adults learning English as a second language. For lack of space only two most recent overviews of these studies in Krashen (1981:51-63) and in Dulay, Burt and Krashen (1982:200-231) can be cited here. Other approaches have focused on methodological problems of vocabulary expansion in a foreign language (e.g. Twaddell 1973, Richards 1976, Nilsen 1976, Judd 1978). In the areas of English morphology and lexicology several studies have analyzed the "objective" lexicon, the make-up of lexical units, individual types of English word-formation as well as the degrees of productivity in coining neologisms (e.g. Soudek 1967, Marchand 1969, Algeo 1980, Caso 1980). Instances of yet other important contributions that might shed light on multilingual MLs are various studies of polyglot aphasia. For example, Paradis (1977) has reported several cases of mixed vocabularies and suffix interference between two languages. Last but not least, important advances in neurolinguistics have led to attempts (such as Ojemann and Whitaker 1978) to locate, within the dominant hemisphere, different (as well as shared) areas used for storing words in two languages.

As can be seen from the above tentative categorization of some major approaches, attempts to characterize the developing ML in a second language represent a veritably multidisciplinary venture that faces many formidable

problems. So far, the ML of native users has been largely described in static terms, as a developed, accomplished, "fixed data base" (Miller 1981:66). For the development of the ML in a second language, the dynamic aspects of stages of word acquisition, of growth of the ML will be of major importance (see, e.g., Nelson 1981). While some authors (Rubenstein et al. 1971, Fay and Cutler 1977) have argued for a primary phonemic representation of lexical units in the ML, typical instances of adult learning of a second/foreign language which often takes place through the medium of written (printed) symbols would favor a model of several access files, as proposed by Forster (1978), one of which is based on the orthographic shape of the stored words (see also Taft 1979a, Lukatela et al. 1980). The mode of word acquisition (written, spoken or both) of ML entries is especially significant in English whose spoken and written norms are very different from the point of view of the foreign learner and may be acquired separately. This will probably apply to an even greater degree to languages such as Chinese whose written form is not at all based on phonological representations.

Because of severe space limitations only a handful of other essential problems of second language ML research can be enumerated here rather telegraphically. The empirical distinction made by language teachers between receptive (passive recognition) and productive (active) vocabulary in second language learning will have to be examined in terms of either uniform or separate mechanisms of word comprehension and production in the ML. (Both present writers "know" a lot of words in English, and in other languages that are not their first language, in the sense that they have a vague idea of some of their properties (such as semantic range, graphic, phonemic, syntactic shape, idiosyncrasies) but not of others, and thus would not dare to use them actively.) Closely linked with this phenomenon is the frequent use of avoidance strategies which often cover whole semantic fields where the foreign learner feels insecure. Other problem areas include phenomena of overlap, interference or integration between and among several MLs in multilinguals, problems of lexical hybridization when affixes from one language are freely attached to bases from another language in a bilingual's speech, problems of mental fatigue and overload which often block or restrict the bilingual's capacity to retrieve well known terms in another language, questions of storage of lexical forms common in both languages of a bilingual (or a second language learner) but a) having entirely different semantic loads, b) sharing some meanings but differing in others, c) belonging to different stylistic or functional levels or d) displaying different grammatical categories such as gender or part of speech in each of the two languages. As certain types of bilinguals freely switch from one language to another but will have difficulties to translate, it seems possible that in some kinds of bilingualism "equivalents" may be stored separately, even without a mutually directive pointer system. Finally, the well known phenomenon that, under certain conditions, long eradicated "errors" in second language performance tend to reemerge (see Selinker's "backsliding", 1972:215-216) appears to

suggest that, with lexical units, "incorrect" representations may be stored along with "correct" entries in the ML.

A plausible model of a dynamic second language ML will have to cope with these problems as well as with other formidable challenges. Such a model has yet to be designed.

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Dual-Plane Strategies in Foreign Language Learning

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1.

Dual-plane strategies try to combine spontaneous language acquisition and conscious language learning in the classroom in order to develop the learner's communicative competence. In modern language study, the problem dates back to Harold E. Palmer's distinction between spontaneous and studious language learning capacities, and through him, to Ferdinand de Saussure's methodological antinomies.

In his Memorandum on Problems of English Teaching, published in Tokyo in 1924, Harold Palmer, the distinguished linguist and language educationist, called the interest of Japanese scholars to the Saussurian way of thinking,¹⁾ and put forward suggestions in the field of linguistic pedagogy. His Memorandum proved to be an early document of cooperation between theoretical and pragmatic linguists and psychologists. It is timely to remember it at this International Congress of Linguists which has first opened a section in its program for the topic of "Language Acquisition and Language Learning".

Palmer's seminal ideas took shape in some modern theories of language learning and communication pedagogy. The theoretical underpinnings of this paper can be described in terms of four connected and empirically supported hypotheses:

- 1) the acquisition—learning hypothesis,
- 2) the dual-plane hypothesis,
- 3) the monitor function hypothesis,
- 4) the antinomic character hypothesis of the dual plane.

The acquisition-learning hypothesis makes clear distinction between spontaneous language acquisition, and conscious language learning through error correction and explicit instruction. (Stephen Krashen and others). It emphasizes realistically that acquisition is possible without learning, and that learning does not lead necessarily to acquisition. On the other hand, it leaves out of interest a whole scale of stages between the two extremes.

The dual-plane hypothesis suggests that human communication proceeds on two planes. The first plane is the domain of full-awareness activities, generally

1) According to Tullio de Mauro (1968. Ferdinand de Saussure. Bari) the activity of Harold E. Palmer in Tokyo contributed to the chronological fact that the first translation of F. de Saussure's *Cours de Linguistique Générale* was published in Japanese.

pertaining to message construction. The second plane is the sphere of reduced-attention activities, with monitored communication skills and emotionally charged psychic reactivities. Holistic language pedagogy in using the dual-plane hypothesis greatly relies on the Lozanov-model.

The antinomic character hypothesis suggests that fully conscious and para-conscious aspects of human communication on the one hand, formal language learning and informal language acquisition on the other, are usefully opposed in language pedagogy. This methodological antinomy, however, can be also treated as two extremes of a cline.

The monitor function hypothesis claims that in normal language use the person who knows the language is able to detect his own errors in speech even though he is not listening for them. (R. Lado). This hypothesis has been largely extended during the past two decades. (S. Krashen, A. A. Leontiev, A. Szentgyörgyvári).

In the following I propose to point out some implications of the dualplane and the monitor hypothesis, as an outcome of experiments and experiences with adults.

2.

If we accept that, in fluent language use, we are normally aware only of the message and some key-points of linguistic performance, we have to admit that units and patterns of the message-processing mechanisms are recalled automatically on the second plane. Old experience and new experiments give evidence of it. From the pedagogical point of view we may add that language skills are not only recalled, but also economically acquired on the second plane (Lozanov, Krashen). The crucial problem for dual-plane strategies centers on how to develop language skills on the second plane in the classroom.

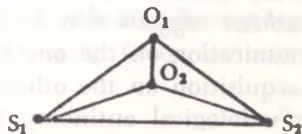
Message-processing language skills are developed in the context of speech situations. Tatiana Slama-Cazacu's dynamic-contextual theory offers a good approach for analysing it. For the purpose of this short discussion, however, we can try to grasp the core of problem with the help of a simplified (topological) graph, by mapping out the essential means of expression in speech situations.

Based on semantic research (Alen Gardiner, Adam Schaff), it can be postulated that the minimal number of meaning components can be reduced to four: someone is needed to speak, someone to listen to, something to be spoken about, and linguistic signs for the expression. Correspondingly the following symbols can be used:

S_1 for the speaker's coordinates,
 S_2 for the listener's coordinates,
 they are the objective components
 of meaning

O_1 for the things which elicit communication or serve as a context for it,
 O_2 for the socially accepted linguistic signs which carry the message,
 they are the objective components
 of meaning

The explicit and implicit peculiarities of the essential components of meaning, their interrelations and the set of triangles they form, offer a synoptic view of the main functions in the speech situations. (Static model).



As we are interested in communication skills, we have to overview the speech situation from the point of view of activities which involve skills. They are given in the three interrelations between the speaker and the listener (S_1S_2 , $S_1O_2S_2$, $S_1O_1S_2$). In the dynamic dimension they can be called information channels:

- 1) the pragmatic channel (S_1S_2)
- 2) the linguistic channel ($S_1O_2S_2$)
- 3) the sigmatic channel ($S_1O_1S_2$)

The pragmatic channel (S_1S_2) gives information on face-to-face experience: physical aspect, looks, mimics, gestures, postures, voice, rhythm of movements, etc. (kinesics, proxemics, paralinguistics).

The linguistic channel ($S_1O_2S_2$) conveys information according to the rules of language use.

The sigmatic channel ($S_1O_1S_2$) transmits stimuli of the surroundings, likely to affect the communication: things spoken about, light, noise, heat, and the behaviour of persons present but directly not involved in the communication.

In fact, the information channels are inseparable components of the whole information system,—completing, reinforcing or diminishing each other's information value.

3.

If we apply now our dynamic model on the pedagogic message-processing situation, the speaker-listener correlation of the interlocutors will be modified in learner-teacher cooperation. The basic peculiarity of this interpersonal learning situation appears in the disparity of meaning potentials.²⁾ The meaning potential of the language learner is poor because he does not have the necessary means to express what he means. He is lacking in language skills and in the knowledge of how to monitor them. He cannot avoid having his attention divided between message construction and chain-and-choice difficulties of linguistic performance. He will more often focus on the form than on the message.

On the contrary, the teacher as a "knower" has a good command of language

2) We use here M. A. K. Halliday's term in a broad sense: the meaning potential of a person is the set of linguistic and non-linguistic means to express what he means.

skills with the necessary knowledge of monitoring them. He need not concentrate on the message either, as it is a routine message of his professional metacommunication. Moreover, as an instructor by profession, he is supposed to have the training in getting the learner to use the target language. Thus, his meaning potential enables him to mobilize the whole information channels system of the classroom in order to develop the learner's linguistic behaviour, to suggest the message and to keep an eye on the learner's progress in proficiency. He can quickly and frequently shift the focus of his attention from the first plane to the second plane of the learner's communication. Actually, the dual plane of the learner is under his conscious control. This is the basis for dual-plane strategies.

We have to note here that conscious control and guidance does not mean "teaching" in the classical sense. In interpersonal learning, the teacher's role is the role of the knower and instructor who influences the learner by direct suggestions on the first plane, and indirect suggestions on the second plane.

In different countries, dual-plane language pedagogy has scored noticeable results, especially with slow learners in dialogue-fluency. However, at the present state of brain research the results of dual-plane experiments can be evaluated only in probabilistic terms.³⁾

Anyhow, dual-plane pedagogy has led to some considerations which I propose to telescope here:

- 1) The attempt to develop language skills on the second plane has often proved economical in the classroom.

- 2) Second plane reactivities can be elicited more successfully if the learner is strongly engaged in first-plane activities.

- 3) First-plane suggestions of the instructor are more effective if effective support is provided for the learner on the second plane.

- 4) The basic type of dual-plane strategies can be called "focus on the message". Its role is decisive as it brings the learner near to authentic language use. Being the most difficult task in classroom metacommunication, it must be supported with special pedagogical techniques for diverting the learner's attention from the form.

- 5) Some partial skills of linguistic chain-and-choice performance can not be consolidated on the second plane, without their special handling on the first plane. They require strategies with "focus on the form". Here we come to the problem of the monitor because it is relevant to consolidating skills.

4.

In recent years the monitor has become a problem both in psycholinguistics and language pedagogy. Monitoring is considered a control process of linguistic behaviour, but there is no consensus concerning the peculiarities of the monitor as a control-process and the processes to be controlled by it. My remarks in this

³⁾ Szentgyörgyvári A., 1981. Holistic methods in adult language learning. In: *AILA '81 Proceedings* I. B. Sigurd and J. Svartvik eds.

paper are to be limited to the paraconscious monitor as a general phenomenon of normal language use, and the learner's conscious monitor.

The paraconscious monitor is called paraconscious because it is not entirely conscious. It follows the message-processing in a state of latent vigilance, but it rises in the scale of consciousness to the stage of actual cognition if an error or the imminent danger of an error occurs.

Paraconscious monitor may be acquired by formal learning, by inferencing, or under social constraints. It is to control automated language skills. If the skills are not automated, the monitor must be fully conscious. But the fully conscious monitor is no more the peculiarity of normal language use, it belongs to a special message-processing, characteristic of the learner. The learner's conscious monitor has been described vigorously by Krashen. It is clear now that conscious monitor cannot replace automated language skills, neither develop them. However, the relevance of the conscious monitor to language skills cannot be decided on the basis of this statement. Paraconscious monitor can be developed, reinforced and refined if it is sometimes raised on the scale of awareness to the stage of conscious monitor.

In conclusion, I should like to remark that dual-plane strategies, with their findings and fallacies, fulfilments and failures, represent a working hypothesis pertaining to integrative approaches of language learning. They suggest a challenge to developing the theoretical and experimental foundations of language acquisition and language use.

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Linguistics-Based Revision of Foreign Language Teaching Materials: An Experimental Study

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In John B. Carroll's model of predicting success in foreign-language learning, 'quality of instruction' is one instructional variable next to 'opportunity to learn' as the second one and 'language learning aptitude', 'motivation' and 'general intelligence' as individual variables (Carroll 1962). I consider 'teaching material' one subfactor of 'quality of instruction' besides 'teacher-learner-interaction' and 'methods and techniques of instruction'. All these are yet rather coarse categories. I submit, that we are yet far from understanding what kind of effects are linked with certain aspects of instruction. So also in the area of teaching materials. Up to now we do not have much information on what features of teaching materials actually contribute to teaching and learning a foreign language successfully. This lack of information, no doubt, is due to the intricacies of the interactions among the various factors influencing language learning in a classroom situation.

Mueller/Harris 1966 showed that a programmed audio-lingual course for teaching French in a US college neutralized the predictive force of language learning aptitude as tested by the Carroll-Sapon test and positively influenced the students' motivation. In this study, teaching materials, the teachers' use of them, teaching techniques and modes of interaction are all lumped together as one single independent variable. But it stands to reason that one of the features of the course tested, its having been carefully programmed, may have contributed to the overall improvement of learning in comparison with a control class taught by more traditional methods and materials.

Experimental studies by Politzer 1968, Valdman 1974 and Knapp 1980 indicate that sequencing of linguistic properties in a teaching programme influences learning results. There is also evidence from non-experimental work which supports the hypothesis that order of presentation of linguistic properties to be learnt may be a factor making for ease or difficulty of a specific language learning programme. The evidence partly comes from experience with teaching programmes (Aronson 1966; Zabrocki 1970; Richards 1972; Christ 1979), partly from studies of learners' errors (George 1972).

In a study conducted jointly with Frank Achtenhagen, educationist at the University of Göttingen (West Germany), we observed the use of learning materials for English as a foreign language in classroom interaction between teachers and learners. The construction of the learning materials with regard to syntactic properties of English had been analyzed previously. Selection,

ordering and distribution of syntactic properties in a textbook had been described with a transformational grammar of English designed specifically with that purpose in mind. Certain constructional features of the teaching materials could be expressed on the basis of the syntactic analysis of all the sentences of the teaching materials (Achtenhagen/Wienold et al. 1975).

Ten weeks of two beginners' classes of English in a German high school were recorded and video-taped. Again, we analyzed the selection, ordering and distribution of syntactic properties in the classroom presentation of the teaching materials. We tabulated learning difficulties in the classroom and errors in a test administered at the end of the observation period. The test was constructed on the basis of the linguistic description of the teaching materials as used in the classroom. From the analysis of learning difficulties and errors such syntactic properties of English were selected for which we were able to argue that difficulties and errors were related to the construction of the teaching materials, i.e. ordering and distribution of linguistic properties in the materials. For this set of properties the materials were revised. The revised version was used in the next year's beginners' classes. Again, teaching and learning with the revised materials were observed, recorded and analyzed.

The children participating in the study were tested for language learning aptitude, general intelligence, anxiety. Grades in English after the first half year of instruction and teachers' assessments of the learners' abilities were also collected. Multivariate statistic analysis of the difference in test results with the original and the revised materials showed a highly significant positive effect on learning by the revised materials. Please note that the revision pertained only to narrowly defined aspects of the construction of the teaching materials, that is a small set of syntactic properties and their ordering and distribution in the textbook. All other aspects remained unchanged. We argue, therefore, that the improvement in learning results can be attributed to this narrowly defined change in the ordering and distribution of syntactic properties (Wienold et al. 1967; Wienold et al. 1982).

I have reported here only on the contribution of linguistic description to the project, its contribution to the analysis of teaching materials and classroom use of the materials, to the construction of a test and to the revision of teaching materials. This is only one part of the study. The study also investigated classroom interaction, the differences in frequency and qualities of teaching materials assigned by a teacher to individual students, and possible factors which may account for the variation in teachers' interaction with individual students (Wienold et al. 1976; Wienold et al. 1982).

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When is Language Planning not Planning?

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There are three discernible strands in the development of the theory of language planning. The first is a reaction against unplanned growth in language. The natural processes of language change, such as sound changes, meaning shifts, loans and loan creations, are slow and generally uncoordinated. Language planning can quicken the pace of such changes and impose a definite direction on them. This approach is typified by Haugen's work, particularly in connection with Norwegian (Haugen 1961, 1966). Such language planning tends to be limited to standardisation and various aspects of corpus planning, including vocabulary expansion, orthographic reform, and guide to pronunciation.

The second strand in the development of the theory of language planning is a reaction to the linguist's non-normative and egalitarian attitude to language which is expressed in statements such as "All languages are equal. There is no concept that cannot be expressed in any language, if the need to do so arises. The job of the linguist is to describe not prescribe language use". These well-known concepts are believed to run counter to the basic conception of language planning. If the linguist is right, so the argument goes, there will be no basis for interfering with language. This approach to language planning is typified by Tauli (1974: 51-52) who goes as far as to assert that "No language can express everything adequately... there are also many imperfections in language which need not occur... It is a well-known fact that language lags behind thought". The language planner is, therefore, supposed to re-shape an imperfect tool, impose order on disorder, and bring out the "beauty" of the language. This type of language planning concentrates mainly on corpus planning, with excessive emphasis on changing features of an existing language code.

The third strand in the development of the theory of language planning is that language planning is a reflection of the planning model generally. As in economic planning, "goals are established, means are selected, and outcomes are predicted in a systematic and explicit manner" (Rubin 1971: 218) and the planner makes "a conscious choice between alternative ways of solving a problem—a choice that is made on the basis of a conscious effort to predict the consequences of the proposed alternatives" (Thorburn 1971: 254). Applied to language planning, this approach presupposes the identification of some language problem through fact-finding, a plan of how to cope with the problem and the possible outcomes, a policy decision, implementation, and evaluation.

at every stage (Rubin 1971). It is this canonical model of language planning that has become most influential in the theory of language planning. In this paper, it is suggested that the model is inadequate because it does not account for a lot of language planning situations, particularly in the developing countries of the world.

The canonical model of language planning is defective in five major respects: it forces language behaviour into the narrow mould of economic planning, its operative processes are idealistic and Eurocentric, its emphasis is on the negative aspects of language, it is weighted in favour of corpus planning, and it is too much government-oriented.

Although language planning may gain something from the rigour of economic planning models, and insights such as that planning is "problem-solving", "future-oriented" and susceptible to "cost-benefit analysis" (Thorburn 1971), it is an over-simplification to assume that language can be treated like any other resource that can be easily regulated (Jernudd and Das Gupta, 1971). Pool (1979) has drawn attention to the important fact that, in so far as language planning involves change in language habits, it may also be said to involve another kind of planning which he calls "identity planning". Das Gupta (1976: 210) has also shown that language planning differs substantially from economic planning in terms of resource allocation, political versus economic constraints, and the consequences of delay in implementation. What all this shows is that there cannot be an exact fit between an economic planning model and a language planning one. The latter has to take account of the societal and idiosyncratic aspects of language behaviour.

The canonical model of language planning assumes an ideal situation in which the so-called planning is rational and systematic (Jernudd and Das Gupta 1971: 196). It has, no doubt, been influenced by the situation in the Western democracies with their parliaments, commissions of inquiry, planning commissions and bureaucratic procedures. The situation in most of the developing countries is very different. Quite often, no preparatory steps are taken to identify problems, consider alternative solutions and forecast possible outcomes. Rather, decisions are simply taken, and their implications and the strategies for their implementation are only worked out *after* such decisions have been taken.

Because of the need to justify planning against the background of perceived needs or problems, the tendency in the canonical model of language planning has been to emphasize "language inadequacies" for which remedial action is required. The term "language treatment" (Neustupný 1974) also implies diagnosis of a linguistic ailment. As Kubchandani (1975: 163) has rightly pointed out, it may be more useful to abandon the "handicap model" in favour of the "adaptation model" which takes into account both the assets and the inadequacies of the language in question.

In spite of the claim of the canonical model of language planning to cover both status planning and corpus planning, one finds that the examples cited in the literature that fit the model tend to be of corpus rather than status

planning. Thus, the activities of the Swedish Language Committee, the Swedish Centre for Technical Terminology, the Academy of the Hebrew Language etc conform to the model of problem identification and decision making. But when we take a look at decisions on language choice, the model invariably breaks down, and the processes of such choices are said not to fall strictly within the ambit of language planning. For example, the emergence of New Guinea Pidgin in Papua New Guinea or the choice of Swahili as Kenya's national language.

The canonical model of language planning is not only too much government-oriented, it even places undue emphasis on the existence of a central authority to the neglect of subsidiary levels of government. In a federal set-up, for instance, in which states have concurrent jurisdiction over language matters, it is to be expected that there will be several dimensions of planning (Das Gupta 1976: 209). It is even possible that divergent policies may be pursued at the different levels.

Actual experience with language development activities, particularly in the developing countries, shows that the canonical model of language planning fails to account for many significant developments in language policy as well as the effective work being carried out by non-governmental bodies. Examples from Africa, such as the decision to adopt Swihili as Kenya's national language, the decision on a common script for Somali, and the formulation of the national policy on Education in Nigeria, illustrate the inherent weakness of the canonical model in this respect. Of course, it is easy to dismiss such counter-examples as instances of language treatment as opposed to language planning. But how useful is a model of language planning, if it is so severely restricted? We are left with two options: to maintain the canonical model in spite of its shortcomings, or to modify it so that it will embrace a wider range of language activity at different levels involving governmental and non-governmental effort. There can be little doubt that the second option is more likely to make language planning a more meaningful and fruitful field of study.

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Contrastive Linguistics (CL), Error Analysis (EA), Interlanguage (IL) and Their Relevance for Language Planning

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There is no doubt that 'modest' CL (= 'weak version')¹⁾ has a role to play in foreign language (FL) teaching. It may be that during the discussion of some of the disputed issues a few more general and important 'by-products' of CL have been ignored. (1) The role of the mother tongue in FL teaching: Though different according to individual parameters it has been brought back into discussion not only as one of the important sources of negative transfers, but also as the important channel of thoughts in the communicative process by non-native speakers (e.g. silent translation). No strict principle of the exclusion of the mother tongue in FL teaching seems to be necessary, but rather one of well-dosed use whenever possible. This naturally does not apply to language-heterogeneous situations where teachers cannot possibly know all the linguistic backgrounds. (2) More positive attitudes towards the test form 'translation' as a type of cross-language and cross-cultural test. (3) Teaching materials, including grammars, should include the contrastive element as one among several factors²⁾. (4) Limitations concerning the success of FL teaching under artificial conditions varying according to many factors. More honest and realistic attitudes would certainly help a great deal to lower expectations as to the possibility of producing 'near native-speaker competence', and to describe feasible aims within particular socio-economic and cultural frameworks³⁾.

Systematic EA was mainly meant to help CL to find short cuts for its falsification or verification. Now it has become a more and more independent area (there is no doubt that in spite of this independence from CL there is still some considerable inter-dependence between the two fields, and sometimes up to about 50% of all errors, particularly on an adult level, can still be explained in contrastive terms depending upon a great deal of factors mentioned

1) For the dichotomy 'strong version' vs. 'weak version' cf. Wardhaugh, R., "The Contrastive Analysis Hypothesis." In: Schumann, J. H./Stenson, N. (eds.), *New Frontiers in Second Language Learning*. Rowley, Mass., 1975, 1-19.

2) Cf. e.g. *Asian Language Notes. Some Likely Areas of Difficulty for Asian Learners of English*. Published by The Language Teaching Branch, Commonwealth Department of Education, Canberra, 1979.

3) Cf. e.g. van Ek, J./Alexander, L. G., *Threshold Level English*. Oxford, 1980; Trim, J., *Developing a Unit/Credit Scheme of Adult Language Learning*. Oxford, 1980.

above⁴⁾: (1) The role of the mother tongue in producing all kinds of negative transfers with varying degrees according to factors mentioned above. (2) The 'natural' status of errors within the language-learning process. (3) A more lenient attitude in error evaluation, which also implies encouraging praise in connection with rather exceptional errorless performances. An interesting phenomenon should be mentioned here, too, namely the sociolinguistic fact that in general native speakers will evaluate and judge non-native speakers' performances more leniently than non-native teachers⁵⁾. There may be several different reasons for this, and one can only hypothesize on this point: (a) The communicative aspect is perhaps automatically given more consideration. (b) Sometimes perhaps a certain degree of admiration because of one's own lack of FL competence. (c) A higher degree of language security covering all kinds of registers, dialects and also a knowledge of 'objective difficulties'. This problem urgently needs further investigation. It certainly, even at this stage of hypothetical reasoning, automatically leads towards the demand for more native speakers in FL departments concerned with the evaluation of FL competence. Certainly error evaluation within FL teaching is not primarily a linguistic, socio- or psycholinguistic problem, but rather a pedagogical one. Still, some of the parameters that could be called in within the highly complicated process of evaluation would be for instance: communicability, acceptability (by native speakers in FL situations), contrastive factors including the parameter of so-called 'difficulty', and the pedagogical parameter (time spent on practising a given structure). Needless to say, their hierarchical ordering would depend e.g. upon the learning objectives. (4) Within error evaluation the problem of grammar vs. lexis is still one of the major problems. (5) There is also the very important therapeutic aspect of EA, namely remedial teaching. Psycholinguistic insights with which we can differentiate between different types of learners as to whether they can be considered the type of data-collector, grammar-orientated learner... something similar to memorizer or even 'contraster' have led to liberal pluralism of language-teaching methods. In the field of therapeutic EA, advice will range from cognitive to drill suggestions⁶⁾. (6) Since errors are very often due to lack of motivation among students, all kinds of motivating factors have to be taken into consideration. Some of the most important ones are: (a) More lenient attitudes towards students' errors according to principles mentioned above. (b) The teaching and use of all kinds of legitimate simplifications (marked vs. unmarked forms, clearer distinctions between active and passive competence,

4) Cf. e.g. Nickel, G., "Contrastive Linguistics—Error Analysis—Interlanguage: Implications for Foreign Language Teaching." In: Lohnes, W. F. W./Hopkins, E. A. (eds.), *The Contrastive Grammar of English and German*. Ann Arbor, 1982.

5) For a particular experiment in communicative studies cf. Palmberg, R. "Non-native judgments on communicative efficiency." In: *Interlanguage Studies Bulletin*. Vol. 6, No. 1/1981/82, 79-92.

6) For different views cf. Krashen, S., "The 'Fundamental Pedagogical Principle' in Second Language Teaching." In: *Studia Linguistica*. Vol. 35, No. 1-2/1981, 50-70.

LSP, threshold level, etc.)⁷⁾.

The IL phenomenon has raised a great deal of controversial issues. There is still the problem of describing this transitory and dynamic learning principle and also its applicability to didactic frameworks. The protagonists of this psycholinguistic principle, such as S.P. Corder, for example, seem to associate the phenomenon of IL with certain factors⁸⁾. While the more recent idea of IL is the one of climbing from less complicated levels to more complicated ones and ending up sometimes on fossilized levels, one should also accept the other possibility of simplifying IL on a performance level, thus establishing the dichotomy 'IL of competence vs. IL of performance'.

Apart from the factors mentioned above, another important one is the question of 'norm' in all its dimensions. As we all know, there are contradictory views among native speakers⁹⁾. But there is also the problem of the learner's norm within his communicative competence. One should be reminded here that all linguistic models are primarily concerned with mother-tongue problems. It must be seriously questioned here also whether communicative competence from the FL learner's point of view means the same as communicative competence from a native speaker's point of view. In connection with international communication this problem would have to be analyzed in detail. What we need is very precise and empirical work of the socio- and pragmlinguistic type to inquire into this highly important and subtle field.

While the native speaker with varying degrees of tolerance, which may even differ from country to country, expects foreigners to commit errors as a sociolinguistic warning signal¹⁰⁾, there is another only superficially contradictory aspect to be also taken into consideration: RP and Standard English are still expected as the most neutral form from non-native speakers. Uses of intimacy of the stylistic or lexical type, including regional dialects and substandards, are slightly frowned upon and should only very tactfully be used. Thus we have the interesting phenomenon that on the one hand over-correct and too-good-to-be-true English has a slightly freezing effect raising high expectations, but

7) For applications to the designing of teaching materials cf. e.g. Lander, S., "Towards a reformulation of interlanguage in the light of work on pidginisation and ethnic minority in English in Britain." In: *Interlanguage Studies Bulletin*. Vol. 6, No. 1/1981/82, 56-77.

8) Cf. e.g. Corder, S. P., "The Study of Interlanguage." In: Nickel, G. (ed.), *Error Analysis*. Stuttgart, 1978, 37-63 and also Corder, S. P./Roulet, E. (eds.), *Actes du 5ème Colloque de Linguistique Appliquée à Neuchâtel, 20-22 mai 1976: The notions of simplifications, interlanguages and pidgins and their relation to second language pedagogy*. Genève, 1977.

9) For interesting observations in this field with excellent bibliographical data cf. Preston, D. R., "Sociolinguistics and language teaching and learning." To be published in a special volume of *IRAL* in 1984. Cf. also the most recent presentation of the problem, Janicki, K., *The Foreigner's Language in a Sociolinguistic Perspective*. Poznań, 1982. (Seria Filologia Angielska Nr. 17.)

10) For a different opinion on the level of phonetics, cf. Leahy, R. M., "A Practical Approach for Teaching ESL. Pronunciation Base on Distinctive Feature Analysis." In: *TESOL Quarterly*. Vol. 14, No. 2/1980, 217.

on the other hand the use of intimate signals of all kinds is also an area to be entered only with great care. It is interesting that today the norm question is attracting interest from all quarters. One can clearly notice the trend towards accepting all kinds of 'Englishes' all over the world¹¹). All these factors should help us in re-evaluating teaching and learning objectives in a more realistic way without giving up certain levels of standards.

11) Kachru, B. B. (ed.), *The Other Tongue: English Across Cultures*. Champaign/Illinois, 1982. Cf. also *World Language English*. Vol. 1, No. 1/1981. Quirk, R., "International communication and the concept of nuclear English." In: Brumfit, C. (ed.), *English for International Communication*. Oxford/New York, 1982, 15-28.

The Process Based Model of Language

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The process based model of language connecting research in CL and AI with Psycholinguistics and Cognitive Science is concerned with four distinct objects:

1. with the *natural language representation*, which is a set of one dimensional chains of discrete signs $R_1 = \{\alpha_1, \alpha_2, \dots\}$. It has been attempted to enumerate the set of valid sentences in R_1 by a formal grammar, familiar from mathematical linguistics $G(S, T, T_n, R)$.
2. with the *non-linear, mental (internal) representation* of the world: $R_2 = \{\alpha'_1, \alpha'_2, \dots\}$. Mental representation is related to language representation and it is customary to explicate mental representation in structural terms (discussed in more details by Bierwisch at this congress).
3. with the *mapping process from language representation into mental representation*, which is called comprehension process $(P(R_1, R_2))$. and
4. with the *mapping process in the other direction from mental into language representation*, i.e. with language production or synthesis process $(P'(R_2, R_1))$.

Processes presuppose the corresponding source and target representation, they are principally more complex, in as much as they operate in time, i.e. the execution time is an additional evaluation factor at their description.

Both representations R_1 and R_2 as well as processes P and P' can be described in algorithmic form. Algorithms describing R_1 and R_2 state conditions of well-formedness; they determine merely whether a given string belongs to a given language or not. Algorithms constituting the processes P and P' translate structures from one level to the other, i.e. they describe the way how a given string α of R_1 is transformed into α' of R_2 . Algorithmic descriptions imply procedures, i.e. they correlate not only initial and final states to each other, but they also define a particular sequence of using grammar rules to obtain a derivation. In view of grammars the inherent dynamicity of algorithmic description remains "idle run", i.e. the rule sequence implied by the algorithmic description is irrelevant, it is a sort of overspecification, e.g. no element of reality is reflected by the fact that in expanding a grammar rule like $S \rightarrow NP VP$, although arriving at the same final structure, the algorithmic description requires the decision to expand either the NP-node or the VP-node first.

The algorithmic description of processes on the other hand makes use also of the implied rule sequencing. In a process based model the algorithmic description of language has no "idle run", or overspecification; the sequence of the rule-application is object of inquiry, just as in a purely representation

oriented description the shape of rules is the object of inquiry.

Linguistics (traditional and modern) is concerned with relating the two representational levels (R_1 and R_2), each of which can be regarded as a set of structures in timeless abstract space. Language comprehension and production (P and P') on the other hand was considered as the domain of psycholinguistics, i.e. they were considered to lie outside of the scope of linguistic proper.

While it has been generally presumed that language comprehension and production processes depend on representation, the interdependence in the other direction (i.e. dependence of representations on processes) has been studied only occasionally (Batori 1981).

The point is that there are a number of structural properties of natural language, which have no representational justification, i.e. there are language phenomena which cannot be reduced ultimately to some objects or propositions mirroring reality, such as word order variants, or active-passive sentence pairs and the like.

One of the representationally not motivated language devices is pronominalisation. Pronouns and pronominalisation are treated in representationally based linguistics typically in terms of distributional privileges or constraints.

The representationally based model does not explain, however, the uncertainty of intuition in selecting pronouns and does not answer the questions: Why are pronominal references easier to be understood than nominal references (cf. Keenan 1976 : 308) and why do languages use pronouns at all?

The process based model of language avoids these difficulties: Pronouns are considered primarily as instruments of reference assignment in the mapping process of $P(R_1, R_2)$ and $P'(R_2, R_1)$.

Technically speaking the process based model of language relies on structured memory, in which—among others—discourse objects are stored. In order to keep apart old and new discourse objects (in the sense of Daneš (1974) and Sgall (1973)), Discourse memory (which is needed also on independent grounds for the adequate treatment of definite noun phrases) allows to utilize pronomina: 1. pronouns provide a handy abbreviation system for longer names (cf. Smaby 1978), 2. and probably more important: they allow to avoid repetitious analysis and 3. as a side effect by referring to objects in discourse memory instead of noun phrases in sentences directly the danger of infinite recursion (Bach-Peters-Paradox, cf. Karttunen 1971) can be avoided: pronouns are treated like variables.

The pronominal assignment operates with the following assumptions:

1. Like comprehension process in general pronominal assignment operates from left to right, sentoid by sentoid.
2. There is only one interpretation delivered. In case of a wrong interpretation a consciously guided new analysis will be necessary.

The basic assignment rule is the following:

- (R1) Within a sentence frame S assign referents to the noun phrases as they

occur in the left to right linear order: C_i, C_{i+1}, \dots

(R2) If there is no possible referent for the pronoun in C_i , leave it temporarily unassigned and proceed with the next sentoid C_{i+1} .

(R3) The pronoun in sentoid C_i can be bounded, if in the succeeding sentoids C_{i+1}, C_{i+2}, \dots a noun phrase within the minimal domain of the pronoun has been found (in the sense of Reinhart 1976: 183, which implies also the configuration in which the noun phrase both precedes the pronoun and it is in a higher clause, cf. Langacker 1969).

(R4) Otherwise the pronoun must either refer to an entity already in discourse memory (i.e. introduced prior to C_i) or it is entirely new and must be entered (as semantically defect) new entry at this point.

Within these frames four possible strategies of pronominal assignment are conceivable (Bátori 1981: 131-140):

Table 1 Strategies of Pronominalization

Illustrative example like	A ↓	B ↓	C ↓	D ↓
(1)	+	+	+	-
(2)	+	-	+	+
(3)	+	+	-	+
(4)	-	+	+	+

The optimal configuration is (3) in which the noun phrase both precedes and governs the pronoun. Consider:

- (1) Before John left home from the party he phoned the police
- (2) Before he left home from the party John phoned the police
- (3) John phoned the police before he left home from the party
- (4) *He phoned the police before John left home from the party

In (1) the noun phrase precedes the pronoun, however, the noun phrase is in a subordinated clause. The crucial example is (2) in which the pronoun precedes the noun phrase referred and it is in a "lower" clause than the noun phrase itself. The pronominal reference is retained unresolved in the memory as long as the succeeding main clause has been processed, during which the bounding noun phrase must be detected. In sentences with pronouns preceding in a superimposed main clause like (4) the pronoun cannot be correferential with the following noun phrase (for further details cf. Bátori 1980).

The empirical consequences support also the validity of strategy A, which can be stated as follows: The resolution of pronominal references proceeds from left to right and top to bottom.

The process based model of language allows to study the interconnection

between representations and processes, i.e. to formulate strategic rules in comprehension process beyond the power of current representational models. The semantically hollow yet in natural languages wide spread syntactic categories such as subjects, extraposition etc., apart from pronominalisation, can be given a natural explanation in the procedural framework (Batori 1981). Empirical studies within the process model suggest a new view of grammar.

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Demisyllabic Synthesis by Rule Using Lingua

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Demisyllabic synthesis experiments in American English and Japanese are reported on. *Lingua* has been improved using a new duration adjustment scheme in combination with the event-marking dictionary. The Japanese version experiments with a new scheme for preservation of pitch fluctuation patterns in the natural utterances of source syllables due to consonantal effects.

This paper reports on the current status of one version of speech synthesis by rule experiments being conducted at Bell Laboratories, Department of Linguistics and Speech Analysis Research. This scheme is based on the concatenation of syllables, phonetically stable and relatively independent units. In order to reduce the size of segment inventory, we decompose each syllable into an initial demisyllable, which exhibits consonantal characteristics and the transition into the vowel, and a final demisyllable, which includes the stationary portion, if any, of the vowel, the transition into the consonant, and, in most cases, all the consonantal characteristics of the final cluster. In certain cases (word finally) phonetic affixes are attached to the syllable core (i.e. initial and final demisyllables) [Fujimura 1976.]. The word 'band', for example, is decomposed into the core /bæ n/ and the phonetic affix /d/. The core consists of the initial demisyllable /b(æ)/, and the final demisyllable /æ n/.

This results in an inventory of 850 items in American English [Lovins *et al.*]. The items are excised from natural speech, and are parameterized using linear prediction coding (LPC) [Atal and Hanauer] as the means of representing the spectral information of the speech signal. In order to produce speech, these LPC-encoded demisyllables are concatenated and adjusted using a computer program called Lingua [Browman].

The input to Lingua consists of a string of syllables in phonetic coding (ARPAbet), plus stress markings and boundary indications. A preprocessor breaks the syllables up into the appropriate demisyllables. Then a rule interpreter determines which rules apply to the input string. It refers to a set of rules supplied by the user, as well as to a dictionary of information about the demisyllables [Macchi]. There are four kinds of rules. Pitch is determined according to the stress mark by an early version of Pierrehumbert's pitch program [Pierrehumbert]. The amplitude is a simple transform of the pitch contour, except low amplitude portions which are copied from the original stored patterns. The only explicit smoothing occurs between vowels and glides.

There are a variety of duration modification rules, all of which are compressional. Three major compressional factors, operate on the syllable level

(or higher): stress, phrasal position, and number of syllables in the word. Thus, syllables are compressed depending on their stress level—for example, a syllable with secondary stress is compressed to 95% of its stress duration. In addition, syllables in non-phrase-final position are compressed by 90%. Non-word-initial syllables are compressed by a factor that is dependent on the number of syllables following in the word. These factors are multiplied together, as appropriate, to arrive at the final compressional factor for each syllable.

There are two additional factors that determine the susceptibility of any demissyllable in a particular environment to the compression forces of the environment. The first factor is the Linguistic Control Structure (LCS), which depends, among other things, on the phonetic identity of the demissyllable. For example, final demissyllables containing voiceless stops are relatively compressible. In addition, some demissyllables including these have a weak LCS, which means that there is another factor that causes compression of particular parts of the demissyllable. For example, initial voiceless stops generally have incompressible articulatory closure period, and therefore, most of the specified demissyllabic shortening occurs within the vowel and aspiration.

Synthesis of Japanese using the same demissyllabic principle is proceeding. Ultimately, a Japanese inventory will be combined with a slightly modified version of Lingua. One new method with which we are experimenting in the Japanese synthesis is the preservation of natural pitch fluctuations that are inherently related to the syllable-initial consonantal characteristics. The pitch value for each syllable is represented by that at the end of the initial demissyllable, and this value is adjusted according to an exponential pitch declination scheme. For the final demissyllables a linear interpolation rule connects the end of the preceding initial demissyllable to the beginning of the following initial demissyllable.

Pitch contours, for Tokyo dialect, are generated as ramp functions implementing the accentual drop for marked syllables, phrase-final drop, and phrase initial rise unless the initial syllable is long or accented [Fujimura 1972]. Two phrase types are used. A major phrase boundary resets the pitch value to a starting point, followed by an exponential declination toward a fixed bottom. A minor phrase defines an accentual domain, in which at most one accentual drop can occur.

The duration adjustment rules are almost nonexistent at the moment, except for a simple phrase-final elongation rule. Some simple vowel devoicing measures are taken. Amplitude information also retains the original fluctuation in the source material. There are slight differences in smoothing processes between Japanese and English. [Tape demonstration was given in the meeting in English and in Japanese.]

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Computer-based Information Retrieval by Means of ILo Summaries

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Introduction

The problem of documentation and retrieval of information is concerned chiefly with information coded in the form of texts, even though these texts occasionally include formulae, illustrations and tables. One of the fundamental problems of automatic documentation is therefore translation of such texts. During translation the language of the text is presumed to be an arbitrarily chosen code which can be varied or even completely replaced at will. After all, what is to be stored in a documentation-system for subsequent use is almost exclusively semantic information. Neither the biological nor the historico-cultural role of the language used is of any relevance to this documentation. The communicative (semantic) role can therefore just as well be fulfilled by an interlinguistically planned language, e.g. ILo. But does this offer any advantages? The (more or less) complete absence of idiomatic expressions in ILo makes it more suitable for automatic (computer-based) processing than any ethnic language.

One such system is PREDIS (German: Plansprachliches REchner-DIalog-System, or in ILo: Planlingva RETrova DIalog-Sistemo) which has been developed in FORTRAN IV in Paderborn, Germany between 1975 and 1981 and adapted in Budapest, Rio de Janeiro and Timisoara, Rumania. ILo (since 1980 one of the working languages of the Association Internationale de Cybernetique) is a "modular" (agglutinative) language without grammatical exceptions. This facilitates algorithmic semantic analysis of texts coded in it.

How does PREDIS work?

The PREDIS-programme is capable of answering two types of questions:

- (i) questions concerning a person, e.g. an author, a scientific collaborator or a learner. The user enters the name of the person concerned and receives all the information stored about him.
- (ii) questions about a certain topic. If one is looking for documents pertaining to a certain subject or for a list of persons who are interested in a certain publication or conference, one enters certain characteristic key-words. To make the question more simple and more widely applicable, the computer does not process the words themselves but only the radicals contained in them. As a foolproof programme for dissecting words into radicals hasn't yet been devel-

oped, the user must enter his words already separated into radicals, affixes and grammatical endings. The modular structure of ILo makes this possible. As the computer cannot distinguish between different types of morphemes, it simply ignores all those having less than three letters. This is the most common cause of "noise" in this system as ILo contains several three-letter prepositions and affixes in addition to a large number of "significant" three-letter radicals. After receiving the entry thus separated into radicals, the computer checks if at least two thirds of the descriptors (morphemes with three or more letters) appear in any of the texts stored in it and prints out the titles of the texts it finds. A complete text may also be requested by the user in any particular case.

As for the actual documentation, we receive at our institute articles on education written in various languages. These are accompanied by an author's summary. This summary is then translated into ILo with the help of a multilingual dictionary compiled at our institute for this purpose. This ensures a standardized terminology and thus reduces the chances of a "miss". The translation is then entered into the data-bank with separated morphemes. For example the word *komputilo* (=computer) is entered as *komput-il-o*. The addition of a translation programme enables the user to obtain the summaries requested by him in a language of his choice. An ILo-German programme called SUSY developed in Saarbrücken is at present being tested for this purpose. (fig. 1)

A further refinement of this system is the Aktives Dokumentations-System (ADS) which becomes increasingly important as the library of stored summaries increases in size. In this case every new summary entered into the system is automatically compared with the summaries already stored in it. The basis of this comparison is the theory of "fuzzy" subsets and the Hamming-distance defined in a space consisting of such elements. The author of the article just entered is informed of available articles that may be of interest to him. (fig. 2)

Until now we have tried out this system on documents about following subjects:

- A—teaching by computer
- B—discussion about the development of science
- C—international languages
- D—reactions of teachers and learners by computer
- E—software
- F—choice of planned language
- G—teaching of law at university
- H—teaching of German

Experiments showed that I. the use of two descriptors leads to widely differing results. No general conclusion is possible.

II. The use of three descriptors almost always causes noise.

III. The use of four descriptors lessens the recall-capability because of the

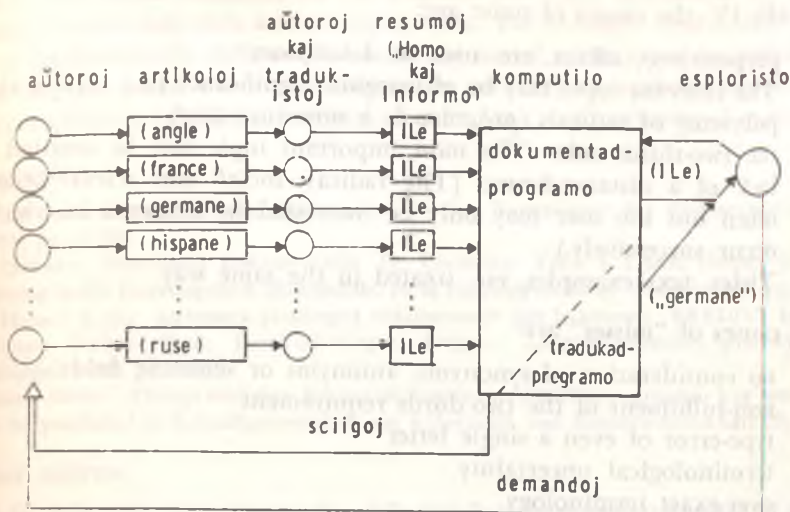


Fig. 1 Documentation by PREDIS (traduki=to translate; esploro=research)

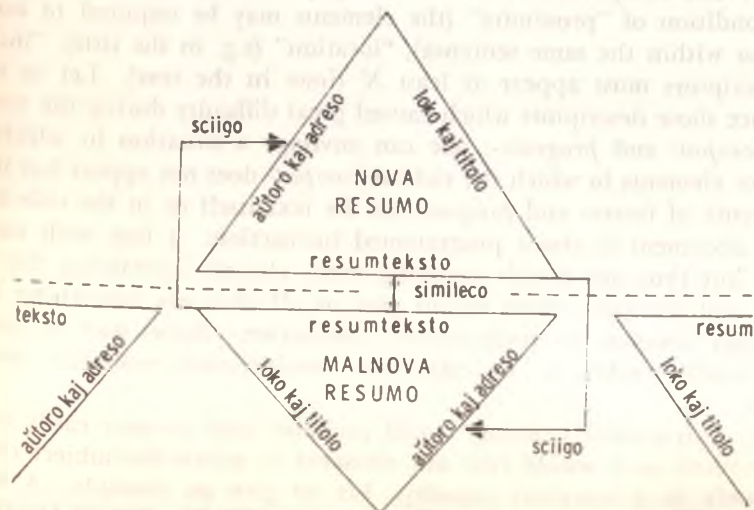


Fig. 2 ADS (Aktives Dokumentationsystem) (sciigo=indication)

2/3 rule, still this leads to increased precision.

Specifically IV. the causes of noise are:

- (i) prepositions, affixes, etc. used as descriptors
- (ii) The relevant topic may be of marginal significance in a certain element.
- (iii) polysemy of radicals (*program-* is a notorious case)
- (iv) the two-thirds rule—The most important topic may be omitted.
- (v) lack of a distance-bound (The radicals *social-* and *scienc-* occur very often but the user may only be interested in elements in which they occur successively.)
- (vi) Titles, text examples, etc. treated in the same way.

and V. causes of "misses" are:

- (i) no consideration of synonyms, antonyms or semantic fields
- (ii) non-fulfilment of the two-thirds requirement
- (iii) type-error of even a single letter
- (iv) terminological uncertainty
- (v) over-exact terminology

Concluding remarks concerning search-methods

We have seen that the 2/3 rule can cause noise and also hinder retrieval. It is necessary to realize that the 2/3 rule cannot replace the logical OR as we have (X AND Y) OR (X AND Z) OR (Y AND Z) and not X AND (Y OR Z). The result obtained by the two-thirds rule can be interesting if it is applied in conjunction with a "weighted" search, i.e. in the search hypothesis using three descriptors one accepts elements containing just two of these but under the further condition of "proximity" (the elements may be required to occur successively or within the same sentence), "location" (e.g. in the title), "incidence" (both descriptors must appear at least *N* times in the text). Let us examine for instance those descriptors which caused great difficulty during the test-phase: *instru-*, *komput-* and *program-*; one can envisage a situation in which one is looking for elements in which the radical *komput-* does not appear but in which the occurrence of *instru-* and *program-* in the text itself or in the title indicates that this document is about programmed instruction: a link with computers can exist but thus one avoids receiving every element containing the radicals *program-* and *komput-* which would give us all elements concerning software without any relation to programmed instruction. Some sort of structured samples would enable us to take into consideration synonyms and near-synonyms.

Finally a structured summary would probably lead us more easily to an appropriate result as it would rule out elements in which the subject concerned appears only in a marginal capacity. Let us give an example. A summary structured under the headings PREMISE, CONTENT, CONCLUSION, METHODOLOGY would enable the system to distinguish between these parts dur-

ing the search. So if one has word the *komput-* under the heading **METHOD-
OLOGY**, one could eliminate in the response all those summaries in which a
computer is used only as a test or application. For example instruction by com-
puter is fundamentally different from say the use of a computer to evaluate test-
papers. At this point we must mention the necessity of further intensive effort
in the compilation of a thesaurus and of rules for the structuring of summaries.

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A Model of Automatic Analysis of Time-Related Expressions in Japanese

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I. Introduction

This paper discusses an algorithm for a model of automatic analysis of the meaning of tense and aspect in Japanese sentences at the pragmatic level. It is generally understood that there is no one-to-one correspondence between the grammatical expressions of time in sentences of natural languages and the extra-linguistic time of situations. If there is no correspondence, it is impossible to simulate the process of analyzing linguistic expressions to determine their meaning, which is needed for machine translation. This paper introduces an algorithm of the automatic analysis of Japanese time expressions by introducing the notion of focus and by examining the phenomena on the pragmatic level.

II. Meaning

In this model, the meaning of time expressions (tense and aspect) is determined in terms of extra-linguistic phenomenon and focus of the speaker. Extra-linguistic situations are divided into two categories; dynamic and static. The former is the one that is conceived of as being unchanging throughout its

Diagram 1

EXTRA-LING. PHENOMENON	FOCUS	ASPECT
DYNAMIC	BIGGER	PERFECTIVE
DYNAMIC	SMALLER	IMPERFECTIVE
STATIC	SMALLER	STATIVE

Diagram 2

TENSE	ABSOLUTE	RELATIVE
BASE	Speaking time	Main phenomenon
Before	PAST	ANTERIOR
Same time	PRESENT	SIMULTANEOUS
After	FUTURE	POSTERIOR

duration, while the latter is the one that has any of several temporal contours (Lyons, 1977).

'Focus' of the speaker is defined here as the period along the extra-linguistic time line upon which the speaker directs his attention. The meanings of tense and aspect reflecting the relationship between extra-linguistic phenomena and the speaker's focus, are shown in Diagrams 1 & 2.

III. Japanese Time-Related Expressions

In Japanese time-related expressions, there are two kinds of contrasts in form: one with *-tei-* and without it, and the other with *-(r)u* and *-ta*. The meaning of the combinations of these forms are shown in the following diagram.

Diagram 3

TENSE ASPECT	ABSOLUTE TENSE			RELATIVE TENSE		
	PAST	PRESENT	FUTURE	ANT	SIMUL	POST
PERFECTIVE	ta	○	ru	ta	○	ru
IMPERFECTIVE	teita	teiru	teiru	○	teiru	○
STATIVE	teita ta	teiru ru	teiru ru	○	teiru ru	○

Since the same forms share different meanings, an automatic semantic analysis in terms of forms is impossible. However, about 800 sentences actually used in novels, essays and travelongs, which were analyzed for this paper, show the meanings are determined almost unambiguously.

Some verbs do not have some of the above forms, and some types of clauses restrict the appearance of the above forms. And on the pragmatic level, semantic ambiguity disappears due to the appearance of particular word classes within the sentence.

IV. Tense

In Japanese, absolute tense and relative tense share the same forms in many cases. However, in most cases the types of clauses in which particular tense forms appear determine which type of tense. The following is an algorithm to determine the meaning of tense in terms of clause:

ANALYSIS OF TENSE

- [I] [TEITA] PAST
- [II] 1. Clause with *tekara*, *teirai*, *ato*, *totan* ('after') ANTERIOR
2. Clause with *mae*, *izen* ('before') POSTERIOR
3. Clause with *aida*, *uchi* ('while') SIMULTANEOUS

4. Conditional clause with *ba*, *to*, *tara*, *temo*

「TEIRU」	SIMULTANEOUS
other	ANTERIOR
5. Main clause, quotation and clause with *ga* ('but')

.....	(A)
-------	-----
6. Clause with *toki*, *koro* ('when'): relative clause with nominalizers *no* and *koto*

.....	(B)
-------	-----
7. Conditional clause with *nara*, *toshitara*, *node*, *kara*, *taine*, *noni*

a. 「RU form」 in main clause	(A)
b. 「TA form」 in main clause	(B)
8. Relative clause modifying noun

a. Time adverb in the clause	(A)
b. other	(B)

(A) 「TA form」	PAST
「RU form」	Non-PAST
(B) 「TA」	ANTERIOR
「TEIRU」	SIMULTANEOUS
「RU」	POSTERIOR

V. Aspect

Types of verbs have much to do with the meaning in aspect. Kindaichi (1955) classifies the Japanese verbs into the following categories:

- 1) Stative verbs
- 2) Durative verbs
- 3) Punctual verbs
- 4) the fourth type of verbs.

The verbs of the first category such as *iru* '(animate) to be', *aru* '(inanimate) to be', *dekiru* 'to be able' do not take *-tei-* forms. The verbs of the fourth category such as *sobieru* 'to be sky-high' and *magaru* 'to bend' always appear as *-tei-* forms. The durative verbs with *-tei-* usually indicate the continuation of an action, while the punctual verbs with *-tei-* usually indicate the state resulting from an action.

However, the durative and temporal verbs with *-tei-* may indicate the opposite situations, respectively. Therefore, we need to further analyze the verbs.

By analyzing the data, we found that some verbs never indicate the state resulting from an action, and that some other verbs do not indicate the continuation of an action. The following is an algorithm for an analysis of aspect in terms of verb categories.

ANALYSIS OF ASPECT (-teiru)

MEANING		e.g.
Type of verb	with -tei-	without -tei-
STATIVE	STATIVE <i>iru</i> 'to be'

INGRESSIVE	STATIVE	}	PERFECTIVE	deru 'to get out'
PROCESS	IMPERFECTIVE			omou 'to think'
PUNCTUAL	STATIVE			tsuku '(a light)
	(IMPERFECTIVE)*			to be on'
DURATIVE	IMPERFECTIVE			yomu 'to read'
	(STATIVE)**	}		
PUNCTUAL	IMPERFECTIVE***			saegiru 'to cut off'
DURATIVE	STATIVE****			

- * with *tokoro*- or particular adverb (e.g. *dondon* 'one after another')
- ** with particular adverb (e.g. *takusan* 'much')
- *** with animate subject
- **** with inanimate subject

VI. Conclusion

In order to incorporate the above algorithms in a model of tense and aspect analysis, we have to mark the verb categories as well as temporal adverb classes in the dictionary, while types of clauses can be analyzed in terms of conjunctives. By this algorithm more than 90 per cent of sentences actually used can be analyzed successfully.

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Hierarchic Class Networks: A Diagrammatic Formalism for Natural Language Modelling

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Abstract. A diagrammatic formalism for conceptual modelling of natural languages called hierarchic class networks (HCN) is discussed. The formalism is intended to serve as a general purpose tool and methodology for linguistic systems analysis and as a framework for formal representation and manipulation of language and knowledge structures. It is believed that large and complex systems like human languages do not lend themselves to concise description without a sound theory of hierarchic structures, which is yet to be developed. In addition, it is necessary to develop intuitively and perceptually efficient and practically convenient means of representation in order to enable one to manage complex system descriptions cognitively.

The formalism presented is capable of capturing complex aspects of structure such as syntagm, paradigm, context, recursion, context dependence and ambiguity, relations of relations etc. The various types of topological connections possible in hierarchic and recursive class graphs are analysed and defined whereby the formalism itself is used as the vehicle of description. Binary connections are then extended to conceptual i.e. typed binary relations, higher order binary relations and general n -place relations that can be defined in terms of classes.

Being a linguistically oriented and natural like in its appearance the formalism should allow linguists, systems analysts, and specialists of various fields of knowledge to work in terms of concepts intuitive and familiar to them. The formalism superimposes an orderly, systematic working discipline, that yields formally well defined descriptions that can serve as specifications for system implementation. The formalism is aimed at bridging the gap that presently prevails between formal mathematical treatment, informal linguistic description, and programming and thus purports at bringing linguistics closer to an exact science and an engineering discipline.

1 Introduction

Graphs have the advantage that they can be represented as tangible diagrams, whereby abstract structures and their properties become intuitively perceivable. This is of great importance in the study of large and complex systems such as natural languages, whose conception by symbolic means alone is very difficult. A graphical representation can serve as a kind of cognitive aid and an intermediate language between abstract structures of mathematics,

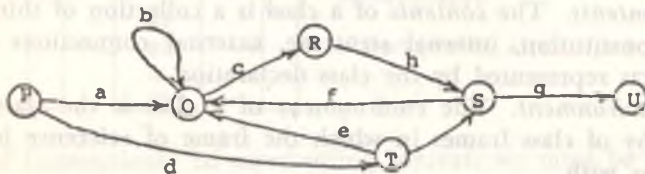
linguistic analysis, practical systems engineering and programming, and artistic visualisation.

2 Classes and Class Graphs

The underlying mathematical concepts cannot be discussed here. Only the very basic definitions are stated.

Elements, Sets and Classes. A set is a collection of some symbolic objects called *elements* of the set. A set that has another set as an element is called a *class*. More generally, a class is an entity that has sets or other classes as its elements. Thus, by definition a class is a hierarchical and potentially recursive object. We assume, however, that any recursion is of finite order.

Nodes, Arcs and Graphs. A graph $G = (N, E)$ is a binary relation E defined on a set N . The elements of N are then called *nodes*. The elements of E are pairs of elements of N i.e. of the form (a, b) and called *edges*. Whenever direction has meaning, i.e. (a, b) is unequal to (b, a) the edges are called *arcs* or *arrows*. Graphs are represented graphically by line diagrams, whereby the location and form of nodes and lines are of no significance other than practical or perhaps aesthetic.



Class Graphs and Networks. A class graph is a graph, whose nodes are classes or class graphs. The definition is again recursive. Thus, a node in a class graph may contain a class graph. Moreover, arrows may connect not only class nodes of one and the same level, but may also pass over from one level to another. The typology of various possible topological connections will be treated in detail below. The fact that interconnections can pass from one level of hierarchy to another makes the concept of class graph *hierarchical* and potentially *recursive*. A class graph that has symbols associated to its nodes and or arcs is called a *network*.

3 Representing Classes

Class Frame. Classes are represented graphically and symbolically as frames. The formalism itself is used to explicate its own concepts. A frame is an object that has the following appearance, structure and imports:

Class: Frame

Has: Name, Boundary, Contents,
Environment and Relations

Graphically a class is represented as a frame contour enclosing its *internals*, i.e. the specifications of its imports.

As with graphs the size, form and location of frames have no other significance than practical and perhaps aesthetic. The frame size should, however, always be large enough to hold the necessary subclasses, symbols, arrows etc. needed for its description. The internal structure and the external connections in any description should be defined to the level of detail required in the current context or perspective. For practical reasons rectangular frames are normally used.

As in programming, it is recommended that no bigger frames than what can be perceived and managed mentally at one time be used. Thus an A4 page size or a screenful would be maximum frame size. This can be achieved by adjusting the conceptual and symbolic working levels accordingly. Appropriate means of abstraction, naming, referencing, perspecting, filtering etc. as used in systems engineering and programming are applied.

Naming Classes. For identification and referencing purposes classes are given *names*. The name is usually placed in the upper left corner of the frame. In itself the name has no significance. Its sole purpose is to allow identification and referencing of the entity represented by the frame from other contexts.

Class Contents. The *contents* of a class is a collection of things that define the type, constitution, internal structure, external connections and functions of the objects represented by the class declaration.

Class Environment. The *environment* of a class is the class structure i.e. the hierarchy of class frames in which the frame of reference is contained or has relations with.

4 Structural Relations of Class Networks

Aspects of Structure. In describing natural language structures it is necessary to be able to cope with the following aspects of structure:

Class: Aspect of Structure
Is: Syntagmatic, Paradigmatic,
Hierarchic, Contextual,
Conceptual or Computational

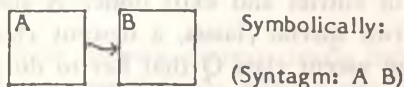
It is quite difficult to capture all of these in one uniform formalism.

In the following the standard diagram representation conventions are extended to allow more aspects to be represented in a uniform graphical notation. Symbolic notations are also given. Let us first consider *binary relations*.

Types of Links. In hierarchic class graphs there is a rich topological typology of links. The type of an arrow is determined by the mutual position of the connected frames in a class hierarchy. The following types of connection are distinguished:

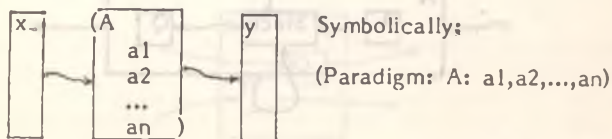
Class: Link Has: Internal, Conventional, External Hierarchic and Recursive

Syntagmatic Connections. A *syntagmatic* connection, i.e. a concatenation or juxtaposition of elements is denoted by an arrow as with simple graphs:



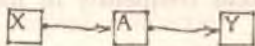
The syntagmatic representation corresponds to sequential composition and representation of graphs by enumerating their links or chains.

Paradigmatic Connections. The *paradigm* of an entity in a syntagmatic position, say A in xAy, is represented by a class frame:



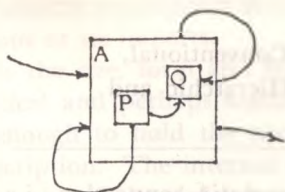
Contextual Connections. In representing *contexts* we must be able to cope with not only regular and context free, but also with context sensitive constructs. Suppose we are to deal with the possible context class X Y of the class A in a syntagm XAY. The context class is often called *distribution* in linguistics.

Regular or Contextless Connections. The contextual connections of regular or as we call them, *contextless* languages are represented simply by concatenating the classes concerned i.e. as a syntagm:



Context Free Connections. The term context free is somewhat misleading when syntagms are concerned, while it refers to the form of the production rules allowed in their generating grammars rather than the characteristics of the resulting language strings, which as a matter of fact depend on context. E.g. the rule $A \rightarrow aAb$ produces strings, in which the number of b's depend on the number of a's.

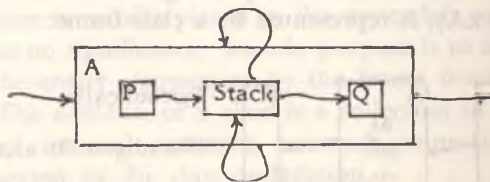
In order to allow diagrammatic description of such a dependence it is necessary to introduce a recursive arrow notation, which involves a *descending* and an *ascending* recursion loop:



Symbolically:

(Recursive: A
Has: Entry: P
Stack: A
Exit: Q)

Such a recursive class must have a push down stack mechanism, which keeps count of the number of entries and exits done. A stack may be represented by a class containing two special classes, a descent class P that recurs to the host class or exits to an ascent class Q that has to do as many recursive *exits* as the descent class has done *entries*, i.e. until the stack is empty whereafter the host class can finally be exited. Alternatively, the stack class can be represented explicitly:



Push down machines that correspond to *context free* languages can be thought of as state graphs, whose nodes (a Moore type machine) or arrows (a Mealy machine) may contain lower level state graphs, as in the ATN formalism (Woods 1970), that have to be traversed before a node can be exited or a transition made over an arrow.

Since the *order* of recursion is never known in advance, it is not possible to represent a push down machine in terms of simple graphs as a finite diagram. The introduction of the above notational conventions for recursive classes solves this problem. For related work see also (Duncal 1978, Wendt 1980).

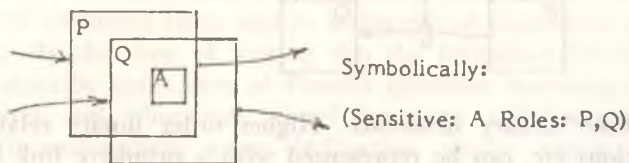
Recursive structures do occur frequently in natural languages, particularly in syntax.

Context Sensitive Connections. One of the most perplexing features of natural languages is the property of *context dependence*, which is difficult to capture in both representational and computational respects.

Ambiguity and Context. In natural languages situations occur where one and the same constituent or class of constituents may appear in a number of different contexts and have different meanings that can only be resolved with the help of the respective contexts.

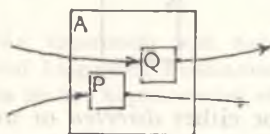
Roles and Role Classes. We say that a constituent or class then has several *roles*. The context classes or distributions as they are called in linguistics may be distinct i.e. complementary or they may partially overlap with each other.

In our formalism the various roles of a constituent or class may be represented by enclosing the ambiguous entity into a corresponding number of frames, e.g.:



Each role may then have its own context clearly indicated by its *incoming* and *outgoing* arrows. In automata theory terms a role can be interpreted as a *state* or a set or class of states called *mode* in which the entity is encountered and processed. Each role of the entity is named so as to reflect its interpretation in the respective context.

In an alternative notation the role nodes are placed inside of the ambiguous item or class frame, which is named accordingly:



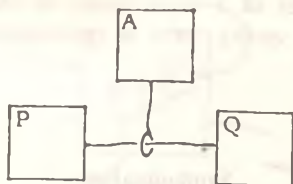
This convention may be preferable in cases, where the interpretations of various role classes are related to each other and constitute a meaningful class together.

Complex Connections. The relations and connections discussed above have been of simple type i.e. they have involved only concatenation or selection. In addition to simple types of connections it is often necessary to consider more complex types of relations that we call *complex connections* or type relations. The following types of complex connection are distinguished:

Class: Complex connection
Has: Conceptual, Higher order, Relational and Computational

Conceptual Connections. In knowledge representation it is necessary to use problem oriented concepts as *link types*. But since concepts need to be described, it is appropriate to generalize a conceptual link into a class, whereby it becomes possible to use the same formalism for the links.

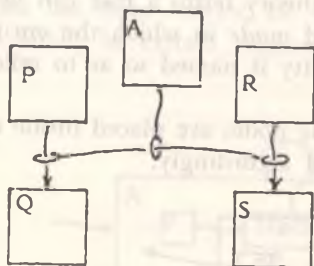
The binary character of a link imposes certain constraints on the type of classes that are applicable as links. A link class frame is represented by a frame that is connected to its link by a loopheaded link or simply *loop*:



Symbolically:

(Loop: A With: P Q)

Higher Order Binary Relations. Higher order binary relations, i.e. relations of relations etc. can be represented with a primitive link having a loop-head in both ends and called a *couple*:



Symbolically:

(Loop: A With:
Couple: (P Q) (R S))

A couple may of course be either *directed* or *undirected*, which is indicated by adding an arrowhead as normally. Higher order relations allow to do abstraction, reasoning by analogy, applications etc.

5 Classes as Linguistic Concepts

Above the class networks have been considered as a representational concept. It is now time to consider, how they relate to linguistics and knowledge engineering.

Words and Grammatical Categories. In linguistics language constituents have traditionally been classified into many kinds of categories such as word classes, cases, modes, aspects, forms, parts of speech etc. The term class has been used to refer to fairly specific things like word and grammatical classes. In the sense of the present formalism it is to be interpreted much more generally, as an abstract mathematical concept or as an epistemological one.

It should be understood as the a fundamental means that allows and is necessary to state differences between things and to recognize similarities between them, in this case linguistic and other concepts. As a mathematical concept class can be paralleled with data type as a programming concept and with word and the concept of concept itself as a linguistic concept.

In principle it should cover whatever term and linguistic concept that has been traditionally used in linguistics or that may be of inrerest in the future. Typical concepts that immediately assume interpretations as classes would be distinctive features of phonetics, phonological categories, syllabic and mor-

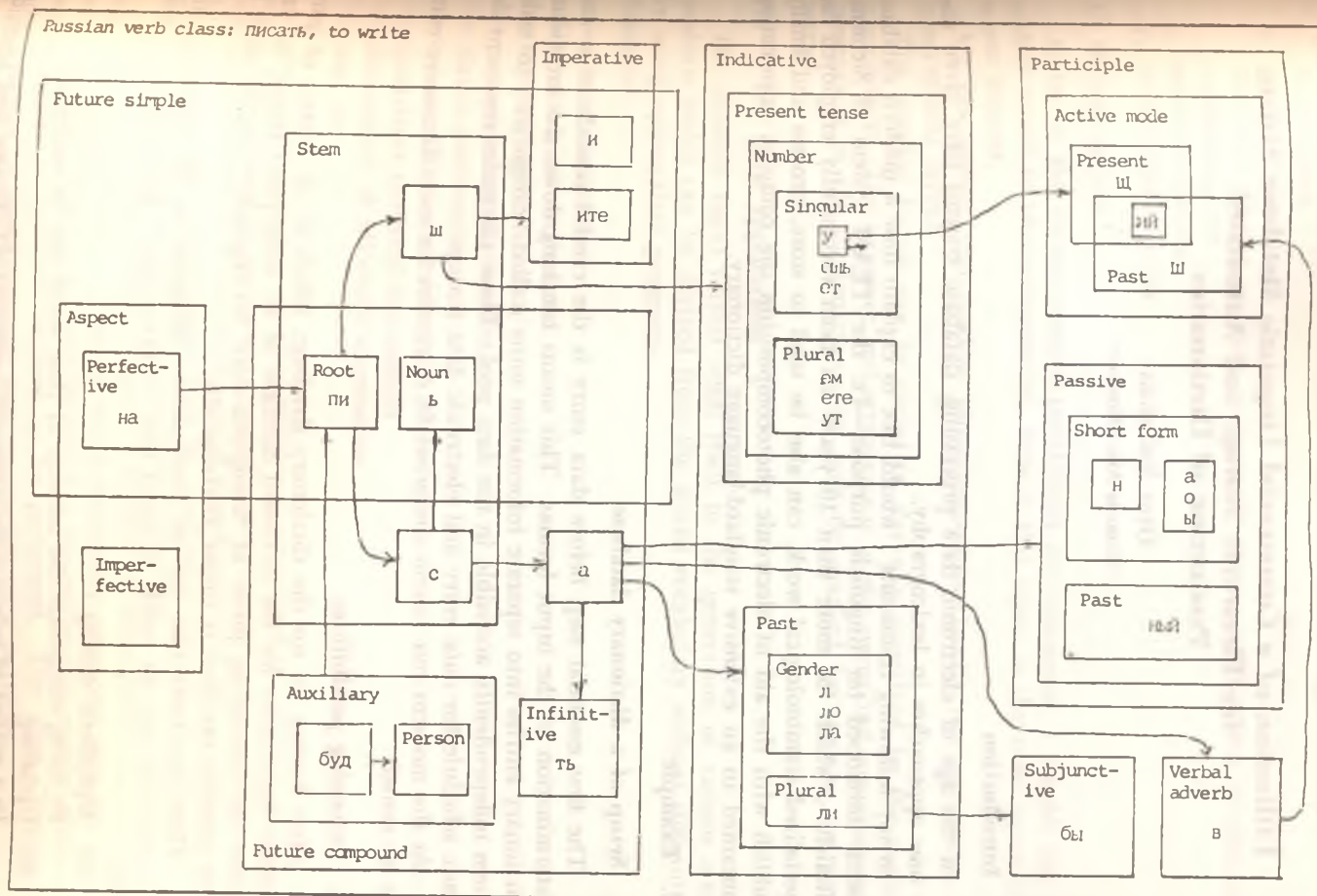
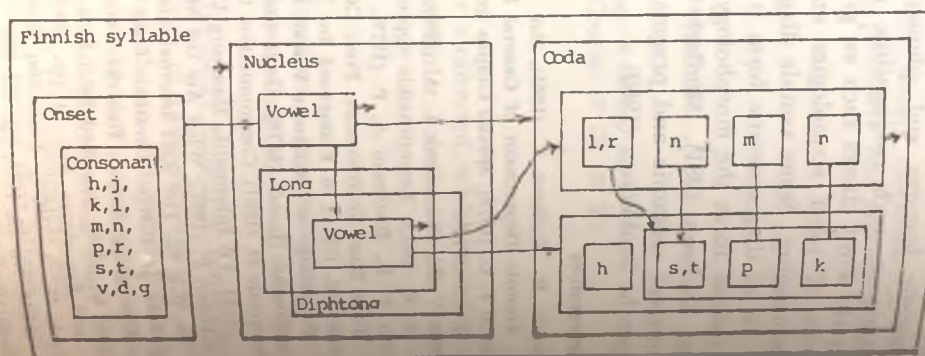
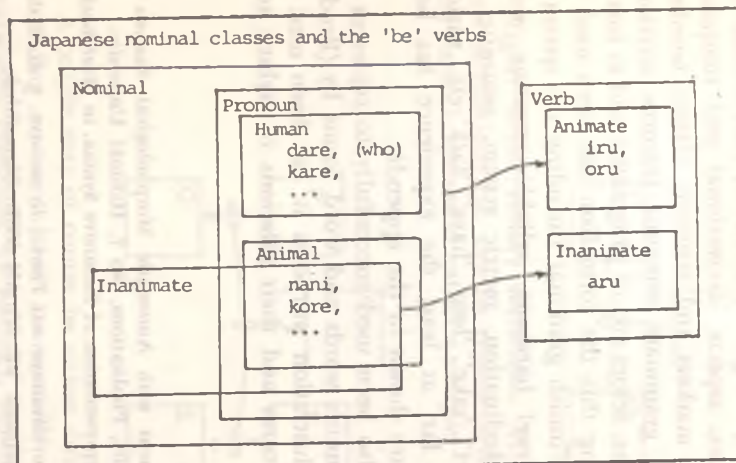
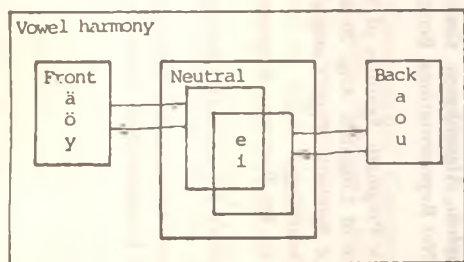
phemic constituents and classes of morphology, word classes, inflectional, conjugational, declinational classes, modes, aspects, derivational and compound classes of lexicology, phrase structure markers and syntactic classes, terminals and nonterminals of transformational grammars, semantic features, attributes and categories of semantics, roles, agents, actors etc. of pragmatics and so forth.

Conclusion. By the time of writing this the formalism has been used by the author to describe some parts of Finnish grammar including the system of phonetic features and phonemes, vowel harmony, syllable structure, word formation, verb conjugation, noun declination, metric system, semantics of certain word classes such as 'give', 'get', 'take', 'lose', 'have', 'lack' etc., various fields of word etymologies etc. So far, at least the experience has been encouraging. Some sample diagrams are shown in the appendix.

In its diagrammatic form it has also been used successfully to represent in a concise form the morphology of Finnish words and word forms by (Brodda and Karlsson 1981). Similarly for illustration purposes it has been used to represent semantic and pragmatic features and their relations in stylistic text types by Saukkonen (1982).

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Utilization of a Commercial Linguistic Database System for Electronic Storage and Automated Production of Dictionaries

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I. Introduction

In the age of electronic data processing, database systems have given rise to new approaches in lexicography.

In my following comments I would like to explain how a modern database system developed for linguistic purposes—i.e. the TEAM system of Siemens, Munich—, which for more than 10 years has been successfully employed for specialized terminological work, can also be used to store, process and finally publish with the aid of electronic photocomposition the complex information contained in an extensive standard-language dictionary.

II. Example

1. Setup of a dictionary database

The first essential step before data entry is the careful consideration and determination of the input format. This means breaking down the individual dictionary entries into separate information units (called "categories") to make them independently accessible in the data pool. These categories represent the basic schedule for data entry and electronic data storage.

At the moment our system comprises 98 categories for each dictionary entry to be stored.

2. Processing possibilities

After having set up the dictionary database, which is, of course, of paramount importance for the successful outcome of all subsequent stages, we can enter the fascinating phase of "computer-aided lexicography" which starts at the moment the data is stored electronically.

There are tasks that can be conveniently delegated to the machine, and the machine can do much to speed and facilitate work. Some of these tasks are:

a) Alphabetic sorting

By using computers the amount of time saved can be enormous.

b) Updating

In a matter of minutes the computer integrates all changes and additions

into the work and suitably rearranges the surrounding material.

- c) Error search
- d) Various other checks
- Reference checks, statistics, uniformity checks, coordination of definition vocabulary, etc.
- e) Selection options
- f) String manipulation

3. Output options

As far as the various output possibilities are concerned, I would like to confine myself to photocomposition, because this aspect is the most interesting in our context.

We can assume that modern photocomposition hardly sets any limits to typographical layout.

The printing format can be varied in any way prescribed by the authors or publishers. The sequence and arrangement of the terms and supplementary information in the dictionary entries may be changed by altering the parameters.

III. A few more applications

The detailed input format lends itself to the generation of various other products which can be derived from the stored vocabulary without difficulties by using appropriate programs.

Examples:

- reverse dictionaries
- etymological dictionaries
- rhyming dictionaries
- pronunciation dictionaries
- dialect dictionaries
- synonym dictionaries
- specialized technical dictionaries
- dictionaries of idiomatic expressions
- ... etc., etc.

A dictionary database can offer additional advantages far beyond automated sorting and typesetting processes. Generally speaking, the main advantage of a dictionary database is the multiple use of stored entries in many different ways. On the one hand, the database may be used with the aid of selection programs etc., to create several different printed products.

On the other hand, there are quite a number of other possibilities of using such a dictionary database, none of which necessarily connected with the publication of books. Information may be stored in the entries that enables semantic, morphological or syntactic analyses to be performed. The term "computer-aided analysis" is now commonly used to describe such activities.

A computer-stored dictionary might eventually play an important part in one of the integrated information systems of the future.

IV. Conclusion

I have tried to give you a short outline of the enormous opportunities a database system can offer.

One of the aims of my paper was to show that new dictionary editions can be produced more rapidly and more economically in this way and that the use of computers makes checking and control much easier and more efficient, thus assisting in the production of more reliable dictionaries.

Sprache und Musik

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1. Musik und Sprache sind aus der gleichen Wurzel entsprungen und haben außerordentlich viele Gemeinsamkeiten. Die wichtigste Gemeinsamkeit ist der Ton, die Schallwellen, die gleichermaßen die materielle Basis sowohl für Sprache als auch für Musik darstellen. Die flüchtige Natur des akustischen Phänomens bedingt, daß beide menschliche Ausdrucksformen nur in ihrem augenblicklichen Vollzug existieren. Dadurch ist eine lineare Lautkette bedingt. Eine sprachliche ebenso wie eine musikalische Struktur wird erst sichtbar, wenn der eben verklungene Ton im Gedächtnis festgehalten wird, und die noch vor Ohren stehenden Teilstücke auf die Summe der im Gedächtnis eingeschliffenen Hörerfahrungen bezogen werden. Das intuitive Erfassen der Systemhaftigkeit der Sprach- oder Musikklänge ist eine Leistung, die das menschliche Individuum jedesmal aufs neue vollbringen muß. Sie wird von jedem Individuum etwas anders vollzogen, und wahrscheinlich ist der Bewältigungsvorgang bei ein- und demselben Individuum niemals gleich.

Es ist außerordentlich aufschlußreich, daß es zahllose Definitionen von Sprache gibt, aber kaum eine autonome Definition von Musik. Jede grundsätzliche Reflexion über Musik mündet vielmehr über kurz oder lang in Metaphern aus dem Bereich der Sprache ein. Freilich sind diese Metaphern zumeist nicht als solche gemeint. Die tausendfache Rede von der *Logik* der Musik und von den *Gedanken*, die in ihr ausgedrückt sind, wird als ein Mittel schulgerechter Deskription und durchaus nicht als metaphorisch verstanden, ist aber in fast allen Fällen theoretisch unzureichend begründet. Der unzureichend fundierte Gebrauch der Termini *Logik* und *Gedanke* führt zu dem Begriff des Verstehens, mit dem die Musikwissenschaft außerordentliche Schwierigkeiten hat. Dem "Verstehen" ist im Jahre 1973 ein ganzer Sammelband gewidmet worden ("Musik und Verstehen. Aufsätze zur semiotischen Theorie, Ästhetik und Soziologie der musikalischen Rezeption", hrsg. von Peter Valentin und Hans-Peter Reineke), ohne daß es nun klar geworden wäre, was denn nun eigentlich das ist, das an der Musik verstanden oder nicht verstanden werden kann, welcher Art die Inhalte sind, die die Musik transportiert, und ob man diese Inhalte tatsächlich zu recht (und nicht nur metaphorisch) als "Gedanken" bezeichnen kann.

Im folgenden soll versucht werden sowohl den fundamentalen Unterschied zwischen Sprache und Musik herauszuarbeiten, als auch die merkwürdige Tatsache zu erklären, daß die Musikwissenschaft überall da, wo sie über eine reine Beschreibung der musikalischen Faktur hinausgeht, gar nicht anders

kann, als sich der Metaphern aus dem Bereich der Sprache zu bedienen, d. h. Musik so zu deuten als wäre sie Sprache.

2. Ein wesentliches Charakteristikum aller sprachlichen Äußerungen ist ihre *Übersetzbarkeit*. Der gedankliche Inhalt eines jeden Satzes kann im Prinzip in jeder Sprache wiedergegeben werden. Oder umgekehrt ausgedrückt: Sätze gänzlich verschiedener Sprachen können Wiedergaben der gleichen Gedankenverbindung sein. Allerdings entsteht dabei die Schwierigkeit, daß ich dieses Gleiche, auf das sich verschiedene Sätze beziehen, nur auf dem Wege über die Sprache fassen kann. Keine Erläuterungen und keine algebraartigen Formalisierungen können mich außerhalb der Sprache stellen. Auch die Zeichen einer Rechenaufgabe verstehe ich nur, wenn ich sie in eine, meine Sprache, übersetze. Zwar kann ich mir das Phänomen des Übersetzens durch die Annahme eines "sprachfreien Gemeinten" plausibel machen, der sprachfreie Raum ist aber schon deshalb eine Fiktion, weil ich ihn ohne Sprache nicht beschreiben und also auch nicht verstehen kann.

Eine Übersetzung ist selbstverständlich niemals gleichwertig. Das Übersetzen ist ein Filterverfahren, das die Transferierung des kognitiven Gehaltes möglich macht, während die Transferierung anderer Ebenen, z. B. die Ästhetik des soziationen nur dann möglich ist, wenn die Zielsprache analoge Mittel in diesem soziation nur dann möglich ist, wenn die Zielsprache analoge Mittel in diesem Sachbereich zur Verfügung hat. Zielt man auf die Denotation und auf den gedanklichen Inhalt, so ist Übersetzen immer möglich. Zielt man auf die Totalität einer sprachlichen Äußerung mit all ihren Aspekten, so ist Übersetzen niemals möglich.

Was sich beim Übersetzen zwischen verschiedenen Sprachen abspielt, ist auch innerhalb ein- und derselben Sprache möglich: Auch in der gleichen Sprache kann jede Äußerung paraphrasiert werden, d. h. der gedankliche Inhalt in andere Worte gegossen werden. Und auch hier gilt die gleiche Regel: Exakte Paraphrasen oder Inhaltsangaben sind immer möglich, aber sie sind niemals mit der Ausgangsäußerung völlig äquivalent.

In der Musik wäre die Frage nach der Übersetzbarkeit absurd. Man kann nicht aus einer "Tonsprache" in eine andere "Tonsprache" übersetzen. Eine solche "Übersetzung" könnte als solche tatsächlich nur bewiesen werden, wenn ein hinter beiden musikalischen Äußerungen stehender gleicher "Gehalt" wahrscheinlich gemacht werden könnte. Dem Bezug auf ein angebliches und fiktives sprachliches Gemeinte müßte ein ebenso fiktiver musikfreier Bezugspunkt entsprechen. Wenn die Rede vom Verstehen der Musik tatsächlich im eigentlichen Sinne berechtigt wäre, müßte ein solcher außermusikalischer Bezugspunkt existieren. Die Hörer ein- und desselben Musikstückes müßten in paralleler Weise diesen quasi außermusikalischen, aber nur durch Musik zugänglichen Bereich, erfassen. So wie die sprachliche Kommunikation immer zu einer möglichst vollständigen Verständigung tendiert, müßte auch in der Musik eine ihr selbst innewohnende Mechanik zur Sicherung der Übermittlung ihres "Gehaltes" wirksam sein. Dieses ist aber nicht möglich, weil ein

hinter einer musikalischen Äußerung stehendes fiktives Drittes, das tendenziell unabhängig von Sender und Empfänger besteht und objektivierbar ist, nicht existiert.

Es gibt allerdings zwei Phänomene, die sowohl der Sprache als auch der Musik eigen sind, die die irrige Vorstellung hervorrufen, daß es sich bei ihnen um durch Analyse ablösbare Inhalte handele: Die *Funktion* und die *Assoziation*. Selbstverständlich gibt es verschiedene Musikstücke gleicher Funktion. Es ließe sich eine offene Liste von möglichen Funktionen von Musik aufstellen. Ganz unproblematisch wäre die Aufstellung einer solchen Liste der möglichen Funktionen von Musik für jeweils eine individuelle Kultur. Es wäre aber auch durchaus denkbar, daß man aus mehreren dieser Listen zu einem universalen Katalog möglicher musikalischer Funktionen gelangen könnte. Ein solcher Katalog wäre durchaus vergleichbar und zum Teil sogar identisch mit einer universalen Systematik der Sprechakte. Genau wie bei den Sprechakten würde sich aber keine zwingende Korrelation zwischen Funktion und musikalischer bzw. sprachlicher Ausdrucksform ergeben. Bekanntlich können ja (um einige sowohl der Sprache als auch der Musik möglichen Funktionen zu nennen) ein Befehl, eine Liebeserklärung, ein Triumph über einen Sieg, eine Klage über einen verstorbenen Angehörigen, selbst in der gleichen Sprache, bzw. Ton-sprache mit den verschiedensten Mitteln zum Ausdruck gebracht werden.

Ist die Funktion nun ein "Inhalt", den man "verstehen" kann? Es scheint doch so, als sei die auf der Flöte gespielte Melodie, mit der ein indischer Berghirte seiner Liebessehnsucht Ausdruck verleiht, in ihrem "semantischen Gehalt" dasselbe wie ein Gitarrenstück, das ein Spanier in ähnlicher Gemütslage spielt. Da sich bei solchen Vergleichen Parallelitäten im äußeren musikalischen Ausdruck in der Regel nicht ergeben oder doch rein zufällig sind, sie von vornherein musikwissenschaftlich völlig unergiebig. Die Feststellung der Funktion von Musikstücken gehört zwar unzweifelhaft in eine Deskription, aber schon allein die Tatsache, daß sie ebenso gut in eine musikalische wie in eine soziologische Deskription gehören würde, zeigt, daß Funktionsbeschreibungen zwar das Umfeld und die Lebensbedingungen von Musik erfassen, aber ihr eigentliches Wesen, ihre Innenstruktur nicht erreichen. Funktion ist also nicht mit Inhalt gleichzusetzen.

Mit der Funktion hängen eng die Assoziationen zusammen, die die Angehörigen ein- und derselben Kultur gewöhnlich beim Anhören von Musik empfinden. Dabei ist es ganz sicher und kann ernstlich nicht bezweifelt werden, daß die überindividuellen Assoziationen nur in ganz groben Umrissen durch die Musik *unmittelbar* auf dem Wege einer analogen Kommunikation hervorgerufen werden. In der Regel bilden sich die festen Assoziationen auf dem *Umweg* über die Funktionen heraus. Man wird zwar auch bei völliger Unkenntnis einer Tonsprache einen Kriegsmarsch nicht für ein Schlaflied (und umgekehrt) halten, wohl aber gehört zu der Assoziation bestimmter Orgelmusik mit Beerdigung die unmittelbare Erfahrung der Funktion.

Assoziationen, die beschreibbar und objektivierbar, die sogar als Kriterien

systematischer Darstellungen dienen können, sind auch notwendige Bestandteile der sprachlichen Zeichen. Assoziationen können sich sekundär mit bestimmten sprachlichen Zeichen verbinden, und sie können auch wieder verschwinden. So kann zum Beispiel eine Zahl, die einen Paragraphen eines Gesetzbuches bezeichnet, dessen Abschaffung gerade heftig diskutiert wird, sich mit explosiven emotionellen Assoziationen aufladen, die auch dann noch wirksam sind, wenn dieser Paragraph gar nicht gemeint ist. Eine sprachliche Zustandsbeschreibung wäre unvollständig, wenn sie nicht diesen Tatbestand mit zur Darstellung brächte. In gleicher Weise kann sich eine Melodie oder vielleicht auch nur ein kurzes Signal mit bestimmten Assoziationen fest verbinden, die selbstverständlich ohne Kenntnis der historischen Zusammenhänge aus der Melodie selbst nicht ableitbar wären.

Funktion und Assoziation sind also den sprachlichen wie den musikalischen Zeichen in gleicher Weise eigen. Ist es deshalb aber berechtigt, sie als Teil eines "Inhalts" anzusehen? Die Antwort lautet nein, und der Beweis für diese Behauptung liegt in der unterschiedlichen Bindung von Musik und Sprache an die *Situation*. Selbstverständlich leben beide, Musik und Sprache, immer in einer jeweiligen Situation: Sei es die ursprüngliche oder sei es etwa die sekundäre als linguistischer Beispielsatz oder als Zu-Gehör-bringen eines musikalischen Motivs im Rahmen einer musikwissenschaftlichen Vorlesung.

Es gibt ein Indiz dafür, daß sprachliche und musikalische "Inhalte" von grundlegend unterschiedlicher Natur sind. Es ist möglich, Bruchstücke einer unbekannten Sprache, über deren Funktion zunächst keinerlei Vorstellung besteht, mit Sicherheit zu entziffern. Die innere Stimmigkeit der Entzifferung wird nicht nur durch die Aufdeckung des fremden Sprachsystems, sondern vor allem auch durch den Nachvollzug in der Sprache des Entzifferers erwiesen. Da Musik nicht übersetzt werden kann, da die Empfindungen, die sich beim Anhören von Musik einer unbekannten "Musikssprache" einstellen, ganz allgemeiner Natur sind, und die "Richtigkeit" dieser Empfindungen nicht nachprüfbar ist, ist ein entsprechendes Verfahren bei der Musik völlig undenkbar. Zwar könnte ein Aufdecken und Beschreiben des zu Grunde liegenden tonalen und rhythmischen Systems ohne weiteres gelingen, aber ohne Kenntnis der Funktionen ist eine weitergehende Interpretation nicht möglich.

Was der komponierende Mensch aus sich herausstellt, ist nur lebensfähig und sinnvoll als Bestandteil einer institutionalisierten Handlung. Es kann zwar sinn- und zweckentfremdet zum Teil einer anderen Handlung gemacht werden, ist aber zu keinem Eigenleben fähig. Die Welt, in die Musik jeweils versetzt, besteht in einer Umschaffung der gegenwärtigen Situation zu einer neuen Wirklichkeit. Die Sprache dagegen schafft durch die Kraft der Darstellung eine jeweils zweite Welt, die sich von der gegenwärtigen Situation lösen, ihr sogar entgegengesetzt sein kann.

Musik kann nur erfahren, d. h. erlitten oder genossen werden. Sprache hat dagegen einen kognitiven Bestandteil, über den unabhängig von jeder Situation Verständigung zu erzielen ist. Assoziationen, die mit sprachlichen Zeichen

verbunden sind, können mit Hilfe von anderen sprachlichen Zeichen einwandfrei beschrieben werden. Musikalische Assoziationen können nicht mit Hilfe von Musik intersubjektiv dingfest gemacht werden. Eine Verständigungssicherung ist nur auf dem Wege über die Beschreibung der zugehörigen Situation möglich. Wenn Musik überhaupt einen Inhalt hat, dann kann sie ihn nur durch sinnstiftende sprachliche Äußerung erhalten.

3. Es ist außerordentlich aufschlußreich, verschiedene Methoden der Sprachbeschreibung, hinter denen jeweils verschiedene Theorien über das Wesen der Sprache stehen, daraufhin zu prüfen, ob sie in analoger Weise auch für die Beschreibung von Musik verwendet werden könnten. Wenn das Verfahren übertragbar ist, ohne daß ein Rest im Sprachbeschreibungsverfahren übrig bleibt, der auf die Beschreibung von Musik nicht angewandt werden kann, liegt der Schluß nahe, daß die hinter diesen Sprachbeschreibungsverfahren stehenden linguistischen Theorien das Wesen der Sprache nicht vollständig erfaßt haben. In der Tat gelingt es nicht mit Hilfe der drei hier näher betrachteten linguistischen Methoden das Spezifische der Sprache gegenüber der Musik sichtbar zu machen. Dieser Umstand ist ein weiteres Indiz für das Ungenügen dieser theoretischen Ansätze.

a) Das strukturalistische Verfahren der Corpusanalyse läßt sich sinngemäß auf die Analyse eines musikalischen Corpus übertragen. In beiden Fällen ist es möglich, die kleinsten Einheiten physikalisch zu bestimmen und sodann in einem Testverfahren festzustellen, welche Unterschiede nur Differenzen der jeweiligen Realisation sind und welche Elemente einer zu Grunde liegenden Struktur sind. Phonetik und Phonologie haben also in der musikwissenschaftlichen Analyse ihr exaktes Analogon. So wie es aus den unterschiedlichsten Gründen verschiedene Realisationen eines Phonems in einer Sprache gibt, die mit dem zu Grunde liegenden Phonemsystem nichts zu tun haben, so gibt es bei der Interpretation eines Musikstückes Tonhöhenschwankungen und rhythmische Unregelmäßigkeiten, die allein der Realisation angehören (meist auch bewußte Ausdrucksmittel sind), die aber das jeweils zu Grunde liegende tonale System oder die zu Grunde liegenden rhythmischen Strukturen nicht berühren.

Weiterhin ist es möglich, sowohl in der Sprache als auch in der Musik, größere Einheiten zu isolieren, ein Morphem-bzw. Motivinventar aufzustellen.

Aber eine sprachliche Analyse, die ausschließlich von der äußeren Erscheinungsform ausgeht, kann nur in der Phonologie erfolgreich sein, weil hier die Erscheinungsform mit der Funktion praktisch deckungsgleich ist. Ein Phonem /u/, das als Idee hinter allen Realisationen von /u/ steht, hat lediglich die Aufgabe anders zu sein als alle anderen Phoneme. Eine Flexionsendung deutsch /e/ kann in einer Item-And-Arrangement-Grammatik, die allein von der äußeren Erscheinung ausgeht, nicht befriedigend plaziert werden, sie muß vielmehr auf das hinter den Flexionsendungen stehende grammatische System bezogen werden. Die Grammatik ist ein System grammatischer Kategorien und nicht ein System von Morphen oder Morphemen. Der postbloomfieldsche Distributionalismus ist an der Morphologie und Syntax gescheitert. Die Adaption einer von der äußeren

Erscheinungsform ausgehenden Morphemanalyse auf die Musik gerät nicht in diese Schwierigkeiten. Die herkömmliche musikwissenschaftliche Analyse ist durchaus mit dem Verfahren einer linguistischen Methode vergleichbar, die allein von der äußeren Formen ausgeht. Die in Rede stehenden Linguisten haben das Wesen der Sprache verkannt, während ihr theoretischer Ansatz für die Musik durchaus brauchbar ist.

b) Auch die Semiotik gerät bei einer Sprachbeschreibung in ein unauflösbares Dilemma. Das sprachliche Zeichen stellt eine unauflösbare Einheit von äußerer Gestalt und Bedeutung dar. Die isolierte Betrachtung des signifiant oder des signifié ist eine Notwendigkeit linguistischer Beschreibung, aber sie zerstört gleichzeitig dieses selbe Zeichen, das ja nur als untrennbare Einheit von beidem als solches angesprochen werden kann. Da nur auf dem Gebiet der Phonologie eine 1:1 Relation zwischen signifiant und signifié besteht, muß der Linguist auf allen anderen Ebenen der Sprache sich entscheiden, ob er die Form oder die Bedeutung als oberstes Einteilungsprinzip gelten lassen will.

Da die Semiotik die Sprache nur als *ein* Zeichensystem unter vielen betrachtet, bei allen anderen Zeichensystemen aber der Aspekt des signifié Schwierigkeiten macht, (den äußerlich abgrenzbaren Zeichen entsprechen häufig keine distinktiven Sinnelemente), tendiert sie dahin, auch die Sprache als ein durch die äußere Gestalt ihrer Zeichen konstituiertes System zu betrachten. Damit gerät sie in die gleichen Schwierigkeiten wie der oben charakterisierte postbloomfieldische Strukturalismus.

Welche Schwierigkeiten die Semiotik auch auf dem Gebiet der Musik hat, die durch eine Analyse des Klangs gewonnenen Strukturelemente als Zeichen im Sinne der Zeichenlehre zu verstehen, zeigt sich bei J. J. Nattiez, der in seinem zusammenfassenden Werk *Fondements d'une sémiologie de la musique*, 1975, das Problem von allen Seiten beleuchtet.

Die auf dem Gebiet der Sprache so einleuchtende Lehre de Saussures von der willkürlichen Setzung der Zeichen (*arbitraire du signe*) ist auf die Musik wohl kaum übertragbar. Wenn etwa die Bedeutung der musikalischen Strukturelemente mit dem Klangerlebnis des Rezipienten identisch sein sollte, dann möchte man nicht glauben, daß die Klangsegmente willkürlicher Setzung entspringen könnten, und man dieselben Eindrücke auch mit anderen Klängen hervorrufen könnte. (So wie in einem entsprechenden Kontext die verschiedenen Klänge "painting" und "Gemälde" dieselbe Vorstellung wecken können).

c) Besonders verlockend war es natürlich, die generative Theorie auf die Musik zu übertragen. Es sei verwiesen auf den Aufsatz von J. Sundberg und B. Lindblom, *Generative Theories in language and music descriptions*, in: *Cognition*, 4, 1976 und auf das Buch von L. Bernstein, *The Unanswered Question*, 1976. Das praktische Verfahren der Übertragung auf die Musik läßt sich ohne Schwierigkeiten vollziehen. Der Grund dafür liegt auch darin, daß auf dem Gebiet der Musikwissenschaft schon sehr viele Arbeiten existieren, die sich mit der Aufdeckung hinter einer gegebenen Melodie stehenden sehr abstrakten Grundstrukturen befaßt haben. So lassen sich reich figurierte Melodien auf

einfache Akkordfolgen reduzieren, die dann in typischer Reihung immer wieder auftauchen und Musikstücken verschiedenen Stils und verschiedener Funktion zu Grunde liegen. Es sei hier an die Lehre Hugo Riemanns von den Kadenzzen erinnert. Bernstein treibt die Abstraktion mit Hilfe zum Teil zweifelhafter musikethnologischer Annahmen noch weiter.

Bei der generativen Sprachanalyse erhalten wir in der Tiefenstruktur eindeutige logische Grundmuster, die bei ihrer Transformierung in die Oberfläche verunklärt werden und ihre Eindeutigkeit verlieren. Die Sprache als ein lebendiges organisches Gebilde, das immer auch einen ästhetischen Aspekt hat, ist mit dieser Betrachtungsweise natürlich nicht zu erfassen.

Was für einen Status haben die musikalischen Grundstrukturen? Die Musikwissenschaft hat sie vielfach als Logik bezeichnet, was aber nur in einem allgemeinen metaphorischen Sinn zutreffend sein kann. Bernstein leitet sie aus den physikalischen Gegebenheiten der Obertonreihe ab. In jedem Fall aber sind diese Strukturelemente Resultat eines Analyseverfahrens am toten Objekt. Die musikalische (ebenso wie die sprachliche) Wirklichkeit speist sich aus mehr Quellen.

Ganz unakzeptabel aber dürfte für die Musikwissenschaft die Abwertung der Oberfläche gegenüber der Tiefenstruktur sein, die ein wesentliches Charakteristikum der generativen Linguistik ist. Man möchte selbst das simpelste Menuett nicht eintauschen gegen die ihm zu Grunde liegenden Kadenzzen, oder gegen eine Betrachtung der Grundbefindlichkeiten der abendländischen Diatonik.

4. Bei tierischer Kommunikation sind Sprache und Musik identisch. Wilhelm von Humboldt sagt hierzu: "Der Mensch, als Tiergattung, ist ein singendes Geschöpf, aber Gedanken mit Tönen verbindend" (VII, 61). Wenn Sprache also ein Gesang ist, der eine Verbindung mit dem Gedanken eingegangen ist, dann müßte die Musik auf der Stufe der tierischen Kommunikation stehen geblieben sein. Betrachtet man die vorliegenden außerordentlich umfangreichen und differenzierten Analysen von Vogelgesang, so kann man in der Tat keinen grundsätzlichen Unterschied zur Musik der Menschen feststellen. Auch im Graduellen nicht: Die Zahl der Motive, der Reichtum ihrer Kombination, die individuellen Stilausprägungen einzelner Vogelindividuen, alles das läßt sich durchaus mit menschlicher Musik vergleichen. Auch die Verknüpfung von bestimmten musikalischen Hervorbringungen mit bestimmten Situationen teilt der Vogelgesang mit menschlicher Musik. Institutionalisierte Handlungen und zugehörige musikalische Äußerungen können genau wie in der Tierwelt eine untrennbare Verbindung eingehen. Daß viele Menschen weder singen noch musizieren, viele sogar gänzlich unmusikalisch sind, bezeichnet einen graduellen, aber keinen grundsätzlichen Unterschied. Er erklärt sich leicht daraus, daß die Sprache eben in vielen Fällen als das wirkungsvollere Werkzeug an die Stelle der Musik getreten ist.

Daß dennoch Musik mehr ist als ein Atavismus, ein aus animalischer Urzeit stehengebliebener Rest, kann man aus ihrer geschichtlichen Veränderung ableiten. Auch tierische Kommunikation verändert sich in der Zeit. Das aber

sind Veränderungen wie die Erosion einer Landschaft.

Durch den Sprachbesitz wird der Mensch zum Menschen. Von diesem Augenblick an sind alle seine Handlungen etwas anderes als die Handlung von Tieren. Nicht bewußte Planmäßigkeit und Zweckmäßigkeit gewinnt er mit der Sprache, sondern er wird im Gegenteil durch sie von der Verfolgung der baren Nützlichkeit abgelenkt. Er schafft sich mit Hilfe der Sprache eine zweite Welt ("Ideologie") und diese zweite Welt bestimmt sein Handeln oft mehr als die Umwelt mit ihren Zwängen und Nöten. Der ideologiesteuerte Mensch ist zu allem fähig, was ihm unzutraglich ist: Er fastet, statt zu essen, er treibt Askese, statt sich zu vermehren, er martert oder tötet seine Artgenossen.

So wird auch die Musik zu einem Teil seines Weltbildes, seiner Kultur. Sie verändert sich mit ihr.

Um die Welt zu verstehen, betrachtet der Mensch die Phänomene seiner Umwelt als seien sie eine Sprache. Er muß die Steine zum Reden bringen, damit er im Dialog mit ihnen sie versteht. Erkenntnis ist ein sprachliches Phänomen.

Indem der Mensch Musik als Sprache betrachtet, hebt er sie auf die Ebene seiner Kultur. Sie wird ihm dadurch zu einer besonderen Form der Sprache und er kann sie daraufhin auch wie eine Sprache handhaben. Der fundamentale Unterschied zwischen Vogelgesang und menschlicher Musik liegt also nicht im Äußeren der jeweiligen Erscheinungsform, nicht in der Emanzipation von instinktiven Handlungsabläufen, sondern darin, daß Musik unter den Händen des Menschen zur Sprache und damit zu einem Teil menschlicher Kultur wird.

Die Semiotik leistet den Nachvollzug dieses Vorgangs und somit ist das Kleben an der linguistischen Metapher nicht ein Mangel, sondern es spricht sich darin ein wahrer Tatbestand aus.

La genèse du langage dans la perspective de la sémiotique peircienne

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Depuis plusieurs années, la question de l'origine (surtout phylogénétique) du langage est redevenue un sujet d'actualité. La réflexion est cependant à la fois handicapée et stimulée par un problème central, celui-là même qui est la source de la fameuse interdiction, à la Société de Linguistique de Paris, de présenter des communications ou des articles sur le sujet. Ce problème, c'est évidemment la difficulté de faire plus que spéculer. Il ne sert en effet à rien, de nos jours, de ne proposer que de nouvelles opinions qui remplaceraient les anciennes sans qu'il y ait un progrès quelconque.

Des conditions du progrès, je mentionnerai les suivantes.

1. En premier lieu, il faut interroger le principe de l'équivalence des langues du monde, principe mis de l'avant par la linguistique moderne. Ce qui est en cause, c'est l'éventualité d'une investigation directe de la phylogénèse. On se doit de prendre au sérieux ceux qui affirment que la recherche directe "sur le terrain" est possible, si l'on ne veut pas se priver d'une source d'information possible. Mais ce n'est pas mon propos ici (cf. Pesot 1981).

2. Il faut également rechercher aux confins de l'humain, là où il n'y a pas ou plus ou pas encore de langage. C'est l'approche indirecte de la phylogénèse, qui comporte évidemment tous les risques d'une approche indirecte. Même en s'imposant certaines conditions quant à la formulation d'hypothèses, la généralisation, la compatibilité avec des données scientifiques dans des domaines connexes, etc., on n'évite pas toujours l'argument fallacieux ou le rapprochement superstitieux (du genre *post hoc ergo propter hoc*). Mais la voie indirecte reste nécessaire et peut donner des résultats puissants (comme l'a montré, dans son domaine, la linguistique historique et comparative des langues indo-européennes) (cf. Morin & Piattelli-Palmarini 1974; cf. aussi Pesot 1981).

3. Un progrès peut être conditionné par un échafaudage théorique nouveau qui est d'autant plus utile qu'il est conçu indépendamment du domaine auquel on l'applique. C'est ce troisième point que je voudrais développer quelque peu ici.

Pendant longtemps, on appréhendait mal le phénomène du langage et par conséquent on ne savait pas très bien de quoi on cherchait l'origine. Depuis Saussure, on comprend mieux le langage (à l'aide de concepts comme ceux de la relation signifiant-signifié, de l'arbitraire du signe, de la valeur, de l'opposition entre signe et symbole, etc.), mais ces concepts n'aident pas beaucoup la recherche glossogénétique. En effet, par exemple, la linguistique structurale nous oblige

à considérer la parole et l'écriture essentiellement comme deux façons coexistantes de représenter les mêmes formes et à ignorer que l'ordre historique (qui va évidemment de la parole à l'écriture) est un ordre régi non par le hasard mais par une nécessité plus profonde (cf. Fónagy 1981a). Autre exemple. Selon Todorov, il y a des signes (ternaires) et des symboles (binaires), et les soi-disant signes des "primitifs" et des "sauvages" ressembleraient en tous points aux symboles des gens "évolués" (Todorov 1977). Cela revient à admettre qu'on manque d'un schéma global, qu'on continue à se servir d'un modèle pensé pour un langage "normal", un langage "linguistique", sur lequel on greffe les "autres" langages (Pesot 1978).

Quand Piaget, de son côté, affirme que dans l'esprit du petit enfant, le signifié n'est pas encore sevré du signifiant sensori-moteur (p.ex. dans Piaget-Chomsky 1979: 58), on a affaire à la même difficulté conceptuelle. La naissance d'un signe à partir d'une matière non déjà sémiotisée, on ne peut la penser en termes de signifiant et de signifié. Avant le signe, on peut poser un mouvement, un cri, un objet; pour que ce mouvement, ce cri ou cet objet devienne signifiant, il faudrait supposer qu'il se scinde brusquement et que les deux moitiées constituent, du jour au lendemain, les deux faces abstraites du signe, lui-même renvoyant éventuellement à une troisième entité, le référent. Une deuxième possibilité est de supposer que les premiers objets sémiotisés sont, non des signes, mais des symboles dont la formation reposerait sur l'association entre deux entités préexistantes; c'est concevable, mais le problème de la naissance du signe ne s'en trouve que reporté, car nul ne sait comment le signe précéderait du symbole.

Nous avons donc besoin d'un modèle sémiotique qui permettrait de rendre compte de ce qui est appelé "signe" et "symbole" (et, dans leur sillon: "arbitraire" et "motivé") et, en même temps, de leur acquisition, de leur usage, de leur perte pathologique ainsi que de leur création et de leur développement. Je crois que le modèle peircien est présentement le meilleur candidat (v. Peirce 1978; cf. aussi Pesot 1979).

Dans la sémiotique peircienne, il n'existe que des relations triadiques. La première, celle qui décrit le signe comme processus pragmatique, relie un moyen (ou representamen) à un objet grâce à un interprétant. Le moyen, ce peut être un mouvement, un cri ou une chose quelconque, à condition qu'un interprétant l'interprète comme renvoyant à autre chose (l'"objet"). On entrevoit un premier avantage de ce modèle pour la glossogénétique: un mouvement, un cri ou une chose quelconque peut exister comme tel indépendamment de l'apparition d'un interprétant; celui-ci ne fait qu'établir une relation nouvelle. L'objet auquel le moyen renvoie peut lui aussi exister avant ce renvoi. En revanche, l'interprétant n'est jamais innocent sémiotiquement; il constitue à la fois troisième terme du premier cycle sémiotique et premier terme (en tant que moyen) d'un second cycle. C'est donc avec lui que commence et que continue toute sémiologie.

Le célèbre tableau représentant les trois trichotomies du signe constitue un véritable modèle de la *sémiogénèse*. Conçue comme un ensemble fini de

catégories phénoménologiques, la série priméité—secondéité—tiercéité peut en effet être vue, grâce aux rapports d'implication asymétrique, comme les trois étapes de l'évolution sémiotique. En d'autres termes, aucun être ne peut créer ou utiliser des signes de la tiercéité (légisignes) avant d'avoir expérimenté des signes de la secondéité (sinsignes); aucun être ne peut expérimenter des sinsignes avant d'avoir vécu des signes de la priméité (qualisignes). Ainsi, le sentiment inanalysé, la perception sensorielle, l'instinct, l'émotion, l'expressivité, l'intuition, le ton de la voix, la sensation synchrétique et naïve, etc., constituent des priméités et se situent au bas d'une échelle logique et évolutive. Le deuxième palier (secondéités) comprend l'existence réelle et vécue d'un objet *hic et nunc*, le commandement, l'action-réaction, l'agression, l'usage semi-intellectuel d'objets. Au sommet (tiercéités), on trouve l'habitude, les classes et les modèles, le raisonnement, les lois qui gouvernent les faits du futur.

L'asymétrie de ce tableau formalise donc une progression logique et temporelle. Mais elle fonde également une gradation de complexité, d'abstraction, de conscience. C'est là l'origine de la notion de *degrés de sémioticité*. Les signes de la priméité sont peu sémiotiques; ils sont simples, indifférenciés, incontrôlés, sans structure déterminée, sans syntaxe, en symbiose avec le contexte. Au commencement, il n'y a pas d'autres sortes de signes, dans la mesure où les choses et les signes de la secondéité et de la tiercéité ne sont pas encore construits. L'expérience, à son tour, est le domaine de la secondéité. L'usager de signes de la secondéité infère des règles à partir des cas, il crée des associations spatiales ou temporelles entre les choses, entre la trace et l'animal, entre la localisation d'une proie et tel mouvement du bras qui l'indique, entre la fumée et le feu. C'est le stade, davantage sémiotique, où l'humain se dégage de son environnement au moyen des signes qu'il produit. La maîtrise de cet environnement est réservée à la tiercéité, stade hautement sémiotique où l'humain (espèce ou individu) a fini de construire sa réalité. Le langage est ainsi un phénomène "de troisième génération" mais qui incorpore en quelque sorte les deux paliers inférieurs.

C'est par les deux autres trichotomies du signe que le tableau peircien rend compte de cette incorporation. La première ne fait que classer les signes en fonction du mode d'être de son moyen. La seconde attribue un des modes d'être à la relation entre le moyen et son objet. La relation première et primitive, la plus simple, la plus perçue et la moins conçue, la moins sémiotisée, c'est la relation iconique (ressemblance). La relation peut, ensuite, être de nature indiciaire (contiguïté de fait). La relation la plus sémiotisée est de nature symbolique (contiguïté instituée). Or, si un qualisigne est forcément iconique, un sinsigne est soit indiciaire, soit iconique; un légisigne est soit iconique (p.ex. le mot /maison/), soit indiciaire (p.ex. le mot /je/), soit symbolique (p.ex. la structure syntactico-sémantique /Paul ouvrit la portière, descendit de sa voiture et se dirigea vers le policier/). Faute de temps, je négligerai ici la troisième trichotomie.

Il me faut abréger. Le modèle que je viens d'esquisser a été déduit par un

logicien-philosophe-mathématicien pour rendre compte de toute activité quelle qu'elle soit. Il ne s'agit pas d'un classement de données recueillies au préalable. C'est pourquoi on l'a souvent qualifié de jeu gratuit, éloigné de la réalité. Mais je ne connais pas de domaine où, bien appliqué, il ne fonctionnerait pas. Mon application, qui en fait un modèle de sémiogenèse et, *a fortiori*, de glossogenèse, confirme son caractère d'hypothèse universelle.

Ce modèle s'avère utile, en particulier, lorsqu'on considère les quatre sous-problèmes suivants. Premièrement, il peut répondre, à sa façon, à la question que posait Sebeok (1974: 66), à savoir "comment il se faisait, si le langage est tout ce que les linguistes ont dit qu'il était, que les autres systèmes de communication n'aient pas dé péri." Deuxièmement, il admet l'existence, dans le langage, de formations onomatopéiques (légisignes iconiques) ou, plus généralement, de séquences dont la structure a un rapport non arbitraire avec leur signification (cf. Ross 1980; Pesot 1980). Troisièmement, il est tout à fait compatible avec la conception psychanalytique que la métaphore (une des icones chez Peirce) constitue une "régression 'éclair' [qui] récapitule la genèse du langage" (Fónagy 1981a: 127), plus précisément, qu'elle exprime "a momentary, controlled regression to an early phase of mental processing, to a preparatory phase of conceptual thinking." (Fónagy 1981b) Enfin, ce modèle permet de voir sous un jour nouveau le fait mis en évidence par la neurologie, à savoir que la voix n'a pas originellement été destinée à un usage linguistique et qu'elle continue d'être utilisée par les zones cérébrales qui règlent l'expression involontaire d'émotions (cf. Myers 1976).

En un mot—et je terminerai là-dessus—, le modèle nous invite à abandonner certaines frontières strictes sur lesquelles on a l'habitude de s'appuyer: entre abstrait et concret, entre arbitraire et motivé, entre mot propre et mot figuré, entre "linguistique" et "non linguistique". A la place, il nous propose une progression cumulative à la fois logique et évolutive où le langage "linguistique" ne représente qu'un aboutissement, qu'un sommet, sans abolir son histoire. Après tout, l'avènement de l'homme n'a pas non plus entraîné l'extinction des singes!

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Forms, Functions and Applications of Paralanguage as a New Multidisciplinary Area

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Poyatos' classification of paralinguistic phenomena into Primary Qualities (timbre, volume, resonance, tempo, pitch level, registers, intervals and range, intonation range, syllabic length, and rhythm), Qualifiers (respiratory-, glottis-, laryngeal-, velar-, pharyngeal-, articulatory-, labial- and maxillary types of control, and articulatory tension), Differentiators (laughing and crying—of great morphological and functional importance—, coughing, sighing, yawning, sneezing, belching, hiccupping, and extremes of loud and low voice), and Alternants (both articulated and inarticulated sounds, whether vocalic or consonantal, and silences, such as clicks, throat clearing, nareal and pharyngeal ingressions and egressions, hissing sounds, moaning sounds, etc.) supports a revision of the concept of discrete segmental elements (here classified as words, alternants, silences, and kinesic constructs) and overriding nonsegmental ones (intonation features, primary qualities, qualifiers, differentiators, and parakinesic intensity, range and velocity). In addition, the highly lexical value of alternants demands that we establish many labels (verbs and nouns with which to refer to them, as we have 'to hiss' and 'a hiss'), transcription forms beyond the IPA, and orthographic forms to which both the layman and the professional writer could resort.

Once paralanguage has acquired its own status many applications can be launched in a scientific way, such as in the study of normal and abnormal psychology, social stratification, gender differences, cross-cultural studies, formal and informal interactions, nurse/doctor-patient relationship, the business encounter, the court, and its important functions in literature, not only in the novel but in the theatrical performance, beyond the playwright's script, as an important semiotic tool.

A Semiotic Analysis of Meeting and Parting Rituals in Japanese and English¹⁾

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This is a report of on-going research into verbal and nonverbal behavior used in meeting and parting rituals by Japanese and Americans. The data are 565 meeting and parting scenes from a total of 29 hours of Japanese and American television dramas and movies. The purpose of this report is to clarify the following aspects of meeting and parting rituals: 1) their structure and function; 2) the type and frequency of nonverbal behavior appearing in the rituals; 3) non-linguistic factors restricting nonverbal behavior; 4) the universality of aspects of these rituals and culturally related differences between Japanese and Americans.

1. Structure & Function: Meeting ritual is classified into pre-greeting, greeting and post-greeting. Parting ritual consists of pre-closing, closing and post-closing. The function of each step is the following, respectively: preparatory step to greeting, confirming friendly relations, confirming more personal and intimate relations; suggestion of closing a conversation, re-confirming friendly relations, re-confirming more personal and intimate relations. These structures and functions can be seen equally in both Japanese and American rituals.

2. Type & Frequency of Nonverbal Behavior: From the data, 10 types of nonverbal behavior were found for Japanese and 13 types for Americans. Of these, the following 6 types of behavior are common to both: nods, waves, touching an arm or shoulder, taking or holding hands, lifting one's hat, and standing. They comprise 49.5% of the total gestures for Americans and 34.1% of the Japanese gestures. As for gestures peculiar to each culture, there are 4 for Japanese and 7 for Americans. The gestures peculiar to Americans are shaking hands, 19.5%; kissing, 12.6%; the head toss, 10.5%; hugs, 4.7%; linking arms, 1.6%; patting a hand, 1.1%; blowing a kiss, 0.5%. In all, these gestures make up 50.5% of the data. Of these seven gestures, 5 are touching gestures. If the percentages of touching an arm or shoulder and taking or holding hands are added, more than 50% of the data is comprised of touching gestures in the case of Americans. On the contrary, the Japanese data show only 6% touching

1) The research on which this paper is based was done in collaboration with Laura Ford, of the University of Pennsylvania.

gestures. This is a major characteristic difference between the two cultures. As for gestures peculiar to Japanese, first is the ordinary type of bowing, which is a typical Japanese gesture and makes up 58.4% of the total Japanese data. Second is the formal type of bowing, 5.8%. In the formal type, the way of aligning both hands is more exact and the head is dipped lower than in the ordinary type. Third is lifting a hand, 1.0%. Fourth is sitting on tatami or wooden floor before bowing, 0.7%. These four gestures together make up 65.9% of the data.

3. Restrictions Determined by Non-linguistic Factors

3.1. Distance: Distance is classified into four categories: intimate, personal, social, and public as proposed by Hall²⁾. Concerning distance, gestures common to both cultures do not show much difference; however, those peculiar to each culture show the following difference: Americans have 6 gestures used in intimate distance, which is less than 1.5 feet, yet Japanese have no gestures. In social distance, however, which is from 4 to 12 feet, Japanese can use all 4 gestures, while Americans can use only 2 out of 7. These results show that the frequently used or preferable distance for gestures in greetings and farewells is different for each culture. Both cultures use personal distance in a like manner, but Japanese prefer social to intimate distance and Americans prefer intimate to social or public distance.

3.2 Gender of the participants: Although few differences can be seen in gestures shared between the two cultures, different rules appear in a comparison of unshared gestures. In the case of Japanese there are no particular restrictions concerning gender. On the contrary, Americans have the following rules: 1) if participants are both males, five gestures, kisses, hugs, linking arms, patting a hand and blowing a kiss, are not used in ordinary situations. Instead of these five gestures, the handshake is frequently used between men, 2) between females, these five gestures may be used, yet a handshake is not generally used except in the situation of introduction, 3) from male to female, kisses are not usually blown, 4) from female to male, every possible gesture can be used. In this way concerning the rules based on the gender of participants, it was found that Japanese don't have particular restrictions, while Americans have certain distinct rules, especially for males.

3.3 Relationship of Participants: In shared gestures, differences can be seen in the behavior of standing: It is not common in Japan to stand when the participants are in an intimate relationship such as "parents and children", "wife and husband", and "lovers"; on the other hand, Americans frequently stand when greeting someone even in such an intimate relationship. Among gestures peculiar to Japanese, the polite type of bowing and sitting belong to the most formal type of gestures in Japanese. As in the case of standing, Japanese tend to avoid using formal gestures in an intimate rela-

2) Hall, E. T., *The Hidden Dimension*, Doubleday & Company, Inc., 1969.

relationship as well as when a participant of high position is speaking to one of lower status. Among gestures peculiar to Americans, there are two basic restrictions: Among family, lovers, and friends, which are in-group in American culture, every gesture may be used; there is, however, one exception, the handshake between husband and wife and between lovers. For participants in such a relation to shake hands with each other would indicate that the degree of intimacy had become lower than before. In business and service relationships, which are out-group for Americans, kisses, hugs, linking arms, patting hands and blowing kisses are not shown. This shows that these five gestures are among the most intimate gestures used among in-group members in American culture.

3.4. Context: First, in the case of Japanese it is interesting to take note of the following three contexts related to the family: greetings in the morning at home, upon a return home and when going to bed. In these three contexts, there are no gestural signs that are usually used in Japan, while Americans may touch an arm or shoulder, nod, kiss, hug, pat a hand and use a head toss. Second, Americans have two gestures which are not restricted by any context, that is, the nod and the head toss. Japanese have no such gestures. Third, in American gestures, blowing a kiss has the particular characteristic of being used only in parting rituals, never when meeting. Fourth, generally speaking, Americans may use various gestures in each context, but in the context of the introduction, the number of possible gestures is limited to five, that is, nod, standing, lifting a hat, shaking hands and the head toss. Most of these gestures are not touching gestures, because the degree of intimacy between participants, a factor of their relationship, is low in this context. Business and service contexts, as well, seem to be quite restricted, and this, too, is because of the lack of intimacy in the participants' relationship.

3.5. Major Non-Linguistic Factors for Each Culture: Of the four factors we've examined, distance, gender of participants, their relationship, and non-linguistic context, we have seen that the relative importance of each factor is different for the two cultures. For American nonverbal behavior, gender of the interactants and their relationship, in other words, the degree of intimacy between them, are most important. For Japanese, the context or setting of the interaction combined with the formality of the gestures is the major factor.

4. Hypothesis: As we've seen, there are many culturally related differences in the type and frequency of nonverbal behavior, and in factors which restrict their use; nevertheless, the internal structure of meeting and parting rituals and their functions are basically similar. So even though differences seem to be quite conspicuous on the surface, once these differences are analyzed in the framework of the fundamental structure of the rituals, we find that they fulfill the same functions. From this consideration, the following hypothesis is proposed in conclusion: culturally related differences in the meeting and parting rituals of various languages should appear in the types and frequency

of nonverbal behavior and in the restrictions on the behavior; the function of meeting and parting rituals and their internal structures, on the other hand, are expected to be much more universalistic, varying little from culture to culture.

WORKING GROUPS

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Working Group 1:

History (and Philosophy) of Science and Linguistics

Organizer & Chairman: E. F. Konrad Koerner

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Introductory Remarks

As Schmitter (1982) has recently suggested, it appears that it is because of lack of acquaintance with the work of philosophers of science from the 18th century onwards (e.g., Vico, Voltaire, Turgot, Ranke, et al.), that linguists have embraced Kuhn's morphology of 'normal science' and 'scientific revolutions' so enthusiastically. However, Kuhn's (1962) book was written for the natural sciences, in particular theoretical physics. Falter (1979) sketches an interesting parallel to the (largely uncritical) acceptance of Kuhn's philosophy of science in the social sciences, especially the field of political science which linguistics witnessed in the late 1960s and during the 1970s. Falter points to a series of (additional) reasons for the widespread acceptance of Kuhnian ideas in the social and behavioral sciences (of which linguistics is a part), including the vagueness of Kuhn's definitions (which allowed for a variety of fairly subjective interpretations) and the emphasis on the social dynamics of 'scientific revolutions' (which appealed to many social scientists since it referred to something they were already familiar with).

In 1964 and 1968 two symposia were held to discuss the significance of Kuhn's proposals in the field of linguistics and anthropology (whose results were published in Hymes 1974). But it seems that only Hymes's Introduction, written in the early 1970s, grasped what was happening in linguistics at the time, namely, the attempt on the part of followers of Transformational Generative Grammar to use particular suggestions made in *The Structure of Scientific Revolutions* in such a manner as to demonstrate that a 'revolution', a complete break with the past, was taking place in linguistics. (For details cf. Murray 1980 and Koerner's paper at this Congress.) Percival's (1976) critique of the application of Kuhnian principles to the history of linguistics, largely based on a misreading of Kuhn and motivated by concerns other than scientific, had, it seems, the negative effect of leading to an abandonment of Kuhn's ideas altogether rather than a thorough investigation of his proposals.

Hymes (1974) contained two papers which address questions which still today require discussion: J. Greene's on the relationship between Linguistics and History of Science, and Wolff & Thorne's presentation of essentials of *Wissenssoziologie*. It seems that they had little influence on the subsequent debate

among historians of linguistics. As a result, the present workshop may be regarded as an (albeit modest) attempt to reopen the debate *re* the relationship between linguistics and the natural as well as the other sciences, the question of to what extent linguistics is a 'science' and to what extent its changes through time approximate developments in other fields of inquiry. In addition, issues pertaining to a 'contextualist' approach to linguistic historiography will be raised.

The papers presented for discussion at this workshop meeting address themselves to a variety of topics. Some of them (Bunge, Bell, and, to some extent, Chatterjee) are of a more philosophical nature, others (especially those by Wilbur and Jankowsky) deal with more specific problems in the history of (largely 19th-century) linguistics. Another still (Murray) pleads for a sociologization of linguistic historiography, whereas the remaining paper (Tonisson) launches a critique of current communication departments and their neglect of linguistic theories.

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Philosophical Problems in Twentieth-Century Linguistics

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Most linguists admit that their discipline is at a cross-roads, and some of them hold that it is undergoing a revolution. However, there is no consensus as to the nature of the crisis or its ultimate resolution. Is it the case that linguists have come up against some fundamental problems that are yet to be solved, or have new theories been proposed which in fact are in the process of solving some of the basic problems of the discipline? It is submitted that there is some of each and, besides, a philosophical crisis in linguistics. The aim of this paper is to discuss this crisis by pointing to a few outstanding fundamental problems in the foundations and methodology of linguistics.

Among the unresolved problems in the foundations of linguistics are the

following: 1) What is a natural language? (We know what a formal language is only because we have stipulated what it must be.) 2) Is grammar really the core of every language, or should we relax the definition of a language to allow for pregrammatical symbolic systems in order to allow for language acquisition as well as for the evolution of language? 3) What is linguistic meaning, and, in particular, how are sense and reference to be defined in the case of natural languages?

There are also a number of open questions in the methodology of linguistics, for instance, 4) Can the study of language be conducted fruitfully in isolation from psychology? 5) If not, must it ally itself with mentalistic psychology rather than with physiological and social psychology? 6) If the trademark of a science is that it looks for laws or uses them, does linguistics qualify as a science? 7) If not, is it an idiographic discipline or just a protoscience still waiting for its Newton? 8) Are there genuine explanations and predictions or just descriptions in present-day linguistics? 9) Are linguistic theories being rigorously tested, or is there some manipulation of unfavourable evidence? 10) Is it desirable to keep the present fragmentation of linguistic studies into pure linguistics, historical linguistics, neuro-linguistics, psycholinguistics and sociolinguistics, or is there reason to promote a merger of these various branches?

Surely, some scholars may believe that the above problems are not such, or else that they have already been solved. But it is characteristic of fundamental problems that they pass unnoticed or are taken to have been solved—until they reappear, possibly under a new guise, and block further scientific progress unless tackled. Hence the practical importance of foundational and methodological studies.

The Use of the Term 'Psychology' in the Linguistic Discourse of the 1870s and 1880s

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It was a doctrine shared by all members of the original school of the Neogrammarians that exceptions to the (otherwise) exceptionless laws of language evolution were to be explained by 'psychology'. As Ziemer (*Junggrammatische Streifzüge auf dem Gebiete der Syntax*, 1882, p. 3) expressed it: "Gerade die richtige Abgrenzung zwischen psychologischen und physiologischen Kräften, zwischen Analogie und Lautgesetze... ist ihr d. i. der Junggrammatiker) Streben gewesen." Because the modern Science of Psychology, as we understand it, developed in the period immediately following the great neogrammarian outburst, it is necessary for us to examine closely the meaning of the magical term 'psychology' in the literature of this period, for at this

time it represented an integral part of the order of discourse and constituted a legitimate tool of argumentation.

The claim that the linguists of the 1870s understood the term in a commonsense fashion is attractive, but it really avoids the problem and denigrates the actual scholarly sophistication of those linguists. After all, the works of Herbart and Steinthal, as well as those of the solid German philosophical tradition, were part and parcel of the intellectual foundations of the developing science of linguistics.

It is the thesis of this paper that there was a consensus, a common understanding that is to be extracted by means of a close examination of the major statements made by the Neogrammarians such as Delbrück, Brugmann, Osthoff, and Paul, as well as those from their opponents such as Schuchardt and others. The results must then be compared with the work accomplished by Herbart and Steinthal, and an attempt will be made to demonstrate that these linguists made a careful use of the term 'psychology' even though our understanding of the term one century later is quite different. There is the distinct possibility that our century has consistently misinterpreted some of the major statements of the great Neogrammarians.

On the Concept of 'History' in 19th-Century Linguistics

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Advancement of knowledge in any scientific discipline is to a large extent dependent on the precision of the terminology it employs. Precision is easier to be achieved if a given terminology is applied to phenomena on one single time level, since in that case concept and term could be expected to have a one-to-one correspondence. By contrast, precision is much harder to be obtained when several successive time stages are involved, since concepts change and the changes are rarely reflected in their entirety in the corresponding terminology. Quite frequently identical terms continue to be used for concepts that have grown apart over a period of time to a degree that considerable adjustment would be required in order to restore the original adequacy of this correspondence.

The term 'history' so frequently invoked in the linguistic debate since the early 1800s cannot be expected to have retained a uniform meaning which might be equally applied to various stages in the development of the field. The basic assumptions and concepts have indeed undergone significant and far-reaching changes at various stages of its evolution, and the terminology evolved too, not in isolation, but together as the result of an interaction of conceptually related terms.

The present paper traces the development of the concept (and term) of 'history' in the work of the most prominent 19th-century linguists, in particular Rasmus Rask, Jacob Grimm, Franz Bopp, Wilhelm von Humboldt, August Friedrich Pott, August Schleicher, and the Neogrammarians, especially Hermann Paul, whom some have called the 'apostle of historicism'.

The difficulty for us today to fathom the precise 19th-century meaning of the term 'history' and to determine how it developed is probably due to two causes. First, we do not know (or at least have not paid sufficient attention to) the concepts with which history was most closely associated, e.g., philosophy, empiricism, etc. Thus, an investigation aimed at identifying these relationships is bound to throw light on characteristics of the term which otherwise might go unnoticed. Second, a comparison with any hope to achieve tangible results must analyse the term into its constituent components, at a particular period of time, and with a particular author. As a result, something like a componential analysis is proposed which should produce a significant revision of the traditional interpretation of linguistic theory and practice in the 19th century. It will also increase our understanding of how our own notion of linguistic history evolved and why.

Theories in Linguistics and Philosophies of Science

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There is expanding interest in the scientific status of linguistic theories. Since physical science is considered empirical knowledge *par excellence*, formulating theories in accordance with the criteria has become a major objective. This appears to be a mixed blessing. It is beneficial when it leads to improved or novel solutions to old problems, or even to new problems. On the other hand, however, a major difficulty is that the multiple and inconsistent views of science render different and incompatible criteria for developing and assessing theories. The paper attempts to come to grips with that difficulty and offer suggestions for resolving it. The argument is presented in three parts and a conclusion.

The first part outlines the three views of science dominating linguistics. Inductivism, paradigmatism, and refutationism each provide formulae for generating theories along with standards for evaluating them. In it those differing views are clarified so that the conflicting demands of each become explicit.

In the second part exemplary applications of each view in theoretical linguistics, along with the published commentary, are explored and weighed. Like theories of linguistics, histories of linguistics are also guided by views of science. A section is devoted to the historiographical applications along with

evaluations of them. Amongst those works which provide the substance of this part are Bloomfield, Chomsky, Cohen, Itkonen, Koerner, Percival, and Perry.

In the third part, an argument will be made in favor of the refutationist view, pointing out its advantages over the others. It is superior for use in theoretical linguistics, but, in modified form, it could also be applied to the history of the discipline. (Illustrations of refutationist method in both areas are included.)

The conclusion offers summarized guidelines for employing refutationist method in linguistics. It is hoped that, upon finishing this paper, a linguist will not only have unraveled much of the confusion surrounding scientific method and how it has affected the theory and historiography of this field, but will also have acquired refutationist tools by which to grapple with future theoretical and historiographical problems.

The Notion of 'Rule' and the Scientificity of Linguistics

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Although the paper originated in the writer's work on perfectivizing prefixes in Slavic and the category of aspect, it probes into the different applications of the notion of 'rule' in everyday language, in linguistics, and in physics. It presents the current controversy among philosophers over the existence of rules of language, which has become of interest to theoretical linguists as well.

Illustrations are provided from the literature of well-studied phenomena at different levels of language where 'rules' have eluded formulation, e.g., Arnason on Icelandic palatalization, Šmilauer on Czech aspect, Netteberg on Polish aspect, Bolinger on collocation in English. W. Haas put forward further complications concerning rules and *meaning*. From these and other instances it is open to doubt whether linguistics has in fact met the requirement that "rules must be known to exist" (Itkonen).

It is proposed that some light at least can be shed on the question of 'rule' by referring to the later Wittgenstein's discussion of 'following a rule'. If the reading of Wittgenstein on rules is correct and in fact applicable to linguistics, it would seem that major components of the discipline can no longer be seen as part of a positive natural science, a *Naturwissenschaft* typified by Newtonian physics, but instead as a hermeneutic methodology sharing some traits with quantum physics, and semantics being akin to meteorology, if only, as Firth suggested, because it has nothing so permanent as "the depression from Iceland".

Why the Historian of Linguistics Needs Sociology

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While many historians of various disciplines have abandoned the view that the uninterrupted progress of reason is not sufficient to explain the events and processes of scientific history, and have come to recognize that ideas are carried by social groups whose members do not always act disinterestedly, the study of these groups has nevertheless been largely ignored in a continued stress of 'great men' or in attempts to apply Kuhn's theory of changes in cosmology derived from pre-institutionalized sciences. This paper relates sociological theories of institutionalization and group formation to currents of linguistic theory in 20th-century North America.

The model here advocated assumes that there are more ideas that could be built upon (elaborated, tested, etc.) than ideas that actually are, which suggests that the promulgation of a 'good idea' is not sufficient to explain its acceptance or its popularity. At least some co-ordination of research is undertaken, and without leadership (salient tasks of which are outlined), 'good ideas' may lead to nowhere (i.e., to further work). Evidence for the necessity of leadership is provided, if only *ex negativo*, viz. the failure of ideas not carried by groups to develop and to exert influence.

Beyond accounting for the formation of groups around certain ideas, the model is concerned with distinguishing (and predicting) whether a rhetoric of continuity or a rhetoric of revolutionary novelty ('eclipsing stance') is adopted. This 'choice' indeed appears to depend upon who is recruited to a particular perspective. If established professionals constitute such a group (within an institutionalized discipline), revolutionary claims are unlikely to be made. If, by contrast, a group consists of those with no particular stake in previous theoretical perspectives—prototypically students who have not invested time in being trained or in trying to apply ideas already dominant in a field, revolutionary rhetoric is more likely to occur. However, for it to appear, its practitioners must perceive that access to recognition (particularly to publication) is unjustly denied to them. Neither any absolute discontinuity in ideas nor any discernible bias in refereeing are necessary to the perception of persecution and the escalation of claims of revolutionary new breakthroughs. A third factor (whose importance is not yet quite clear) appears to be the location of group members at institutions previously established as centers in the scientific discipline. The final factor considered is the size of cohorts. Most previous work in the sociology of science has assumed that science (number of scientists, numbers of journals, etc.) will continue its exponential growth (with the same automaticness others used to believe the progress of scientific ideas had). That this

is in fact a variable will be demonstrated with special reference to the case of Transformational Grammar. The adequacy of the theoretical model to account for recent developments in linguistics will be assessed.

A Critique of the Neglect of Linguistic and Non-Linguistic Communication by Present-Day Human Communication Researchers (reserve paper)

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It is an historical fact that human communication researchers have neglected the study of actual processes of linguistic and non-linguistic communication or semiosis. Instead, they have concentrated on the consequences of such communication. In the terminology of speech-act theory, they have studied perlocutionary acts and forces, overlooking the essential locutionary and illocutionary aspects of communication.

The paper argues—after a detailed discussion of the reasons for this neglect—in favor of the establishment of research paradigms that would pay adequate attention to the details of both linguistic and non-linguistic communication processes.

Working Group 2:

New Interdisciplinary Perspectives in Linguistics Through Nonverbal Communication Studies

Organizer: Fernando Poyatos
University of New Brunswick

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This Working Group, in which the speakers delivered full-length lectures to properly develop their topics, proved once more the growing interdisciplinary nature of Nonverbal Communication Studies (today never lacking in the programs of congresses of the social and behavioral sciences, nor in those of linguistics in the last ten years) and its increasing scientific appeal among linguists, as more and more recognize that, although verbal language is the backbone of social interaction, it cannot be studied in isolation any more. It is the responsibility of the linguist as much as of the psychologist, the anthropologist, the sociologist, or anyone studying people's behaviors, to acknowledge the existence of the Basic Triple Structure language-paralanguage-kinesics as well as the other somatic and extrasomatic systems beyond verbal language and their intimate construction with words.

Martinet reported that conversations with persons who were *totally bilingual* (i.e., who had acquired a total command of phonetic, syntactic, lexical, paralinguistic and kinesic features in two languages, whether as adolescents or adults) showed that they either feel uneasy or subject to some degree of rigidity in certain interactive situations, whether conventional or semiconventional. They further showed that an individual is not necessarily accepted by his or her new community. This raises the paradoxical question whether we should aim at "perfection" in all five communicative competences in language teaching. It further raises the question whether research in the area of interethnic attitudes with respect to the foreign speaker should not be given much more attention. *Morsbach*, a true specialist in Japanese nonverbal communication, dealt with an area that only recently has begun to be investigated in depth from the point of view of social interaction and intercultural communication, that of the functions of silence and stillness, applied in his study to Japanese society; he carefully documented the many nuances of both behaviors and their frequent misinterpretation by westerners. *Taylor's* paper, not presented by the author, complemented *Morsbach's* by discussing the many misinterpretations that occur in the realm of nonverbal communication among the Japanese, the Chinese and the people from the West, due to the different ways in which social identity is structured and conceptualized in each of these backgrounds. *Von Raffler-Engel* offered a very extensive and meticulous

documentation on the decisive importance of nonverbal communication during the job interview, disclosing a number of insightful facts of much relevance not only in the highly competitive world of business but in the realm of daily interaction as well. Finally, *Poyatos* offered an integrative and interdisciplinary approach to nonverbal communication and suggested a number of applications in several fields.

Psycholinguistic Time: A First Approximation

Tom Bruneau

University of Guam

Psycholinguistic time has received little focus to date. Current ideas about psychological time are applied to linguistic encoding and decoding behaviors. Notions concerning the temporal expansion and contraction of consciousness in the encoding and decoding of verbal and nonverbal codifications are offered. Nonverbal correlates of temporal variations of consciousness (mind-time expansion or contraction) are offered in the way of discussing facial displays of the meaningful processing of information, the use of thinking stances, the creation and use of psycholinguistic silences, and other departures from linear tempo in the processing of messages.

Some Problems About Acquisition of Communicative Behavior

By Adolescents and Adults

Hanne Martinet

The Copenhagen School of Economics and Business Administration

As the importance of nonverbal features of communication in human interaction is now recognized, one of the manifold questions that arise is that of studying the acquisition of these features, i.e. the acquisition of communicative behavior. How communicative behavior develops in infants in interaction with caretakers has now been studied for some years. However, research in the domain of acquisition of communicative behavior when it comes to adolescents and adults learning foreign languages is still scarce.

In this paper, I will outline some of the questions this problem raises, at the level of the identity of the individual, and at the level of the goals of teaching foreign languages.

The Importance of Silence and Stillness for Japanese Communication

Morsbach, H.¹⁾ & Udo, M.²⁾

¹⁾Glasgow University and ²⁾University College, London

When living in Japan, Western observers tend to notice that silence in language and stillness in movement have a more important (and frequently different) function than in their own culture. Based on previous research by Morsbach (1973), a conceptual framework developed by Poyatos (1981), and a Japanese-U.S.-Australian test administered by Wayne (1973), a description attempted of their occurrence and relative importance.

Some factors causing intercultural differences of interpretation are:—

1. The geographical isolation of Japan, leading to a culturally and racially homogeneous nation with one language.
2. The vagueness and evasiveness of the Japanese language, when compared to Indo-European languages.
3. The importance of honorific speech (when referring to superiors), and deprecating speech (when referring to oneself, members of one's group, and to subordinates.)
4. The self-imposed isolation of Japan by a feudalistic regime for over 260 years (until 1867), where a premium was placed on silence and stillness, especially in the presence of superiors.
5. The ideals of Zen-Buddhism, where silence and stillness are highly desirable states which can only be reached after persistent training.
6. The important ideal of developing long-term amicable relations with group members in face-to-face situations over many years (especially in the case of urbanized, middle-class males working in large companies). Here, silence and stillness can be correctly interpreted by other group members as conveying subtle but important messages.

Cross-Cultural Nonverbal Misunderstanding among China, Japan, and the West

Harvey M. Taylor

UCLA China Exchange Program/I.I.E.M.

Cross-cultural differences may produce everything from appreciation to animosity. The People's Republic of China, Japan, and Western countries have cultural norms which can cause mutual misunderstandings.

Social identity is one cultural difference which forms the background for the correct interpretation of nonverbal behavior. In the PRC each Chinese is assigned to a "unit" for employment; all activity and much thinking are influenced by this "unit-identity." Social identity for an employed Japanese depends

upon the prestige of the firm and upon the employee's position within the carefully articulated company hierarchy. In the West an employee's social identity depends only partially upon either the company or the employee's position within it; apparent wealth, level of education, and personal abilities also figure in at least the North American's identity within society. When people from one of these three backgrounds interacts with those from either of the other two (especially in business or educational research contacts), the potential for serious misunderstanding is great.

The nonverbal communication of these three groups reflects these differing social identities and gives rise to differences in at least four of S. Duncan's six nonverbal communication categories, namely, kinesic behavior, paralanguage, proxemics, and the use of artefacts. Examples of misunderstandings because of differences in these categories are cited and discussed.

The Perception of Nonverbal Behavior in Function of the Age and The Sex of The Rater

Walburga von Raffler-Engel
Vanderbilt University

A ten minute videotape of five job interviews was shown to 28 members (7F & 21M) of the Industrial Personnel Association who had varying years of experience in personnel work. The simulations depicted job applicants whose nonverbal behavior varied in the following ways: (1) excessive hand gesticulation, (2) lack of eye contact, (3) leaning over the table invading the interviewer's territory and facing him directly with uninterrupted eye contact (aggressive candidate), (4) nervous fidgeting, (5) leaning slightly forward over the table and establishing good eye contact (nonaggressive candidate). The age of the judge had a significant influence on general attitude toward hiring the applicant but years of experience did not. Professional women were consistently harsher judges than men. The female personnel officers attributed more weight to nonverbal behavior than their male counterparts. While all the personnel managers preferred the more aggressive candidate (3) to the nonaggressive candidate (5), this preference was slight among the males and pronounced among the females.

New Interdisciplinary Perspectives in Linguistics Through Nonverbal Communication Studies

Fernando Poyatos

University of New Brunswick, Canada

Besides the ever present cultural conditioning and the need to develop not only linguistic but cultural verbal-nonverbal fluency, language must be realistically viewed as one of the various costructured bodily systems travelling over a number of direct and synesthesis channels in social interaction. The Basic Triple Structure language-para language-kinesics, ontogenetically developed and of interchangeable semantic and grammatical value in discourse, is in turn costructured with chemical, dermal and thermal signs in varying hierarchical positions, and the total message can be properly decoded on occasions only when all systems are considered. The formal and functional classification of nonverbal behaviors is shedding much light in cultural and cross-cultural multidisciplinary studies. One area that profits from the perspectives outlined so far is the analysis of the structure of conversation, with its rules, counter-rules, simultaneous activities, and acoustic and visual pauses (silence and stillness being now recognized as communicative systems). The somatic systems thus costructured are in turn finely coarticulated in interaction and noninteraction with objectual and environmental systems, from clothes and perfume, to light, music and the built and natural environments, and even man-animal interaction merits much nonverbal research. In addition, a new interdisciplinary area of multiple applications is being born which looks to the nonverbal repertoires in narrative and dramaturgic characters and their transmission from their creator to his readers, or actors and audience, in both spatial (translation) and temporal dimensions, which generate further topics. Two more examples for this abstract: the emitting and receiving verbal-nonverbal capabilities of the handicapped, and what is being launched as Literary Anthropology, studying all systems through man's narrative.

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Working Group 3:

Developmental Linguistics

Organizer: Ch.-J. N. Bailey

Technische Universität Berlin

Presenters of position papers for the Working Group, organized by Charles-James N. Bailey of the Technical University of Berlin, were Roy Harris of Oxford University, Jerold Edmondson of the University of Texas at Arlington, Herman Parret of the Belgian National Science Foundation and the Universities of Leuven and Antwerpen, Bea de Gelder of Tilburg University, and Heng-hsiung Jeng of the Taiwan National University. Other panelists (for syntax and other subjects) were not able to be present. The occasion had been intended to provide a get-together of scholars from around the world to determine and criticize what each of the others thought about the developmental approach to linguistic theory and analysis. Despite the regretted absence of certain key scholars, this goal was realized, and friendly but sharp exchanges among the panelists characterized the event. There was less participation by members of the audience, doubtless because of their lack of familiarity with developmentalism as an approach to theory and description combining historical linguistics, including creolistics, variation theory and dialectology, etc., and extending to the various subdisciplines of pragmatics and semantax, phonetology, morphology, and the lexicon. The absence of certain invitees left the discussion weighted on philosophical and epistemological issues more than on concrete analysis.

Bailey briefly introduced the speakers and pointed out the non-monolithic diversity of "developmental linguistics." In order to facilitate the following discussions, several technical concepts were briefly characterized: explanation, prediction (illustrated), markedness, and the natural; markedness-reversal was also touched on. The unifying thread of the diverse conceptualizations of developmental linguistics is process, time, or development, it was pointed out; other matters exhibited rather more diversity. Bickerton's views on naturalness were briefly described. Several long papers intended for the Working Group but not presented because of lack of time or the absence of the author will be included in the volume which is planned for publication.

Edmondson spoke on "The biological foundation of language universals." His paper briefly reviewed a few significant findings in this area, particularly in studies related to speech sounds, and concluded that language universals are neither absolute nor random, but present or absent in an implicational patterning. He advanced reasons in favor of the view that implicational patterning

has a biological basis.

Parret discussed a number of Bailey's concepts. He favored psychology and contextuality in opposition to psychologism and contextualism—and their opposites. He named saliency, gradience, and relevance as special characteristics of contextuality. This paper bore the title, "Developmentalism: How to escape psychologism and/or contextualism."

After a lunch break, de Gelder (in "Coming to mind") discussed the philosophical and other disadvantages of non-developmental approaches to the study of language and mind by psychologists. Citing a wealth of views, she concentrated on the problem of cognition and the move by some theorists to reduce language capability to knowledge in the name of true science. She discussed a series of phases in cognitivist strategies culminating in a theory of learning and ultimately a view of the universal properties of language.

Harris, in "Saussure and the dynamic paradigm," suggested that Saussurian studies have not generally emphasized the most crucial aspects of Saussure's work with respect to general theory and analysis. Speaking of two concepts of systematicity and the role played in Saussure's non-chronological concept by the notion of *valeur* in economics, Harris pointed out that Saussure was intent on combatting both nomenclature and a mathematical conceptualization of systematicity—which for Saussure was not the same as patterning but was a decidedly determinate and closed conceptualization.

In a very full study of the acquisition of Chinese by children, Jeng demonstrated the gradualness of the transition from babbling to controlled sound-production in the data; he argued that the analysis also has to be gradient. The data included tones as well as sound segments. Jeng showed that three Chinese children acquired their vowels in different orders and thus in different implicational patterns. He argued that differing acquisition strategies might lie at the root of such differences. This paper was titled, "The relevance of child language for a developmentalist theory of language."

The ensuing discussion of almost two hours proved very spirited. Harris played a critical role and concentrated his first remarks on the implicational universals discussed by Edmondson and given a structure-forming role by Bailey. He contended that an error or mistake not unlike Saussure's was being committed and maintained that such aspects of developmentalism were not adequately or justifiably defined. When Bailey pointed out that they could not be vacuous because they made possible the confirmed predictions of linguistic development that have been made, Harris claimed they were otiose substitutes for real explanation and predictive procedures. Harris expressed a general rejection of rules and favored common-sense approaches to analysis and explanation.

De Gelder pointed out the contradiction in Bickerton's advocacy of Fodor's claim that you have to know a language to learn a language. Bailey provoked a long discussion in concentrating on the problem (in Jeng's paper) of moving from a demonstration of gradience in one's data to proving that the

analysis has to be gradient. Jeng responded that analytical boundaries can be demarcated in the gradient data on the basis of the units of adult language eventually arrived at by the child. The issues of contextualism were debated at length, as was Parret's views on the "biological metaphor." And the ontological and epistemological status of implicational patterns, rules, and similar constructs was recurred to several times in subsequent discussions.

In retrospect, it is clear that the relevance of child-language studies to linguistic theory discussed in de Gelder's and Jeng's papers—and put in a new perspective by Bickerton's *Roots of Language*—did not receive as full an airing as was possible. This was Bailey's fault. The lack of disagreement on various developmentalist positions taken by Bailey in a number of papers might suggest agreement; but it could also be due to our failure to bring the matters up. It was pointed out in the Working Group and should also be pointed out in this *résumé* that the fullest summary of developmentalist views is to be found in Bailey's *The yin-and-yang nature of language*; see also further work referred to there. (This work, however, represents the views of the organizer of the Working Group and does not correspond entirely to the work of those present at the session or to those who had been invited but could not attend.)

The Relevance of Child Phonology for a Developmentalist Theory of Language

Heng-hsiung Jeng

Department of Foreign Languages and Literature, National Taiwan University

Since Jakobson (1968), quite a few studies on the productive aspect of child phonology by Moskowitz, Hsieh, Ferguson and Farwell, Menyuk, Kiparsky and Menn, Ingram, Jeng, Waterson, Peters, Hawkins, Li and Thompson, and Clumbeck have brought to light the importance of the holistic, dynamic and variational phenomena in the acquisition of phonology. Such phenomena, being so diverse and untractable, have stretched out of shape the minilectal and variationless models set forth by the structuralists and transformationalists. Consequently, there is an urgent need for a new model which can account for not only these phenomena, but also the development of phonology from child language to adult language in a natural way. It seems that the developmentalist theory of language as proposed by Charles-James Bailey, with its concepts of implicational relationships, variation, gradience, markedness in terms of the Greenbergian dynamic and typological principles, connatural and abnatural changes, can explain and predict the above-mentioned phenomena rather satisfactorily.

Three problems regarding the above-mentioned phenomena of child phonology are especially difficult for Chomsky's nativist, static and minilectal theory to cope with: 1) Why children should make particular patterned errors at a particular stage; 2) Why there are implicational relationships between different

stages; 3) Why children should have different strategies in acquiring phonology. The third problem is equally difficult for the Jakobsonian model to handle.

Developmental Linguistics

Charles-James N. Bailey

Developmental linguistics is both a return to a more diachronic age of analysis and a resolution of the dialectic between Platonic transformationalism and empiricist glottometry. The speaker defines the philosophical and linguistic issues constituting the background that differentiates developmentalists from the others. To begin with, he offers a broad view of three kinds of reality—process, relations, and continuing things—and their corresponding dimensions of linguistic analysis—temporal development, comparative patterns, and structured systems. The emphases of the three approaches are noted in connection with these basic dimensions of reality as well as with innateness, gradience, and theoretical explanation and prediction. A basic distinction between the results of connatural development (when a system is not in contact with another) and abnatural development (due to system contact) provides the basis for the *yin-and-yang* balance necessary to the nature of language and linguistic health. It is claimed that innateness is not the main issue of linguistics; understanding the nature of language—which requires understanding how linguistic structures develop—is. It is claimed that, since only temporal development and comparison provide a characterization of what is more marked and what is less marked in languages, understanding these matters and many other aspects of the nature of language is necessarily excluded from the purview of synchronic-idiolectal approaches. The position is taken that, since the human organism is attuned to much of the gradience found in linguistic data, the models of linguistic analysis must be just as flexibly tuned. A dimmer provides all the outputs of an on-off switch and more, but the latter cannot generate all those of the dimmer. In approaching the issue of innate cognition, it is claimed that pre-wired is far from being the same as pre-programmed.

Taking the position that theory requires explanation and prediction to be theory, the author devotes a good deal of space to showing that these are impossible in a synchronic-idiolectal approach and possible in a developmental approach. It is also shown that the current distinction between competence and performance leads to a dead-end in crucial matters and, more generally, that the whole synchronic-idiolectal approach necessarily leads to intolerable paradoxes, of which not a few are mentioned. Some of these involve accounting for the way new languages come into being and, more generally, why change occurs at all. When the natural implicational patterns created by natural linguistic development are disturbed (by borrowing and in other ways), connatural

changes occur to change the *implicans* or to restore the *implicate*—and the theory can in principle predict which of these will take place. The power of such patterns is shown in various mirror-image rules exemplifying the principle of the hare and the tortoise. Fundamental to understanding the nature of language as such implicational patterns are, they can in no way be understood in synchronic-idiolectal approaches that maintain the autonomy of linguistics (or of various components of linguistics). Linguistic explanation must rather be sought in neurobiology and in social factors that directly affect linguistic development. Predictions about the future development of linguistic systems can be made on the basis of our understanding of natural patterns (since these involve comparing systems, they can form no part of a consistently minilectal approach), even when the bioneurolinguistic explanation of the patterns is still wanting.

Developmentalism, Psychologism and Contextualism

Herman Parret

Belgian National Science Foundation

Linguists, in the predominant linguistic paradigms (structuralism and Chomskyan linguistics), stress the *autonomy* of their discipline: they reject data and explanations from the "outside" (psychology and sociology of discourse and conversation, logico-philosophical deductive reasoning on language, historical and evolutionary data of language development) as being beyond the concern of the linguist (cf. C.J. Bailey, *On the nature of language*, ms. pp. 20–23). The centrality of the notion of *competence* has as a side effect to eliminate all kinds of linguistic phenomena which do not fit into the model proposed and its proper methodology. Thus it still remains an important issue in linguistic theory to make explicit the underlying view of the "boundaries of linguistics". Pragmatically oriented linguists on the other hand, try to enlarge the scope of linguistics by introducing *socio-psychological* categories (intentionality, mental states, typologies of actions reflected in grammar) and *contextual* information. It will be argued that this reorientation of the conceptual apparatus of linguistic theory is of absolute necessity, but that psychologism and contextualism should be avoided; these are pejorative terms to be distinguished from "psychology" and "contextuality".

The purpose of the paper is twofold: (i) to define psychologism and contextualism listing the identification criteria of these doctrines to be avoided; (ii) to suggest how developmentalism (in the version C.-J. Bailey proposes in his recent publications) can incorporate psychological categories and contextual information as relevant to the explanation of the empirical data reconstructed, escaping at the same time the non-adequate types of psychologism and contextualism.

Language Universals and Biology

Jerold A. Edmondson

University of Texas at Arlington

This paper assess the degree to which language universals may rest on prewiring instead of preprogramming. I argue that the safest universal to base on biology is one that results in cross-language variation, for instance, of the implication type, not absolute universals obtained from studies of one language.

In support of this claim I submit evidence from studies about selective adaptation of voicing as well as new work on dichotic listening and the voicing distinction. From morphology I report on Mayerthaler's concept of *sensory accessibility* and *cross-environmental constancy* of perception. Data and results of studies from many languages are used to support these claims.

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Working Group 4:

Universals of Linguistic Action

Organizers: Florian Coulmas,¹⁾ Hartmut Haberland,²⁾
Jacob L. Mey³⁾ & Jef Verschueren⁴⁾

¹⁾Tokyo, ²⁾Roskilde, ³⁾Odense and ⁴⁾Berkeley

Papers submitted by: Thomas Ballmer, Bochum; Steven Davis, Simon Fraser; Ivan Fónagy, Paris; Thorstein Fretheim, Trondheim; Henk Haverkate, Amsterdam; Thomas R. Hofmann, Toyama; Naomi Miyake, San Diego; Joyce Penfield, Rutgers; Danny D. Steinberg, Honolulu.

Discussants: Danny D. Steinberg, Naomi Miyake, Thomas R. Hofmann, Jens Allwood (Gothenburg), Joyce Penfield, Henk Haverkate, Steven Davis.

Chair (morning session) Florian Coulmas; (afternoon session) Jacob L. Mey. Introductory remarks: Florian Coulmas; Concluding statement: Jacob L. Mey & Hartmut Haberland.

Jef Verschueren, Thomas T. Ballmer, Ivan Fónagy and Thorstein Fretheim were not able to attend the conference.

"Universals are unicorns in an impossible world."

The study of universals has a long tradition in sociology, anthropology, and linguistics. Landmarks of this tradition in linguistics are the conferences in Dobbs Ferry, New York, 1961 (where the emphasis was on universals of language), and in Austin, Texas 1967 (where the emphasis was on universals in linguistic theory), as well as the Stanford project on Language Universals.

However, while the investigation of language universals has flourished in recent years, the study of pragmatic rules of verbal activity has not been part of this boom. One reason for this neglect may be that universals research in the area of linguistic action is more difficult from a methodological point of view than in phonology, morphology, or syntax. The study of pragmatic rules requires either a very good command of the language under investigation, or extremely refined elicitation and observation techniques.

Another reason for the little attention that has been paid to universals of linguistic action is that, in spite of the obvious significance of cultural variables in pragmatics, many linguists were content to leave the study of such universals to the philosophers of language, whose approach tends to be speculative rather than empirical.

Speech activities do vary strongly across languages and cultures, and this kind of variation can hardly be appreciated on the basis of speculation or conceptual analysis alone. Much careful observation and research into this varia-

tion is necessary in order to further our understanding of what people do with words. The working group on 'Universals of Linguistic Action' was dedicated to this endeavor.

The search for universals of linguistic action raises a number of theoretical and methodological questions inviting a variety of different approaches. Some of these questions are:

- Is it possible to define the notion of 'universal of linguistic action' in a meaningful way? If not, why not? If yes, how?
- What kind of universals are universals of linguistic action? Are they empirical generalizations with statistical validity, or categories formulated *a priori* as definitional criteria of human language?
- How do universals of linguistic action relate to universals at other levels of language structure? Do they impose any structural requirements on those levels, or are they, in turn, subject to structural constraints?
- How can universals of linguistic action be discovered? (In the armchair? In the lab? In the library? In the fields?)
- What distinguishes universals of linguistic action from other regularities of linguistic action, and in what sense are the former more basic to language than the latter?
- Is it possible to formulate universals of linguistic action as testable hypotheses about language, i.e., as hypotheses that are to be considered refuted if a given feature or regularity predicted by one such hypothesis fails to prove valid for at least one language?

At present, the answers to questions such as these are far from obvious. However, the papers discussed in the working group suggest that research along the lines of these and similar questions will eventually help us to understand the intricate relationship between cultural multiformity and species-specific universality in linguistic action.

In our working group session, all the seven contributions addressed universal questions of linguistic activity, thought not in the same manner. Mainly, there were two approaches:

- (1) How to do things with words, universally (roughly, in the Greenberg tradition);
- (2) How to represent this doing of things with words, in words, universally (in what could be called the Chomsky tradition).

Without offending anyone, we would assign the contributions by Miyake, Hofmann, Allwood and Penfield to the first focus of interest; those by Steinberg, Harverkate and Davis to the second. Needless to say, these assignments are neither water-tight nor value-judgmental. But the distinction may be useful for the purpose of understanding what we were looking for in our working group. Briefly, the question can be summarized thus:

Are universals of linguistic action the same as pragmatic universals, or not?

We will take this question up again below. But let's first consider some of the possible positions with respect to the two issues raised above.

The first approach (the Greenberg tradition) comprises various positive answers to questions such as: What can one do in language A, language B, language C, . . . , which is the same, cross-languagewise? (e.g. 'quoting', such as Penfield investigates, or Allwood's communication patterns).

The second group of answers (which is tied to what we have called the Chomsky tradition of model-building) looks into the necessary constraints on the use of language, that our representations try to capture. (We are not saying that the representations *are* the constraints, or that there are no other ones). In this way, a paper such as Steinberg's sets up a potential framework embodying what is minimally necessary in order to understand understanding.

While necessity, in itself, is not a linguistic universal (witness Davis' paper) universals (both pragmatic and linguistic) have to incorporate an element of necessity (as we stated above): they are not just found by introspection. While the force of the first approach (cf. above) is its reference to a concrete societal framework, we still need to prove those concrete conditions to be necessary, as stressed by the second approach.

In another way, one could say that universals of linguistic action are what is universally *possible* or realized (which is what Haverkate, e.g., is most interested in), whereas universals of pragmatics address the question of what is *impossible*, and why. While, of course, *all* constraints operate on the assumption that something is not possible (illogical, ungrammatical, etc.), the *specific* of a pragmatic universal is that it is hidden, so to speak, behind the "official representation": it is a condition that surfaces at certain points, in individualized shapes. For example, if questions are not asked in a society (and there seem to be societies, especially North American Indian ones, which come close to that), "The" question is not a universal of linguistic action. (Or compare the use of laughter in Japanese that was quoted by several discussants during the afternoon sessions.)

Thus, if pragmatic conditions restrict the use of what *we* call questions, then we have to represent our question in another way. The act of eliciting information about unknown things subsumes one type of question. The pragmatic conditions are those which specify what kinds of question (taken as an abstract super-concept) are allowed.

The use of terms such as 'universal pragmatics' can therefore be misleading. Pragmatics could be called the science of the "Hidden Impossible". Finding out why a particular utterance, or type of utterance, is impossible, is thus not a universal matter. It may depend on a very concrete 'taking position', a change in conceptual point of view, as Miyake calls it.

The papers that were discussed in our working group strongly suggest that the quest for universals of linguistic action should take its point of departure in different traditions and, research areas. One of the important questions is whether universals of linguistic action are such by definition, and thus logical

consequences of the definition of 'speech act' or 'cognitive processing strategy', or if they can be empirically generalized from observations of speakers who use a specific language in a specific culture. In the first case, they will be independent of specific language or cultures, in the sense that one can ask a question in Igbo or English or Kwakiutl without changing its character as the speech act of question. In the latter case, we might discover that linguistic actions, in the same way as phonological, morphological, and syntactical structures, do not vary in all conceivable ways and to all possible degrees, but only within certain, not necessarily logically determined or obvious limits. The linguist's task would then be to state those limits, within which linguistic actions can vary from language to language and from culture to culture.

Universals of Linguistic Action

Thomas Ballmer

Ruhr-Universität, Bochum

What are the problems of that new field "Universals of Linguistic Action"? A personal experience investigating pragmatics across culture is discussed and a Dilemma of Universal Pragmatics stated.

Two requirements for the methodology and theory of universal pragmatics are stated and a way is shown to fulfil these requirements realistically. A structure of all pragmatically relevant processes is found which serves as a map to aid orientation in an otherwise pathless territory. There is a discussion as to what methodological assumption is implied by the term linguistic action and this is related to some more general views. At the end an example of a phenomenon of universal pragmatics is expounded: the speech act classification of Tiwi, an Austronesian language.

Disjunction and Illocution

Thorstein Fretheim

University of Trondheim, Norway

The idea that there is just one truth-functional disjunctive connective for natural language, the *inclusive* 'or' (cf. Pelletier, 1977), and that the special 'exclusive' sense of 'or' should be accounted for in pragmatic terms, is seen to receive strong support from an investigation of what a disjunctive connective means when it links coordinate phrases or sentences disjunctively in *non-assertive speech acts*. It is universally true that someone ordering or requesting a person to do (either) X or Y gives that person the choice between X and Y, that an offer of the type 'You may have X or Y' likewise means that the

addressee can choose between the alternatives, that a disjunctive promise leaves it to the promisee to pick the alternative that suits him/her best, etc.

Assertive speech acts are rather special in that the assertion of $p \vee q$ pragmatically entails that the speaker presupposes the truth of the (inclusive) disjunction but is unable to tell which disjunct proposition, p or q , makes it true. In exclusive disjunction there is always a *correct* choice, as exactly one of the disjuncts represents a true proposition. The notion of *correct* alternative is not applicable to the speech act categories of directives and commissives (Searle, 1979) involving disjunction, nor to non-assertive subordinate clauses, e.g. 'I shall never speak to you again, if you've told George or Marsha' vs. 'I shall never speak to you again, because you've told George or Marsha'.

Strategies in Linguistic Action

Henk Haverkate

University of Amsterdam

Strategies in linguistic action can be described in terms of a componential analysis of the speech act. The subacts to be distinguished are the phonetic, the illocutionary, the referring and the predicating act.

After a conceptual introduction, some concrete examples are presented in order to demonstrate which kinds of strategies speakers develop in performing each of the four subacts distinguished. At the referring level, for instance, the category of vocatives and the distinction between polite and familiar forms of address is discussed; at the illocutionary level particular attention is paid to the strategies inherent in the performance of impositive speech acts. In relation to this, certain formal and strategical properties of indirect speech acts will be analyzed. Finally, the hypothesis is put forth that languages might differ as to the proportion in which their speakers prefer to make use of phonetic, illocutionary, referring or predicating strategies.

The Effect of Conceptual Point of View on Understanding

Naomi Miyake

University of California, San Diego

Language, when seen as a tool of communication, must have power to allow the listener to build a model of the speaker. This model building requires that the listener can infer, from the speaker's language, from which point of view the speaker describe the topic matter. Language, universally, has to have a potential to convey speakers' points of view. How we manipulate our point of view in language and how it relates to our thinking process is an important

topic in considering universal aspects of language use.

I asked several pairs of people to figure out how a sewing machine works while I videotaped their conversations. In the first sessions, they were not allowed to examine a sewing machine, but they could construct drawings and frameworks. The task was extremely difficult. One important aspect of the subjects' performance was the location of the conceptual point in space from which the speaker appeared to be viewing the machine (C-POV). In this paper I provide a framework for understanding a physical device and connect my observations to the framework. Changes in C-POV can be regarded as a mechanism to promote the process of understanding.

Universals of Quoting Behavior

Joyce Penfield

University of Texas at El Paso

This paper examines a specific type of linguistic action which has been documented in most societies of the world but not yet well-understood analytically or in terms of universals. Quoting behavior, or the act of using the given words of a respected authority in one's society, is examined here in the ethnographic sense. The principal form of quote dealt with in this paper are proverbs. Some discussion is given to the theory of quoting behavior and how it should be studied in line with the interest of the working group regarding data collection.

Drawing on an extensive study of quoting behavior in a selected African society using a functionalist-empiricist approach, the author suggests five functional properties of quotes which are universal to this type of linguistic action. Some comment is made finally about the universality of quoting behavior in general and how the universal properties suggested may be manifested differently from one society to another as a reflection of differing cultural orientations.

Semantic and Pragmatic Universals in Sentence Comprehension

Danny D. Steinberg

University of Hawaii

In ordinary conversation, the interchange of sentences is often very rapid. Listeners do not wait until an entire sentence is uttered before they begin to interpret what has been said and to formulate a response. What happens is that as words are being received, the listener projects a possible semantic interpretation. Additional words serve to confirm or not confirm that interpreta-

tion. In the latter case a new interpretation will need to be projected. Listeners are able to project possible semantic interpretations based on what they know about language and the world and what they might expect given the relative frequency of previously occurring linguistic and world events and situations.

This research will outline how persons project semantic interpretations, i. e., propositional and intentional structures, through the application of strategies which use semantic and pragmatic knowledge. While many strategies are necessarily language specific, the essential functions of the basic strategies will be shown to be universal in nature.

Proto-Linguistic Universals in Speech Acts

Ivan Fónagy

Centre National de Recherche Scientifique, Paris

The maxims of cooperative communication prompt the hearer to look for some other meaning for blatantly false statements such as "Boundless humility builds on my shoulders a snail shell, big and silent" (Arpad Toth, Frindge of the Forest). Conversational maxims, as defined by Paul Grice, make it clear why the listener is compelled to impose his own interpretation on such statements, whereas there is no question of such further elaboration in the case of well-formed sentences. These maxims do not, however, explain what it is that enables the hearer to interpret deviant statements, and why such deviant statements are made and readily accepted.

I would suggest that rule transgressions are inherent in speech acts on all linguistic levels, and based on a code of rule transgressions. This code is universal. A "grammar" of deviances enables the speaker or the poet to express contents that could not be conveyed by means of sentences generated by the grammar of any language.

Working Group 5:

Linguistic Theory and Language Acquisition

Organizer: Yukio Otsu

Tokyo Gakugei University

Generative grammar is said to have given impetus to the study of language acquisition.* In reality, however, there has been little research to date that attempts to connect seriously linguistic theory and language acquisition. The Brownian studies of language acquisition carried out mainly in the 1960s (e.g., Brown 1973) used distributional analysis of the children's performance corpus as a major analytical tool and thus failed to reveal many of the important properties of child's developing grammars, though the researchers used generative framework in their attempts to write the grammars. In the 1970s, there was an extensive shift of attention in the field of language acquisition from grammatical development to the cognitive, pragmatic, and sociolinguistic aspects of language development. Although it certainly is welcome to study various aspects of language acquisition from various viewpoints, these new trends have often misled the field of language acquisition by misrepresenting some of the basic tenets of generative grammar concerning the nature of language acquisition. For example, some people who have been working on the so-called "motherese" (e.g., Snow 1979) often misrepresented the claims of generative grammar concerning the relation between the nature of input to children and the necessity of postulating innate linguistic endowments by confusing two different notions, i.e., "poverty of the stimulus" and degeneracy of the stimulus." See Chomsky (1980: 42, 43) for discussion.

In recent years, however, there has been a growing amount of work that attempts to connect seriously linguistic theory and language acquisition, and good sample of research in this area has been collected in Tarkolian (1981). It must be mentioned that there is an isolated, but very important, forerunner of this relatively recent research, i.e., Chomsky (1969).

In our workshop, six scholars presented papers based on their experimental work. Their names and titles are as follows:**

Helen Goodluck (University of Wisconsin/Madison): Children's Interpretation of WH-constructions

Kazuko I. Harada (Kinjo Gakuin University): The Acquisition of Japanese

* I will use "language acquisition" and "language development" interchangeably, although the two must be distinguished in more technical expositions.

** The abstracts of these papers follow this report.

Case Particles: A New Look

Henry Hamburger (George Mason University): Early Superlative Comprehension

Barbara Lust (Cornell University): On the Notion 'Principal Branching Direction': A Parameter of Universal Grammar

Alec Marantz (Harvard University Society of Fellows): The Connection between Grammatical Relations and Lexical Categories in Language Acquisition and Linguistic Theory

Thomas Roeper (University of Massachusetts/Amherst): How Children Acquire Bound Variables

In addition to these people, we also had the following scholars as designated discussants:

Masayuki Ike-uchi (Aichi Prefectural University)

Noriko Terazu (Toyama University)

Henk van Riemsdijk (Tilburg University)

Thomas Wasow (Stanford University)

As is clear from the titles of the presented papers, various important aspects of children's developing grammars were taken up, and it is hardly possible to summarize what was discussed within the confines of the given space limit. Therefore, in the rest of this report, I would like to take up three issues that are extremely important from the viewpoint of those who are working on language acquisition from the perspective mentioned just above.

First, I would like to discuss the problem of emergence of innate linguistic endowments. There are various logically possible ways in which innate linguistic endowments could emerge. It is logically possible that they emerge just after birth. The study of Eimas and others concerning the infants' ability to detect the difference between syllable-initial [p] and [b] (e.g., Eimas *et al.* 1971) is an attempt to explore this possibility in phonology. It is also logically possible that there are innate linguistic endowments whose emergence is maturationally controlled and is delayed until a certain maturational stage. Still another possibility is that the emergence of the innate endowment is logically dependent on the acquisition (or emergence) of a certain other entity. For example, part of the island constraint which in effect prohibits the extraction of elements out of a relative clause cannot logically emerge until the acquisition of relative clauses is completed. My own experimental work (Otsu 1981) lends support to this possibility. Namely, my data have shown, using sixty English-speaking children from three to six years of age, that once the child learns relative clauses he recognizes that it is not possible to extract elements out of these clauses. There are other logical possibilities concerning the emergence of innate linguistic endowments as well.

Among the papers presented in the workshop, Roeper's and Marantz's papers, in particular, directly address themselves to the above-mentioned issue.

Roeper attempted to show that some of the properties of Universal Grammar, which by hypothesis constitute part of innate linguistic endowments, appear quite early by using such sentence pairs as *who thinks he wears a hat* and *who does he think wears a hat*. Marantz provided an explanation for why emergence ("acquisition" in his terminology) of grammatical relations is delayed until the age of five or six by claiming that the young child cannot take verbs as functors. Ike-uchi in his comments suggested the possibility that the emergence of grammatical relations is delayed because relational concepts in general are difficult for the child to handle.

The second issue that I would like to take up in this report is the importance of cross-linguistic data of language acquisition. The importance of such data has already been recognized by a number of researchers of language acquisition. Cross-linguistic studies become all the more important if we attempt to connect language acquisition with current linguistic theory in the framework of generative grammar (e.g., Chomsky 1981). This is mainly because of the introduction of the notion of parameters. Lust's paper addresses itself to this problem, taking up what she calls "Principal Branching Direction" as a candidate for a parameter in Universal Grammar. Terazu in her comments suggested some structures that might be worth considering in constructing experimental stimuli. Harada summarized major previous findings concerning the acquisition of Japanese case particles, and suggested structural as well as functional views in which these findings can fit well. It is hoped that more research will be conducted concerning the acquisition of core mechanisms of Japanese grammar, thereby providing a basis for cross-linguistic research.

The importance of considering processing mechanisms in language acquisition research is the third issue that I would like to take up. Goodluck proposed a processing mechanism concerning *wh* and *that* relatives by which her experimental data would follow. Keiko Sano from the floor suggested an interesting alternative account of Goodluck's data. Hamburger claimed that the child's difficulty in comprehending phrases such as *the second biggest ball* can best be accounted for in the procedural semantics framework. There is obviously no direct access to children's grammars, and we are always forced to infer their nature through various kinds of performance. Since language processing mechanisms certainly affect the child's performance, it is no doubt important to gain more insight into the child's processing mechanisms and their development.

In the course of the discussion, it has become evident that the kind of language acquisition research represented by the presented papers constitutes an important research field in which we can expect to gain more insight into the nature of language acquisition. As van Riemsdijk and Wasow suggested, the study of language acquisition must keep a close contact with linguistic theory since we are always provided with a lot of interesting research topics by linguistic theory. I agree, and most of the people who attended the workshop would also agree, with Wasow when he said that it is time to consider seriously the construction of a theory of language acquisition.

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Children's Interpretation of WH-constructions

Helen Goodluck

I will present results of experimental work on children's interpretation of relative clauses in English, focussing on the development of rules and procedures for interpreting *wh*-clauses. The analysis will be based primarily on a study on which I am collaborating with M. Krause.

Data from a toy-manipulation experiment carried out by M. Krause on 3-5 year old children's comprehension of relativization from subject, direct object and dative object positions show (1) an error pattern suggestive of on-line gap-filling for *wh* relatives but not for *that* relatives; (2) a marked facilitating effect of pied-piping with *wh* relatives from 4 years onwards. We interpret this data in the context of a language-processing model in which the relative types (*that/wh*) involve different memory stores and in which *wh*-pronouns are plugged into a (potential) gap on-line as a (potential) gap is located in the incoming string. Information concerning potential gaps derives from the lexicon.

Because many of the differences we observe can plausibly be interpreted in terms of differences in processing load and processing mechanisms for the relative types we have studied, the data permits only limited extrapolation to the development of the competence grammar. The results are consistent with theoretical accounts that propose different operations in the derivation of *that* and *wh* relatives, but do not strongly support such an account for these children's grammars unless it is assumed that the processor and the competence grammar stand in a one-to-one relationship (an assumption we do not wish to stand by). If we do suspend disbelief in the use of performance data in in-

ferring competence, the developmental changes we observe in the treatment of *that* and *wh* relatives and in the handling of pied-piping fit a picture of the development of rules for relatives along the following lines: a WH-movement rule is acquired at around 3 years; the rule initially is restricted to 'move NP' where NP is dominated by a category of the main projection (in the sense of Koster, 1978); the rule subsequently generalizes to NP and PP, and is relaxed to permit movement of an NP that is not dominated by a category of the main projection (i.e., to permit stranding).

The Acquisition of Japanese Case Particles: A New Look

Kazuko I. Harada

There have recently been accumulated a considerable amount of experimental studies on the acquisition of Japanese syntax, particularly such constructions as simple (transitive) sentences, passive sentences, cleft sentences, and relative clause constructions. While it has been shown that the results obtained could be explained largely in terms of word order and/or particles, there still remain some facts to be accounted for. This paper will show that the facts that would not be otherwise accounted for can be given a natural explanation when the functions of case particles that have been overlooked in the previous studies, i.e., discriminatory function and discourse function, are taken into consideration.

Early Superlative Acquisition

Henry Hamburger and Stephen Crain

The empirical search for children's preferences among syntactic structures is potentially important for linguistic theory. This point has been argued by Roeper, who, along with some of his students, has achieved results that point to a preference for flat, conjoined structures. However, two methodological obstacles confront the work. First, in the choice of linguistic materials, the degree of semantic complexity can be confounded with syntactic complexity. Second, practical demands of the task by which one attempts to tap competence may involve the subject in complex planning or formation of procedures. These additional factors—semantics and procedures—are not only of independent interest, but also must be analyzed to enable the unconfounded study of syntax.

Superlatives in English interact in important ways with other parts of the

noun phrases in which they appear, notably ordinals and possessives. We will examine semantics and procedures for these interactions and present empirical studies of 4- and 5-year-old children that support our view of what aspects of noun-phrase handling present difficulty during acquisition.

On the Notion 'Principal Branching Direction': A Parameter of Universal Grammar

Barbara Lust

In this paper, we define the notion 'Principal Branching Direction' (PBD) of a language and argue that both theoretical and empirical considerations document it as a "parameter" of Universal Grammar (UG) in the sense defined by Chomsky (e.g. 1981). That is, the notion PBD is defended as a principle which serves as a fundamental organizing principle in grammar for all natural languages, and also provides a fundamental organizing principle in human competence for first language acquisition.

PBD is argued to be a general organizing principle in grammar for natural language in that a number of structural principles of the syntax of complex sentence formation cohere with the PBD of a language. PBD is a 'parameter' of UG in that its precise value (right or left, in right-or-left branching languages) must be set by experiences of a specific language, although the principle itself applies universally.

PBD is argued to be a fundamental principle in first language acquisition on the basis of empirical, comparative, cross-linguistic study of first language acquisition of complex syntax in English, Japanese, Chinese, Sinhalese, Hindi and Arabic. Empirical results are summarized from studies in the set of 6 languages, supporting the claim that children in all languages consult the PBD of their language in early stages of syntax acquisition, resulting in principled differences across the languages according to their PBD.

Specifically, PBD is shown to provide constraint on anaphora in the languages studied. In all languages, children constrain the directionality of early forms of anaphora, but the direction is shown to vary significantly with variation in the PBD of the language being acquired.

Parameter setting of PBD is argued to provide critical structure—dependence in the acquisition of anaphora across languages, and thus to help to significantly explain its acquisition.

The Connection Between Grammatical Relations and Lexical Categories in Linguistic Theory and Language Acquisition

Alec Marantz

Recent *explanatory* accounts of grammatical relations (GRs) (e.g., those in Montague Grammar and the Government-Binding theory) assume only one or two basic GRs. The usual grammatical relations, such as "subject" and "object," are defined in terms of the basic relation(s) and the type of constituent with respect to which the relation(s) is/are held. For example, a "subject" is an item which bears a basic GR with respect to a verb phrase; the object of a verb bears the basic relation with respect to the verb. These theories of GRs relate the basic GRs to basic semantic relations, such as the relation between a semantic/thematic role "assigner" and the constituent whose semantic/thematic role in a sentence is determined by the role assigner.

Studies of language acquisition indicate that children use semantic roles in their early grammars for some of the same functions served by GRs in the adult grammar. For example, experiments by the author suggest that children begin speaking English by ordering words according to the semantic roles they bear, while word order is (partially) determined by GRs in the mature grammar. The question arises, why don't children start the language acquisition process exploiting semantic and/or grammatical relations as in the adult grammar? Why do children employ semantic roles in their stead?

In this paper, I will review the evidence, from my own work and from others', that children do, in fact, depend on semantic roles for their early grammars to serve some of the same functions that GRs perform later in acquisition. Then I will explore two possible explanations for this phenomenon. First I suggest that the young child's inability to treat certain semantic functors, most notably verbs, simultaneously as substantives and as functors leads him to adopt strategies for language use that apply all substantives ("names"), including both nouns and verbs, as a class. For example, the child will develop rules for ordering subject-verb-object sentences according to the roles of the subject, verb, and object (e.g., agent-action-patient), because, unable to treat the verb as both functor and substantive simultaneously, the child cannot exploit the relational quality of the verb in his grammar and must therefore develop a strategy for ordering three elements of the same class, that of "names." This hypothesis about language acquisition correctly accounts for certain category errors common in early speech (e.g., "more up," "allgone eat"). It also explains the difference in behavior between the true functors in a child's vocabulary (e.g., "allgone") and the child's verbs, which I claim do not serve as argument-taking words for the young child. Second, I hypothesize that difficulty with complex constituents could prevent children from a correct rela-

tional analysis of transitive sentences. The distinction between subject and object depends on treating the combination of verb plus object, a complex constituent, on par with an intransitive verb. If, because he could not treat the verb plus object as a complex functor, a child were unable to exploit the correct relational difference between subject and object in a transitive sentence, he might depend on the semantic role differences in his grammatical analysis of such a sentence.

How Children Acquire Bound Variables

Thomas Roeper

Children's stunningly rapid acquisition of language can be explained if we can show that they use abstract principles to determine—or trigger—knowledge of a wide range of structures simultaneously. An illustration follows. It has been shown that children have knowledge of c-command in control (Goodluck 1978) and anaphora (Solan 1978, Lust *et al.* 1981). They know that *he* and *John* corefer in (a) *John thinks he is here* but not in (b) *He thinks John is here*. Otsu (1981) and Phinney (1981) have shown that children must have movement traces by three or four (*What did John hit* (trace)).

We have evidence that children perceive the interaction of c-command and trace in their differential responses to sentences like (c) *Who thinks he has a hat* and (d) *Who does he think has a hat*. The latter sentence does not allow coreference because, though *who* and *he* are in the same order in both sentences, *he* c-commands trace in (d) just as it c-commands *John* in (b): *who does he think trace has a hat*. In a series of experiments with interviews and pictures we show that one group of 5 year olds has the distinction while the other does not and allows coreference in both (c) and (d). The group that permitted coreference, moreover, gave a variable interpretation to *who* and *he* (a set of people are involved). This is predictable since *wh*-traces must be variables. The group who allowed coreference in (c) and (d) seem to have a grammar where there is PRO instead of trace as a subject: *who does he think PRO has a hat* (like: *he thinks he has a hat*). Coreference is allowed but variable interpretation is not necessary. PRO has fewer constraints so it is a natural primitive structure for empty categories during acquisition.

Papers and comments presented at this workshop and the one on generative syntax have been collected in *Studies in Generative Grammar and Language Acquisition*, edited by Yukio Otsu, Henk van Riemsdijk, Kazuko Inoue, Akio Kamio, and Noriko Kawasaki (available from Noriko Kawasaki, Division of Languages, ICU, 10-2 Osawa 3-chome, Mitaka, Tokyo 181).

Working Group 6:

The Use of Script/Frame in Linguistic Semantics

Organizer: Victor Raskin

Purdue University, U.S.A.

1240

— Aspects of Script Semantics (summary) —

The meeting of the Working Group took place on Thursday, September 2, 1982, at 10:30 a.m.—3:30 a.m. The meeting consisted of two parts. In the first part, four formal 30-minute-long papers were presented. At the end of the first part, applications for short 5-10-minute presentations from the floor were invited. The second part began with four such presentations. Then all the speakers were invited to ask questions of each other. The remaining two hours were devoted entirely to questions and comments from the floor and to a free-for-all discussion on the subject. Over 100 people were estimated to attend the meeting. 16 people from 14 countries played an active role in the Working Group.

The proceedings of the Working Group, along with a few papers by colleagues who were unable to attend the Congress (including Professors Renate Bartsch of University of Amsterdam, The Netherlands; Charles J. Fillmore of The University of California at Berkeley, U.S.A.; Ferenc Kiefer of the Hungarian Academy of Sciences; Jerry L. Morgan of The University of Illinois at Urbana-Champaign, U.S.A.; Deborah F. Tannen of Georgetown University, U.S.A.; Yorick Wilks of The University of Essex, England; and Vladimir A. Zvegincev of Moscow State University, U.S.S.R.), were solicited for publication by *Quaderni di Semantica*, and will appear in full as the first part of the journal's Round Table Discussion on Script Semantics. The second part will follow a few months later, in which the same participants will respond to each other's positions expressed in the first part.

The Working Group was devoted to one of the most important problems of modern semantics and pragmatics. Recent developments in semantic theory have led to the investigation of a number of concepts such as presupposition, implicature, conversational postulate, speech act, possible world, etc., which capture semantic information not contained in the lexical entries. It is becoming increasingly clear that extralexical information of this kind is essential for the production and comprehension of numerous ordinary sentences of natural language. The concept of script/frame/schema has been introduced to denote a structured chunk of semantic information which is larger than an individual lexical entry and which is internalized by a competent native

speaker. It is clear that time has come for linguistics to explore the **role** and status of the concept in linguistic theory.

In the first formal presentation, Professor József Andor of Medical University of Pécs, Hungary, spoke on "The Psychological Relevance of Frames." After a brief survey of various definitions of 'frame,' 'script,' 'scene,' and 'schema,' the speaker focused his attention on the relations between frames, which he perceived as linguistic entities, and the other categories, which he treated as empirical and pragmatic. The central part of the paper was a report on a few lexical association tests designed to determine the most salient features of certain frames. Thus, for instance, the frame of 'mailman' was shown to include the concept of 'bag.' If a word appeared on many of the 160 subjects' association lists it was, of course, perceived as an important semantic element of the involved frame.

Professor Roger C. Schank and Mr. Lawrence Birnbaum's (both of Yale University, U.S.A.) paper on "Thematic Memory Structures in Language Processing" was read next by Mr. Birnbaum. The paper introduced a more abstract level of script representation needed when people are reminded of previous stories or experiences which do not share any superficial features with the current input, but do have a broad, thematic similarity with it. For instance, X complains to Y that he is unable to get his wife to cook a steak as rare as he likes; Y is immediately reminded of an incident years before, in which he was unable to get a barber to cut his hair as short as he wanted it. The level of analysis needed to explain examples of this sort concerns the *relationships* among the plans and goals present in the stories. Thematic organization points, or TOP's, were introduced for this level of analysis.

Professor Thomas R. Hofmann of Toyama University, Japan, read his paper entitled "Semantic Frames and Content Representation", in which he argued that a semantic or content representation (CR) of the discourse must be available, in addition to semantic frames or scripts and the general background knowledge, for an adequate account of the comprehension—or an adequate analysis—of a discourse or a text. The paper examined the interactions between the CR and the scripts or frames found in general knowledge. It maintained that a frame is "called up" or "requested" from the store of general knowledge when a referring expression has no referent in the CR, or apparently also, in the physical context of the discourse. Treating a frame as a formal object of the same nature as a CR, the speaker proposed a model in which the particular script or frame chosen from among the multitudes available is the one which (1) includes a possible referent for the unattached referring expression which forced its call-up and (2) maximally overlaps with the CR deriving from recent sentences.

Finally, I delivered my paper on "Script-Based Semantics," in which a semantic theory based on the notion of script was proposed. The theory consists of two components, the lexicon and the combinatorial rules. Formally, the lexicon is a single, continuous, multi-dimensional graph with mostly lexical

cal nodes and mostly semantical links. Each lexical entry of the language is represented in the graph either by a node or by a number of linked nodes. The domain containing the lexical entry is the script which this entry evokes. In the sentence, every constituent evokes one or more scripts from the lexicon, and it is the function of the formal combinatorial rules to calculate all the compatible combinations of the evoked scripts as well as the meaning of the sentence determined by these compatible combinations of scripts. Both the scripts and the combinatorial rules are established and justified on the basis of formal linguistic procedures. The paper dealt more specifically with the boundary between linguistic and encyclopedic information.

In the second part of the meeting, four 10-minute papers were delivered from the floor. Professor Robert E. Longacre of The University of Texas at Arlington, U.S.A., spoke on scripts in explaining some phenomena in lower-level structures. By using such ordinary English texts as "I went downtown to get a hamburger. After eating it, I paid..." and comparing them to such ordinary Filipino texts as "She is beautiful but has no mother," he claimed that the well-formedness and appropriateness of sentences depends on the availability of certain scripts to the user. Professor Richard Hudson of University College, London, voiced his enthusiastic support for script-based semantics and suggested that his newly developed "word grammar," relating words and concepts in a systematic way, has a great deal in common with this theory. Professor Ryszard Zuber of CNRS, France, attempted to relate scripts to opacity and transparency in language and to factiveness and non-factiveness. Finally, Professor Jeffrey S. Gruber proposed a view of semantics based on a complicated interaction of syntactic, semantic, conceptual, and encyclopedic information, with scripts including the three latter types of semantic knowledge. He argued forcefully for the systematic study of conceptual information within semantic theory.

Besides all the speakers, a few more colleagues contributed actively to the discussion. These included: Professor Roger Van de Velde of The University of Antwerp, Belgium; Professor Jacob Mey of Rasmus Rask Institute of Linguistics, Denmark; Professor Albrecht Neubert of Karl Marx University, German Democratic Republic; Professor Burghard Rieger of The Aachen Technical University, Federal Republic of Germany; Professor In-Seok Yang of Hankuk University of Foreign Studies, Republic of Korea; Professor Taka-ichi Okuda of The University of Osaka, Japan; Professor Irena Bellert of McGill University, Canada; and Professor Robert Litteral of Papua, New Guinea.

The central issues of the questions, comments and discussions were identified as follows:

1. The necessity of semantic representation as a separate level of script-based semantics.
2. The objectivity and inter-speaker validity of scripts.
3. The relations between script-based semantics and pragmatics.
4. The use of scripts for the interpretation of every sentence rather than

only of sentences with missing referents.

5. Scripts and translation.

6. The social validity of scripts.

In the process of discussion, appeals were made for more interdisciplinary and international cooperation in the area of script semantics.

As Coordinator of the Working Group, I would like to express my cordial gratitude to all its participants and to the Congress Office. The concerted efforts of both of these groups resulted in a highly successful professional meeting at which a fruitful exchange of ideas took place.

Working Group 7:

Fachsprachen und Kommunikationskonflikte in der modernen Gesellschaft

Theo Bungarten

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(24)

Als Folge der modernen industriellen Entwicklung in den hochentwickelten Ländern, der gesellschaftlichen Arbeitsteilung und fachlichen Spezialisierung haben sich neben der Standardsprache die Fachsprachen der Technik und Wirtschaft, der Dienstleistungsbereiche und der Verwaltung, der Kultur und Wissenschaften immer stärker entwickelt und sich von der Standardsprache abgesondert. Wenn diese Spezialsprachen insgesamt gesehen auch zur optimalen Kommunikation, zur Erfüllung kognitiver und sozialer Funktionen der Experten in dem jeweiligen Fachbereich dienen, so sind mit diesen Fachsprachen und dem fachlichen Sprachgebrauch auch eine Reihe von Konfliktmöglichkeiten gegeben. Zwar sind die Fachsprachen einerseits ein Ergebnis von faktisch bereits durch die gesellschaftliche Entwicklung vorhandenen Bedürfnisabspaltungen und Interessensverschiebungen sich konstituierender Gruppen, die Fachsprachen ihrerseits bergen jedoch als Spezialsprachen, die der Alltagssprecher nicht mehr kennt, ein eigenes Konfliktpotential in sich, das in der Wirklichkeit heutiger gesellschaftlicher und individueller Kommunikationsformen tatsächlich zu Kommunikationskonflikten (= KK) führt. Die Konflikte in Kommunikationssituationen, die durch fachlichen Sprachgebrauch bedingt sind, werden noch dadurch schwerwiegender und komplexer, daß die spezialisierten Bereiche wegen ihrer besonderen Fertigkeiten und Kenntnisse, ihrer Einfluß- und Veränderungsmöglichkeiten meist ein höheres Prestige gegenüber Bereichen der grundlegenden menschlichen Bedürfnisbefriedigung gewonnen haben. Dieses Prestige geht nicht nur durch die Einschätzung der Kommunikationspartner, die durch die Wahrnehmung des Gesprächspartners und der Bedingungen der Situation hervorgerufen wird, in die Kommunikation ein, sondern auch durch die Sprache und den Sprachgebrauch selbst, da die Fachsprache selbst zum Prestigeträger geworden ist. Damit ergibt sich auch die Möglichkeit des Mißbrauchs, der Ursache für die Manifestation latenter Konflikte sein kann.

Empirisch festzustellen sind KK offensichtlich an unangemessenen Reaktionen eines oder mehrerer Gesprächspartner in der jeweiligen Situation (wobei die im Ausdruck der Unangemessenheit implizit angenommene Norm des Verhaltens schwer zu bestimmen ist und ein Hinweis auf diese nur selten durch kodifizierte Regeln (Gesetze), moralisch und ethisch allgemein anerkannte Werte sowie Vernunftspründe gegeben ist). Abhängig sind sprachlich bedingte

Konflikte natürlich von den Kommunikationspartnern und deren Sprachkompetenz. Hier sind allgemein zu unterscheiden KK zwischen Experten des gleichen Faches, zwischen Experten verschiedener Fächer, und zwischen Experten und Laien. In vielfacher Hinsicht konfliktträchtiger werden Kommunikationssituationen verständlicherweise durch fremdsprachigen Fachsprachegebrauch, wie Ingemar Persson am Beispiel der Rezeption deutscher Wirtschaftstexte durch schwedische Studenten zeigte (Lunder Forschungsprojekt FAK).

Am markantesten sind Konflikte zwischen Experten und Laien, nicht nur, weil beide nicht über die jeweilige Vorstellungswelt des Partners verfügen, sondern weil für die Übermittlung der Nachricht kein gemeinsamer Kode erreicht wird. Dies liegt in der Regel nicht nur am Defizit des Laien, sondern auch am teilweisen Verlust einer kommunikativen Kompetenz für Alltagssituationen beim Fachmann.

Zunächst will ich aber versuchen, den alltagssprachlichen Begriff des Konflikts, bezogen auf sprachlich angezeigte Konflikte, etwas einzugrenzen. Helmar Frank hat in seinem Vortrag und in der Diskussion aus kybernetischer Sicht auf eine Reihe von generellen Störfaktoren in der Kommunikation zwischen dem Fachmann und dem Laien hingewiesen, wie etwa unbeabsichtigte zusätzliche Informationen und Informationsverluste bei der Übermittlung sowie Beschränkung der Gedächtniskapazität. Nicht in Frage stehen hier solche KK, die ausschließlich durch unterschiedliche Auffassungen von Sachverhalten bedingt sein können, noch kanalbedingte, noch solche, die durch den Prozeß der physischen Produktion oder Aufnahme der Nachricht hervorgerufen werden. Auch ausschließlich persönlichkeitsbedingte KK sind für die hier zu behandelnden fachsprachlich bedingten KK irrelevant. Gemeint sind hier vielmehr solche KK, die in der Fachsprache selbst angelegt sind bzw. durch den fachlichen Sprachgebrauch auftreten können. Hier sind auch solche KK zu berücksichtigen, die ihre Ursache in unterschiedlichen facheigenen und alltagssprachlichen Kommunikationsnormen (Sprechaktnormen, Konversationsmaximen, u. a.) haben. Unter KK kann mit diesen Einschränkungen verstanden werden eine Situation, in der einer von zwei oder mehreren Sprechern einen Kommunikationsversuch macht oder machen möchte, ohne auf den Ebenen der Lokution, der Illokution und der Perlokution erfolgreich zu sein. Nach dieser Definition besteht somit schon ein KK, wenn eine Kommunikation gewünscht wird und der folgende Kommunikationsversuch fehlschlägt. Ursache für den Fehlschlag kann auch eine Kommunikationsunfähigkeit (mit den obigen Einschränkungen) sein. Damit werden alle Formen von fachsprachlich bedingten Kommunikations- und Sprachbarrieren für die Problematik relevant und zu Konfliktpotentialen.

Eine erhebliche Barrierenfunktion für den Laien haben die Fachwortschätze. Tatsuo Miyajima konnte als Ergebnis eines Vergleiches im Bereich der wissenschaftlichen-technischen Terminologie nachweisen, daß im Japanischen aufgrund der besonderen geschichtlichen Entwicklung der Wissenschaften

in Japan ein auffallend großer, statistisch meßbarer Abstand zwischen Fachterminologie und Grundwortschatz besteht und damit eine erhebliche Barriere für den Laien bedeutet. Herbert Penzl zeigte für eine frühere Stufe des Deutschen, am Beispiel der grammatischen Terminologie, daß eine beträchtliche Kommunikationsdistanz zwischen Fach- und Gemeinsprache schon in früheren Epochen bestand und sich heute kaum vermindert hat. Besonders seit dem Ende des 19./Anfang des 20. Jhds. ist ein immenses Wachstum in den verschiedenen Fachwortschätzen zu beobachten, während zur gleichen Zeit die nationalen Umgangs- und Standardsprachen in ihrem Wortschatz nur geringfügig zunehmen.

Kommunikationsbarrieren sind aber auch die spezifischen syntaktischen, semantischen Strukturen (etwa die in allen modernen Fachsprachen festzustellende Tendenz der Nominalisierung; die erweiterten Attribute im Deutschen), Argumentationsformen, Sprechakttypen, Textsorten. In Inger Rosengrens Analyse von Kommunikationsstrategien in deutschen Geschäftsbriefen, die KK vorbeugen wollen, wurde offensichtlich, wie stark die Pragmatik des fachlichen Kommunikationsrahmens die Produktion und Rezeption von Fachtexten beeinflußt und steuert.

Auch die für die Gruppe der Fachleute wichtige soziale Funktion des Fachsprachegebrauchs (*Schibboleth*) kann fachsprachlich induzierte Konflikte zur Folge haben. Der Fachsprachegebrauch und seine Prestigefunktion können beim Laien Rollenerwartungen geschaffen haben, deren Einhaltung wie auch deren Verletzung durch den prestigebeladenen Sprecher zu spezifischen KK führen können, von der Kommunikationsunfähigkeit bis zu nicht gelingenden Kommunikationshandlungen. Hier ist etwa die 'funzione mistificatoria' (M. Porro) wissenschaftlich-technischer Sprache in der Produktwerbung, in den Medien und in der Politik zu nennen.

KK größeren Ausmaßes treten durch die Entwicklung moderner Fachsprachen in traditionell agrarisch strukturierten Ländern sowie durch die Übernahme fremder Fachsprachen in den Entwicklungsländern (mangel eigener historisch entwickelter Fachsprachen) auf. Hier wird nicht nur der Informationsaustausch zwischen hochindustrialisierten und unterentwickelten Ländern mangels Kenntnisse der fremden Fachsprachen erschwert, sondern es besteht etwa im spanischsprachigen Bereich die Gefahr der totalen Isolierung vom Alltagssprecher durch diese Übernahme fremder (bes. englischer) Sprachformen in neuzuentwickelnden Fachsprachen. R. Spathaky hat darauf hingewiesen, daß ein sog. internationales wiss. Vokabular besonders leicht in die Sprachen von Entwicklungsländern eindringt (etwa im Türkischen, Indonesischen, Chinesischen). Dies erleichtert die internat. fachliche Kommunikation zwischen den Experten, schafft aber eminente Probleme für einen Wissenstransfer zum Alltagssprecher in diesen Ländern und schafft u. U. neue soziale Schranken und Konflikte innerhalb dieser Länder.

Es kann hier und konnte auch in der Arbeitsgruppe nur um eine ansatzweise Beschreibung heutiger fachsprachlicher KK gehen. Es wird hierbei deutliche

Kommt schon die Theorie der Fachsprachen nicht mit rein innersprachlichen Kriterien aus und muß sie auf die Pragmatik des Faches eingehen, so sind die pragmatischen Faktoren in KK-Situationen noch entscheidender für ihre Beschreibung.

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Fachsprachliche Kommunikationskonflikte aus kybernetischer Sicht

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Kommunikation erfordert eine Anpassung zwischen Sender und Empfänger durch mindestens teilweise Übereinstimmung im Zeichenvorrat. Ausgehend vom Endziel der Mitteilung, nämlich die Bewirkung einer gewünschten Veränderung nicht selbst unmittelbar zu leisten, sondern durch den Empfänger zu veranlassen, erkennt man vier hintereinandergeschaltete, grundsätzliche Störstellen (im Extremfall: „Blindheiten“), durch welche diese Übereinstimmung teilweise (oder ganz) entfällt: die physikalische, die syntaktische, die semantische und die pragmatisch-normative. Die fachsprachliche Kommunikation ist vor allem auf semantischer Ebene gestört (unbekannte Fachtermini, uneinheitliche Terminologie, Sprachgrenzwiderstände nicht nur zwischen verschiedenen ethnischen Sprechergruppen, sondern schon zwischen Fachwelt und Öffentlichkeit). Diese Beeinträchtigung ist weit größer als offiziell vorausgesetzt wird.

Eine zusätzliche Beeinträchtigung der mündlichen Kommunikation ist durch zeitliche Beschränkungen verschiedener Art bedingt. Insbesondere ist die bewußte Informationsaufnahme auf etwa 16 bit/sec an subjektiver Information beschränkt, wodurch ein volles Verstehen selbst dann unmöglich wird, wenn der Zeichenvorrat zwischen Sender und Empfänger auf allen Ebenen übereinstimmt, jedoch wegen unterschiedlichen sprachstatistischen Gewohnheiten die subjektive Information empfängerseitig zu groß wird. Empirisch untersucht wurde dieser Effekt bei der Arzt-Patienten-Kommunikation; dieselbe Ursache steckt hinter der Wirkungslosigkeit insbesondere internationaler Fachkongresse. Bei letzteren ist eine Verbesserung grundsätzlich durch Nutzung von Ergebnissen der Interlinguistik möglich.

Der 'Abstand' zwischen Termini und Grundwortschatz

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Die Diskrepanz zwischen Termini und Grundwortschatz ist im Japanischen grösser als in europäischen Sprachen.

Wir haben eine Stichprobe gemacht und aus terminologischen Standardlisten von 10 Fächern 1000 Wörter, 100 je Fach, zusammen mit entsprechenden englischen Termini ausgewählt. Dann wurde deren Beziehung zum Grundwortschatz (5000 Wörter) beider Sprachen geprüft. Der 'Abstand' dieser wissenschaftlichen und technischen Termini vom Grundwortschatz ist wie folgt (100= vollkommene Verschiedenheit; 0= totale Übereinstimmung):

	Mathematik	Physik	Chemie	Elektrotechnik	Maschinenbau
jap.	61.5	60.0	72.0	59.5	55.5
eng.	32.5	29.5	48.0	22.5	26.5
	Aeronautik	Architektur	Zoologie	Botanik	Odontologie
	57.5	54.0	70.0	72.0	78.0
	17.5	22.0	69.0	59.0	45.5

Aus den 100 physikalischen Termini wurden dann 65 ausgewählt. Der 'Abstand' dieser wissenschaftlichen Termini vom Grundwortschatz, verglichen mit ihren Entsprechungen in 4 europäischen Sprachen, stellt sich wie folgt dar:

japanisch	63.8	englisch	31.5	französisch	36.2
deutsch	40.8	russisch	46.9		

Eine doppelte Rolle haben die chinesischen Zeichen bei dieser Diskrepanz gespielt.

Lexikalische Kommunikationsdistanz zwischen Fachsprache und Gemeinsprache

Herbert Penzl

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Der gesellschaftliche Kommunikationskonflikt durch die Entwicklung von Fachsprachen war in vergangenen Epochen des Deutschen noch stärker, auch als Latein nicht länger allgemeines Bildungsmonopol war. Die deutsche grammatische Terminologie z.B., als Fachsprache schon in der Volksschule wichtig, zeigt seit dem 16. Jhd. wechselnde Grade der Kommunikationsdistanz von der deutschen Gemeinsprache. "Gebildete", die die Fachsprache gelernt haben, werden so immer wieder und jetzt von neuem von "Ungebildeten" ohne Fachsprache sprachlich und soziologisch getrennt. In der Gegenwart ist es nicht das Latein, sondern das Englische, das im Falle grammatische Fachsprache die Kommunikation zwischen Sprechern mit und ohne Fremdsprachenkenntnisse (=Englischkenntnisse) erschwert; das gilt natürlich besonders für die internationale wissenschaftliche Kommunikation.

Kommunikationskonflikte bei der Rezeption fremdsprachiger Wirtschaftstexte

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Hintergrundwissen, Fähigkeit, den Text auf dieses Wissen zu beziehen, und Erkenntnis impliziter Relationen im Text spielen nach neueren Forschungsergebnissen eine wichtige Rolle im Verstehensprozess.

Hauptzweck dieses Referats ist es, auf der Basis experimenteller Untersuchungen in Lund, der Frage nachzugehen, in welchem Ausmasse die Sprachkompetenz und die oben genannten Faktoren auf die Rezeption eines fremdsprachigen Fachtextes, in diesem Fall eines deutschen Wirtschaftstextes, durch schwedische Experten und Laien hemmend wirken. Welche Rolle spielt die Syntax verglichen mit dem Wortschatz?

1. Für ein kommunikatives Textverständnis, verstanden als eine adäquate Reaktion auf einen als eine kommunikative Handlung definierten Text, im aktuellen Fall einen Geschäftsbrief, spielen die einleitend genannten Faktoren in Kombination mit dem Wortschatz eine ausschlaggebende Rolle. Die syntaktische Struktur wirkt auch bei Kontrastivität und Komplexität der Struktur nur in gewissem Ausmasse hemmend auf das Verständnis.

2. Für das Verständnis isolierter Sätze (in gewissen Situationen erforderlich), im aktuellen Fall aus wirtschaftswissenschaftlichen Texten, spielt die Syntax eine grössere Rolle. Es gibt keinen Kontext als Stütze für den kognitiven Verstehensprozess.

3. Entscheidend ist also zunächst, was unter Leseverständnis verstanden wird.

4. Texttyp und sprachliches Niveau sind Faktoren, die auch zu beachten sind.

5. Sprachkompetenz und Leseverständnis korrelieren nicht notwendigerweise, indem der Fachmann, hierbei dem Laien gegenüber im Vorteil, durch Hintergrundwissen usw. mangelnde Sprachkompetenz überbrücken kann.

6. Eine interessante Fragestellung ist, über welche Mini-Kompetenz der Fachmann verfügen muss, um sich in seinem Fachbereich zu orientieren.

Kommunikationsstrategien zur Vorbeugung von Kommunikationskonflikten

Inger Rosengren

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Zwei Typen von Kommunikationskonflikten werden unterschieden:

- (a) semantisch bedingte Kommunikationskonflikte
- (b) pragmatisch bedingte Kommunikationskonflikte

Die unter (a) genannten Konflikte sind darauf zurückzuführen, daß der Empfänger **entweder die Illokution selbst oder aber den propositionalen Gehalt** bzw. Teile davon nicht versteht. Die unter (b) genannten Konflikte entstehen, wenn der Empfänger zwar versteht, was der Sender sagt, jedoch nicht versteht, weshalb er es hier und jetzt sagt. Es fehlt ihm sozusagen der Kontext, der die Angemessenheit der Illokution gewährleistet.

Der Beitrag behandelt nur (b). Als theoretischer Ausgangspunkt wird ein vierstufiges Modell gewählt (Rosengren, erscheint 1982), das die Bedingungen nennt, die erfüllt sein müssen, damit eine Äußerung zu einer erfolgreichen Illokution wird. Ein Kommunikationsversuch der ersten Stufe wird zu einer gelungenen Illokution der zweiten Stufe, wenn der Empfänger den Kommunikationsversuch versteht (a) und wenn er ihn als angemessen auffaßt (b). Es wurde an anderer Stelle gezeigt, daß der Sender von vor allem argumentativen Texten (u.a. auch von Briefen) tendiert, seine Illokutionen in bezug auf die genannten Bedingungen strategisch abzustützen. Die auf diese Weise entstehenden subsidiären Illokutionen bilden mit der gestützten (dominierenden) Illokution zusammen eine pragmatisch bedingte Textstruktur. Am Beispiel von Geschäftsbriefen wird gezeigt, welche Strategien der Sender sich bedient, um die Angemessenheit seiner dominierenden Illokution abzusichern und dadurch eventuellen pragmatisch bedingten Kommunikationskonflikten vorzubeugen.

Working Group 9:

Functional Grammar

Organizer: Simon C. Dik
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This working group had the function of demonstrating, by means of a number of detailed examples, the types of questions posed, and the sorts of answers provided, within the theory of Functional Grammar (FG).

The working group seemed to fit in well with the apparently growing interest in functionally, pragmatically, and semantically oriented approaches to grammar. Such growing interest manifested itself in several of the plenary and section meetings of the congress, and although stemming from sometimes quite different sources, it seemed to converge on a clearly delineated, if complex, focus of interest, defined by the study of grammar in relation to its actual functioning as a means of human communication.

Though each of the papers, taken separately, could probably not be grasped and discussed in all its implications, it is hoped that, taken together, they have provided a representative impression of how people go about doing FG. The following papers were presented as part of the working group:

Nominal Predicates in a Functional Grammar of English

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There are good reasons to assume that nouns (nominal predicates) can have more than a single argument position, and can also be associated with a variety of satellite positions. These term positions are characterized by semantic functions partly corresponding (but not identical) to those associated with verbal predicates. A special form of syntactic function assignment is argued to be operative in the nominal domain, and Topic and Focus may also be relevant to the expression of terms formed from nominal predications. The resulting analysis is illustrated with English 'nominalizations' such as *John's gift of a book to Mary*, as contrasted with constructions of the form *John's giving Mary a book*, where the former is based on the nominal predicate *gift*, the latter on the verbal predicate *give*, but both have similar argument frames.

Possessive Constructions in French

Co Vet

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Analysis of four possessive constructions in French: (a) possessive determiner (*mon livre* 'my book'), (b) possessive prepositional phrase (*une amie de/à Jeanne* 'a friend of/to Jane'), (c) prepositional phrase in predicate position (*ce livre est à Jeanne* 'this book is to Jane'), (d) possessive pronoun (*le mien*, lit. 'the mine'). On the basis of a semantic and pragmatic analysis of these construction types it is argued that (a), (b), and (d) derive from the same underlying structure, which is analogous to that for expressions such as *son arrivée* 'his/her arrival'. (c) will have to be described in terms of another structure. Finally it is briefly examined whether the analysis put forward for French can also be applied to French-based creole languages, which often have quite different types of possessive construction.

Two Possessive Constructions in Latin

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Discussion of the semantic, syntactic, and pragmatic properties of Latin constructions of the form (i) *puero liber est* 'to boy book is' = 'the boy has a book', and (ii) *pueri liber est* 'of boy book is' = 'the book is the boy's'. It will be argued that these two constructions differ fundamentally in many respects, and that there are reasons to describe them in terms of distinct underlying predications, such that (i) has an underlying basically existential predication extended by a dative satellite, whereas (ii) has the genitive term as a possessive predicate applied to the term indicating the possessed entity.

Between Object and Oblique: In Defense of Secondary Object

Kenji Kanno

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This paper argues that the grammar of English requires, in addition to the two syntactic functions of Subject and Object so far distinguished in FG, a third syntactic function, that of Secondary Object, if one is to be able to account for such constructions as: *John* (Subj) *gave Mary* (Obj) *the book* (Sec Obj), and *Mary* (Subj) *was given the book* (Sec Obj) *by John*. It is shown

that the assignment of Secondary Object can be used to explain a number of regularities in English syntax. It is offered as an addition to FG, which is not incompatible with the framework of FG as so far developed.

Pragmatic Constraints on Subject and Agent Selection

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This paper reports on experimental research aimed at determining the extent to which the organization of a text is determined by the perspective from which it is formulated. Subjects were presented with two stories, each involving two participants which had been given about equal prominence in the text. They were then asked to re-tell the stories from the point of view of one of the participants. It was found that subjects, in doing so, significantly more often place the participant-in-perspective in Agent or Experiencer roles, and significantly more often assign Subject function to these roles, than is the case with the participant-out-of-perspective. Some implications of these findings are (i) that the impact of pragmatic factors on text production and constitution can thus be demonstrated by means of precise empirical methods, (ii) that a model of Functional Grammar should pay more serious attention to the contextual embedding (including speaker's intentions) than has so far been the case.

On the Functions of *wa* and *ga* in Japanese

Jan de Jong¹⁾ and Yuri Okabe²⁾

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The uses of the particles *wa* and *ga* in Japanese can to a large extent be described in terms of syntactic and pragmatic functions. *Ga* is most often used for Subject-Focus constituents, *wa* for marking Topic or Theme constituents. Contrastive *wa*, which provides special difficulties for such an approach, is argued to be rather closely related to Topic *wa*. The occurrence of *wa*-marked Subjects and the omission of Subject terms are compared with respect to their discourse functions, and are shown to both contribute to establishing and maintaining topics and sub-topics in discourse.

On Subordination in Usan and Other Papuan Languages

Ger P. Reesink

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Subordinate clauses in Usan and other Papuan languages are shown to have special pragmatic significance. Initial subordinate clauses share with Topics the function of presenting presupposed materials. Non-initial subordinate clauses can be explained by a process of syntactization of what one could call 'afterthoughts'. Such non-initial clauses are asserted rather than presupposed. The relevant sentence configurations can be described in terms of such pragmatic notions as the Prague School concept of 'communicative dynamism', and in terms of the psychological notions of Figure and Ground.

Two Constraints on Relators and What They Can Do for Us

Simon C. Dik

Institute for General Linguistics, University of Amsterdam

Relators are defined as adpositions, case markers, or subordinating elements. Then, two constraints are formulated which together are taken to define the preferred positions for relators: (i) the Peripheral Position Constraint, according to which relators prefer a position at or near the boundary of the constituent to which they immediately belong, and (ii) the Medial Position Constraint, according to which relators prefer a position in between the center of the constituent to which they belong, and the constituent to which they relate that center. It is then shown that these two constraints explain quite a few seemingly unrelated phenomena in the typology of constituent ordering.

The papers read in this working group, together with a number of further contributions by people who could not attend the congress, have been published in: Simon C. Dik (ed.), *Advances in Functional Grammar*, Dordrecht: Foris 1983.

Working Group 10:

Sociolinguistic Surveys in Asia

Organizer: R. R. Mehrotra

Banaras Hindu University

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The Working Group met on 2 September 1982 in two sessions chaired by Takesi Sibata and Bh. Krishnamurti respectively. At the outset R. R. Mehrotra, the organizer of the Group stressed the sociolinguistic work in multilingual and multiethnic Asian countries and asserted that the need for sociolinguistic surveys in Asia was never so pressing as today. A resume of the papers presented is as follows:

A Survey of Telugu Dialect Vocabulary Used in Native Occupations

Bh. Krishnamurti

The State Academy of Letters set up by the Government of Andhra Pradesh (India) in 1957 undertook the preparation of dialect dictionaries of the vocabulary used in native occupations. Krishnamurti has been the Chief Editor of the series. So far four volumes have been published: Vol. I Agriculture, Vol. II Handloom, Vol. IV House Construction, Vol. V Pottery. Vols. III and VI will be ready soon.

The objectives of the project are: (1) to elicit and record words which have not so far been recorded in earlier dictionaries; (2) to establish the major regional and social dialects of Telugu and (3) to get insights into the formation of terms for concepts, old and new, in folk speech.

The methodology evolved in the preparation of these volumes sharply differs from the dialect survey work done in the English speaking countries, i.e., U.S.A., England and Scotland. This combines the lexicographic approach with dialect survey. An English *Introduction* describing the methodology can be found in Vol. I (Publisher: A.P. Sahitya Akademi, Kalabhavan, Saifabad, Hyderabad, India).

The four published volumes have yielded nearly 30,000 new terms not found in other dictionaries. A linguistic atlas was prepared on the basis of the distribution of lexical and phonological items recorded for the whole State. It is now established that there are found regional dialects in Telugu—North, South, East and Central. The central region has no exclusive isoglosses. The

isoglosses of the other areas sometimes overlap into this. It is interesting to note that this 'open dialect area' has become the source of modern Standard Telugu. Dialect differences between educated and uneducated classes have been stated, mostly in phonology. Educated speakers contrast aspirated and unaspirated stops whereas uneducated speakers have only unaspirated stops. Six such diagnostic differences have been noticed throughout the State. These have constituted the basis for further research in Telugu social dialects.

Sociolinguistic Surveys in South Asia: An Overview

R. R. Mehrotra

The primary aim of sociolinguistic surveys in South Asia has been the investigation and recording of linguistic variation in relation to socio-cultural factors. The major areas covered by surveys are: dialects and restricted languages, diglossia and bilingualism, pidginization and convergence, forms of address and reference, language planning and standardization.

Whereas the emphasis in Pakistan and Bangla Desh has been on the compilation of dialect dictionaries, the so-called 'caste dialects' received the focus of attention in India. The distinction between the literary and colloquial varieties in diglossic situations has been prominently discernible in Bengali, Tamil and Sinhalese. A recent trend has been towards the merger of the two varieties. Linguistic convergence has been examined in South Asia with special reference to Saurashtri, Konkani, Marathi and Hindi. The main trends in regard to the pidgin and creole study in South Asia have been concerned with (a) the creole origin of a modern language i.e. Marathi and (b) the case of an existing pidgin undergoing a process of creolization as is exemplified by Naga pidgin in North-East India. Forms of address in respect of Bengali, Hindi, Marathi and Telugu have been surveyed and analysed from a number of perspectives. Language planning surveys in South Asia have been concerned mainly with the problems of language standardization, language conflict and language maintenance.

Most sociolinguistic research in South Asia has been inspired by the theories and models developed in England and America and many of these are found to be inadequate and inapplicable. What is needed is a balanced and judicious approach in the matter.

Language Use in Education: A Sociolinguistic Perspective of Multilingualism

D. P. Pattanayak

In a multilingual setting as of Asia a policy requiring use of dominant languages alone in education is bound to keep the majority of people speaking diverse languages outside the purview of education. Most countries in Asia have a colonial past and consequently a dominant colonial language to contact with. Accent on such a language inhibits growth of indigeneous languages and results in the creation of a limited elite. This subsequently leads to various language movements. Multilingual education is the most important corrective to the imbalance in educational system.

Sociolinguistic Surveys in Japan—Approaches and Problems

Takesi Sibata

Japanese sociolinguistics under the label 'gengo seikatsu' (the life of language) began in 1951, nearly eight years before the beginning of sociolinguistics in the U.S.A.

The first large-scale sociolinguistic surveys in Japan were undertaken in 1949 in Shirakawa and Tsuruoka by Takesi Sibata and Nakamura respectively under the auspices of the National Language Research Institute. The surveys revealed that the place of birth was the most important factor in influencing the standardization of dialects. Surveys conducted in 1951-54 in regard to politeness revealed that in Japan females tend to use more polite expression and more frequently than males; longer discourse is considered more polite than shorter discourse; using more Chinese loanwords tends to raise the level of politeness. "Sociolinguistic Survey in Tokyo and Osaka 1974-75" established that the increased urbanization leads to the diversification of linguistic usage.

As early as 1949 the so-called "24 hours" surveys were conducted by Sibata. Some of the findings of the surveys are: the peak of frequency of linguistic behaviour in every individual appears at the time of breakfast, lunch and dinner. The publication of the *Linguistic Atlas of Japan* (1955 to 1975) was an important landmark in sociolinguistic surveys in the country.

The two sociolinguistic problems which call for detailed survey in Japan are (a) orthography reform and (b) linguistic discrimination. Japan being a monolingual and monoethnic country is not confronted with the problems relating to official language, minority language and bilingualism.

The meeting came to a close with a vote of thanks by the organizer who, in his concluding remarks, observed that the utmost need of the hour is the pooling of resources and experiences and launching of surveys with international cooperation and collaboration.

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Working Group 11:

Synchronic Processes in Language Contact Situations World Wide: A Focus on Generalizations and Individualizations and Universals

Organizer: Jacob Ornstein-Galicia
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The composition of this Working Group, as it now stands (as of 22 March 1982) offers discussions by a half-dozen scholars who address themselves to language-contact situations in West and East Africa, Uruguay and Brazil, Japanese-English and Spanish-English groups in Philadelphia, U.S., a large Eastern metropolis, the Southwest U.S., America's largest and most intense region of multilingual contact, as well as observations on Yiddish, which has shared co-territoriality with an unusually large number of speech communities in the *diaspora*, and finally a pioneering and programmatic statement is made by a scholar concerning herself with cross-language and cross-cultural kinesics, a branch of the important and growing field of non-verbal behavior.

While this Working Group cannot resolve and investigate all problems in this field, it can help to provide yet another "successive approximation" toward serious investigations of the consequences and implications of languages in contact, wherever this occurs on the globe.

The group intends to analyze linguistically and socio-linguistically maximally diverse and similar contact situations in different countries and areas of the globe. This will constitute a step toward syntheses of processes and principles reflected at the present time in these contact situations, and toward identification of generalizations and "universals" of language/dialect proximity and contiguity. All this will be in the general spirit of Uriel Weinreich's seminal *LANGUAGES IN CONTACT*, appearing in the 1950's, more than a quarter century ago. While there is no dearth of case studies of language-pairs and language-sets (more than two languages, terminology introduced by me), these tend to remain on the purely descriptive level, leaving a rather serious lacuna in the realm of generalization and the derivation of "universals", to the extent that this is possible, from *such case studies*.

Spanish/Portuguese Contact in Uruguay, South America

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About 200 years of relatively close contact between Spanish and Portuguese has resulted in an emerging "bilingual dialect" of Lusitanian basic stock, with Hispanic interference on different grammatical levels. This dialect evidences a great instability which enables us to speak of *variability* (not *variation*) as one of its specific characteristics. *Variability* of variant forms (e.g. *eu* or *yo* "I", which may occur even in the language of the same speaker) can be measured by their frequency of occurrence: the greater the percentual differences between them, the less variability, and vice versa. Sociolinguistic implications of variability are obvious. Attitudes towards these dialects oscillate among: rejection; negation of their existence and of using them; sense of shame even for understanding a dialog in these "languages", and "marginalization" of people who employ them exclusively and ignore Spanish.

Variability, therefore, is the central concept of our view of contact situations and the main methodological tool for describing language in use within a contact domain. It is moreover, closely related to the following characteristics: *structural instability* and *communicative vitality*. The former can perhaps be considered as a cause of variability, while the second could be viewed as one of its consequences. At any rate, *structural instability* ought to be studied through the already known technique of "languages in contact" (v. gr. Weinreich's) while *communicative vitality* constrains us to employ a psychosocio-linguistic analysis.

The Yiddish Language: A language-Contact Laboratory

Marvin Herzog

Columbia University

Yiddish provided the laboratory for the Uriel Weinreich's earliest theoretical inquiries and the stimulus for some of his most brilliant insights. Yiddish was everywhere a "language in contact", in touch with another language at every location in Eastern and Central Europe", its historical area of distribution. Moreover, extensive immigration had brought Yiddish speakers of diverse dialects in contact with each other and with new idioms in North and South America and The Middle East. No better laboratory could be designed for Weinreich's study of languages in contact, lexicography and semantics, language geography, social dialectology, and language change. No better conditions could be devised for testing his hypotheses concerning the relationship

between language and culture areas and presumed communication conditions. Ordinarily, disturbingly ad hoc, such hypotheses are susceptible of much stronger proof when they are based on the reflection of particular communication conditions, on the geographic fragmentation of the cultures and the languages of two societies occupying the same territory. Hence the significance of Yiddish, the most striking case of large scale coterritoriality: with Dutch, German, Hungarian, Polish, Lithuanian, Belorussian and Ukrainian. The present paper examines the regionalization of Yiddish language and folk culture against the geographic makeup of the languages and cultures of several coterritorial non-Jewish societies shedding light on the interplay of areal communication conditions with other causes of linguistic and cultural variation.

American Indian Language Revival: Critical Mass Vs. Language Antiquarianism

Bates L. Hoffer

Trinity University, San Antonio, Texas, USA

The American Indian groups in long contact with white culture in the U.S. show a variety of patterns of language/culture retention. The Alabama-Coushatta Indians of Texas are an example of a group with two centuries of contact which has resulted in English language dominance. Even with many decades of state support and interest in preservation of their culture, that subset which is left is largely tourist oriented. The recent attempts at language/culture revival are presented in this paper as well as the hypothesis that speakers of the original language, as an entity, will continue to be English dominant, since their number is low, and their average age advanced. A negative result is thus anticipated for language revival, but this will be compensated for in all probability by the building of group pride and the reestablishment of a stronger group/ethnic identity.

A similar experiment in language/culture revival at El Paso among the Tiwas (Tiguas), originally from northern New Mexico, and a Pueblo group, is also discussed. More case studies of this sort are needed to arrive at universals/generalizations on "endangered" speech communities.

On Intimate Code-switching in Two Japanese Immigrant Communities in North America

Miwa Nishimura

University of Pennsylvania

According to Weinreich (1953), 'the ideal bilingual switches from one lan-

guage to the other according to appropriate changes in the speech situation (interlocutors, topics, etc.), but not in an unchanged speech situation, and certainly not within a single sentence.' (p. 73). Recent studies (Gumperz and Hernández-Chavez 1972; Gumperz 1976; Poplack 1979) show, however, that in Puerto Rican or Mexican communities in the US, those who code-switch most intimately and heavily in a single situation are the bilinguals who are most competent in the two languages. These studies conclude that in these communities, intimate code-switching is the appropriate way of speaking in certain situations, functioning as an ethnic identity marker.

This paper describes code-switching behavior in two Japanese immigrant communities in North America, Toronto and San Francisco. Unlike the Puerto Rican communities, intimate code-switching is found only among old Niseis (2nd generation) and some Kibeis (those Niseis educated in Japan) excluding the third generation. This is due to the fact that in these Japanese communities, language shift has been completed in the third generation. As in the Puerto Rican communities, intimate code-switching functions as an ethnic identity marker when employed, and only those who are competent in both languages code-switch intimately. These points will be discussed with actual data from my field work, linguistic and ethnographic. Finally, there is need to derive reasonable generalizations about code-switching behavior of different categories of bilinguals, going beyond mere case studies.

A Socio-Cultural View of Language Contact

Joyce Penfield

University of Texas at El Paso

Although Weinreich (1963) explained many linguistic products in terms of language contact framework, even he recognized the important role of the socio-cultural context in the completion of studies on diffusion, persistence and evanescence. In this paper we argue that the socio-cultural context is primary to explain a language use phenomenon existing in all language contact situations, i.e. why certain dialects or languages, or portions of them, are systematically used in specific socio-cultural contexts even though they are referentially equivalent. Our assumption is that in bi/multi-lingual contact situations the choice of one form versus another form carries social meaning which must be defined in reference to the socio-cultural context of the contact setting.

Drawing on various divergent settings from Nigeria to the U.S./Mexican border, this paper will suggest generalizations or "ethno-universals" which relate key socio-cultural factors to language usage and language choice in contact situations. Some of the key factors dealt with include: (1) symbolic values of language systems and/or linguistic forms belonging to each system; (2) degree of standardization of each language system involved; (3) attitudes towards each

language system as well as their speakers; and (4) cultural functions of each language system.

Aspects of Language Contact in Africa

Edgar C. Polomé

University of Texas at Austin

Linguistic communication in the contacts between the local population and expatriates in the former colonial territories in Africa entailed characteristic developments such as the use of a *Mischsprache* as 'kitchen language' and the introduction of African terms into the European language spoken locally. On the other hand, rapid urbanization bringing together considerable numbers of people of very different ethnic and linguistic background implied typical processes of pidginization and creolization. In the Shaba region of Zaire the growth of the copper industry led to the use of a pidginized form of Swahili as language of interethnic communication, and with the stabilization of the urban population in cities like Lubumbashi this pidgin became the first language of a large part of the younger generation and typical features of creolization (relexification, new grammatical patterns, etc.) set in.

Comparative study shows the parallelism of these developments with the phenomena documented for other parts of Africa, e.g., the industrial areas of Zambia and southern Africa.

Cross Cultural Investigation of Kinesics: New Directions

Walburga Von Raffler Engel

Vanderbilt University, Nashville Tenn.

The investigation of non-verbal behavior/kinesics across speech communities begs for serious research attention, and is to all intents and purposes an unexploited, unexplored area of human communication. In line with the concepts to be set forth in a programmatic paper, summer 1982, at the 2nd International Symposium on Contact and Conflict, in Brussels (sponsored by the Research Center on Multilingualism), the following will be submitted at the Working Group in question:

- 1) Such research begs for serious empirical efforts, rather than mere impressionistic treatment;
- 2) It is necessary to pay attention to the investigation of early childhood kinesic acquisition, from a comparative viewpoint;
- 3) Empirical research needs to be performed upon the comparative mani-

festation of kinesic behavior in a variety of maximally different and similar contexts;

4) Interrelations between verbal and non-verbal behavior need to be investigated.

This brief presentation will be presented against the backdrop of this scholar's own pioneering researches into non-verbal behavior.

Working Groups 12 & 13:

Language Contact in Europe

Organizer: P. Sture Ureland

Univ. of Mannheim, West Germany

1. The present situation of the ethnic groups in Europe and the approximately 64 languages spoken there is a result of a whole series of historical events and linguistic fusions.* We can observe continuous ethnogenesis and linguistic fusion in historical documents and sources throughout history since the decline of the Roman Empire and the era of Germanic and Slavic migrations between the 4th and 11th centuries A.D.. Both the genesis and death of a great number of languages must be taken into consideration in an ethnic-historical perspective on the European languages, which focuses on change and fusion.

2. The starting point of our research on language contact in Europe is the conviction that one cannot arrive at significant results without including both the synchronic and diachronic dimensions. An additional dimension is also required—the ethnic setting. All three dimensions are necessary in describing man's bi- or multilingual capacity. The necessity of an ethnically-based type of linguistics has made it clear that the bilingual individual constitutes an important focus of research in describing linguistic variation and change. Linguists of this orientation do not focus their attention merely on abstract systems or rule schemata in systemic linguistic attempts, but rather on the bilingual speaker in his ethnic setting, who lives with a multitude of social, historical, geographical and ecological variables.

3. In the hey-day of Neo-grammarianism, structural and generative grammar the fact was forgotten or ignored that man is a multilingual creature, who can learn and master several languages with varying degrees of perfection. Bilingualism and diglossia are potential capacities which every human being possesses at birth. He can develop these abilities to a near perfect degree, if the ethnic and social circumstances force him to do so. One can ask oneself whether there is any individual who has recourse only to one system of rules in the sense of the generative grammarians. Every human being lives with a multitude of linguistic varieties around him, which he learns to understand and often to speak within a comparatively short period of time. If he were

* The papers of the work group will be published under the title *Language Contact in Europe*. Papers given at the 13th Congress of Linguists in Tokyo, August 29–Sept. 4, 1982 in the Work Group on Language Contact: *Stratalinguistic and Synchronic Aspects*, ed. by Peter H. Nelde and P. S. Ureland, Ann Arbor: Karoma Press, 1983.

unable to learn or understand these varieties he would also be unable to meet the complex requirements of speech acts.

4. Furthermore, in European areas of multilingualism or in areas with a strongly developed type of diglossia of dialects and a standard language (e.g. Schleswig, Grisons in Switzerland, Alsatia or East Belgium) the inadequacy of systemic linguistics is still more obvious. Here the intricate network of domains, levels of style, and registers has to be investigated before general claims as to linguistic development and change can be formulated. The multidimensionality of linguistic speech acts in these areas contrasts sharply with the one-dimensional rule systems as elaborated within the frame-work of generative grammar. In order to describe linguistic change the multidimensional language gift of man must be taken into consideration. Convergence linguistics or—as we prefer to call it in Europe—contact linguistics is one road to a better understanding of language change and consequently of the nature of language.

5. It is very fruitful to look upon Europe and the Near East as a linguistic and cultural unit which can be described as having coexisted in an process of interesting interaction throughout centuries and millennia. One could also say, from the perspective of our Japanese hosts, that we have been cut off from the rest of the world in a kind of an enormous *cul-de-sac*, a cultural and linguistic dead-end, where the success of attempts to break into other cultures and languages was extremely limited between 360 and 1500 A.D., a period which is marked by the attacks of the Huns in its initial stage and by the fall of the Byzantine Empire (1453) and the discoveries of the searoutes to India (1488) and America (1492) in its final stage. This period of approximately 1200 years is extremely important for the genesis and death of European languages, and the repercussions of this are still visible in Europe. Following the historian McEvedy 1961 we can say that the European-Near Eastern horizon was limited in the north by the Arctic Circle, in the west by the Atlantic barrier, in the south by the Sahara, and in the east by the Ural mountains and the Suleiman Range. Before the 15th century it was not possible to break out of the area within these geographical boundaries because the means of travel had not been developed which were to make it possible to breach the Atlantic barrier permanently, to cross the Sahara desert or circumnavigate it, or to cross the Ural-Suleiman line. We disregard the sporadic Viking contacts with Greenland and North America in the north-west, the Arab crossings of the Sahara in the south (13th century), the Arab conquest of the north-western province of India (Sind) (8th century) in the south-west. Except for these small-scale attempts to break out of the area, the only open land-route to India and China and south-east Asia went via Turkestan, that is via the Silk and Spice Routes. Although the Persian and Macedonian Empires included an Indian province, it is a significant fact that the land-route to India was not attempted by a western army during the entire Middle Ages. The nomads hold on the key to the Far East, Turkestan, remained. The nomad empires, those of the Huns, the Mongols,

and the Turks, were strong enough to block direct cultural and linguistic contacts between Europe and the Far East for centuries.

6. It is known from the linguistic development of the European languages that the invasions from the steppe were decisive for the ethnogenesis of Europe. Aside from historical sources we can reconstruct indirectly the effects of an eastern ethnic invasion of great magnitude sweeping in from the steppe—the Indo-Europeanization of Europe by the Kurgan People between 4000–1500 B.C. for Asia Minor and 2000–1000 B.C. for southern, central, and northern Europe. Thanks to the achievements of historical-comparative philology and modern archaeology, the eastern origin of the European languages (Old Greek, Latin, Old Irish, Old Bulgarian, Gothic, Runic Scandinavian etc.) has been amply described. It is interesting in this context to note that the peoples of Europe have shared the same fate in historical times as the peoples of both the Indian subcontinent and China in having been continuously invaded by the nomads (Huns, Turks, and Mongols), who lived in an area between the three areas in focus here: Europe and the Near East, India, and China.

7. Although these ethnic processes—migration, conquest, destruction, assimilation, racial mixture, and genetic mutation—have been recorded and described in detail by historians, archaeologists, cultural geographers, geneticists, and historical linguists, the thesis that linguistic contact and fusion is one of the most fundamental causes of linguistic change, particularly in periods of prehistorical bilingualism, has been late in becoming accepted by linguists who consider that language developed by evolution. This state of affairs is strange because the continual mixing of peoples and languages has spread common characteristics throughout the Eurasian area under consideration here. We can observe such common properties in the form of common isoglosses which cut across linguistic borders, so-called phonological "Sprachbünde" in the sense of Trubetzkoy, or common syntactic or morphological constructions, as are found for instance in the Linguistic Area of the Balkans, and last but not least a common pool of semantic structures and adaptations between all European languages by means of loan translations, loan creations etc. from Latin and Greek as the learned superstratum models which give rise to a huge Euro-Atlantic Linguistic Area.

8. No archaeologist, geneticist or cultural anthropologist would deny the impact which racial and cultural mixing exert upon the development of a given culture. Language cannot be separated from race or culture, as the three form a single complex. The ethnic-social processes during periods of migration and conquest lead to bilingualism which is often the locus of linguistic change. Such processes underlie the behavior of bilingual speakers and have been studied in synchronic research in the bilingual areas of Europe. Such synchronic interferences and transferences as have been recorded occurring between two languages in contact are believed to have parallels in the past, so that a better understanding can be reached of how new linguistic systems arise because of the fusion of two linguistic subsystems within one individual. Such

racial and linguistic fusion was promoted by the Roman Empire. At the end of its existence its peoples and languages had become fairly homogeneous, with the exception of certain areas on the periphery such as Britain, the Pyrenees (the Basques), the Balkans etc. . The importance of studying such processes of fusion seems obvious, as they give rise to new languages and ethnic structures, e.g. the Spanish language after the fall of Visigothic Spain (737), French and German after the division of the Frankish Empire (888), English after the Scandinavian and Norman-French conquests (800–1100), the Scandinavian languages after the Viking migrations (800–1100), and the linguistic influence of Medieval Latin and the Middle Low German of the Hanseatic League (1100–1500), the Slavic languages after the consolidation of the Kingdom of the Rus' (Old Russian), the establishment of the Bulgarian, Polish, and Czech states between 900–1300. Everywhere we meet with cultural and linguistic adaptation and fusion, where linguistic contacts are established through bilingual speakers, either as a result of natural direct contacts in everyday situations (natural bilingualism) or of the needs of education (literary bilingualism).

9. Putting emphasis on the contact linguistic aspect does not imply that the view of genetically caused changes in the European languages, which seem to arise spontaneously or accidentally is rejected here. The value of a *drift*-hypothesis in the sense of Sapir 1921 is acknowledged here as an explanation of linguistic change of all European languages in the past for which we have no ethnic or historical evidence. However, it is the lack of contact linguistic information in the past, which forces the historical linguist to treat prehistorical language change as a biological process. This genetic-evolutional perspective of linguistic change is abundantly represented in all handbooks of Romance, Germanic, Slavic, and Finno-Ugric philology—a heritage of the historical-biological view in the German type of historical linguistics of the past century. The contact-linguistic perspective on the other hand draws more on the ethnic-social view, but the important role which it plays in the development of a given language has certainly not been acknowledged, since the processes of contact in bi- and multilingual areas do not allow themselves to be easily formalized in categorial developmental schemata. This statement of mine refers to all dogmatic theory formations on linguistic change, whether they are of a neogrammarian, structural or generative nature. A *tertium quid* arises through the processes of contact, so that in the interferences or transferences the seed of a new linguistic form or of a new linguistic variety can be detected. It is only a question of social, political or cultural acceptance, whether this or that interference or transference structure in the speech of bilinguals will be accepted as an innovation by the native speakers or rejected as an alien element in the linguistic system.

The Tokyo work-group on language contact in Europe presented data on linguistic contacts and cultural fusion in Scandinavian (H. Jahr and S. Ureland), Finno-Ugric (I. Bátori), East Slavic (B. Panzer), and Romance—in particular Alpino-Romance—(H. Goebel). H. Munske's paper on the Latin super-

stratum in German was presented by S. Ureland.

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Scandinavian Language Contacts

P. S. Ureland

Univ. of Mannheim, West Germany

My paper is a contribution to a fairly new branch of linguistics: Contact Linguistics. It focuses on the Scandinavian languages because they are an excellent example of languages in contact. Although Scandinavia is geographically on the fringe of Europe, innovations from the west, south, and east have entered these languages at various periods in different ways throughout their history. The essence of every language is the way it varies from a geographical point of view in its development through time and in its social use. This variation is the result of a whole series of factors one of which, language contact between bilingual individuals, is the major cause of language change and variation. This is especially true in areas where we can register social upheavals due to *conquest* (the British Isles and Normandy), *settlement* (Iceland, North Scandinavia, Finland, the Baltic States, and Russia), and/or *trade* (the Varangian settlements in the Ladoga, the Novgorod, and the Kiev areas during the Viking Age or the Low German colonies in the cities of the Hanseatic league (Stockholm, Wisby, Kalmar, Bergen, Novgorod etc.). However, bilingual contacts in Europe also take place indirectly through *learning*, *religion*, *the law*, and *scholarship*. The use of Medieval Latin as the language of the church, the administration, the legal system and research led to language contacts between Latin and the Scandinavian languages which were decisive for their development into effective vehicles of communication. The role of German (especially Middle Low German), French, and English must also be considered here in order to present a complete picture of language change and development in Scandinavia.

Finno-Ugric Languages and their Contacts with Slavic and Germanic —Universals of Language Contact

I. Bátori

EWK Koblenz, West Germany

On the one hand the present paper will focus on the contact between the Finno-Ugric and Slavic, and Finno-Ugric and Germanic languages on the other hand. There are about 24 million speakers of the 16 Finno-Ugric languages today, all in areas of Indo-European linguistic and cultural dominance. Only the Hungarians and the Finns form the local majority in their respective native countries.

In general the socio-economic factors are more important than cultural or linguistic ones for the maintenance of Finno-Ugric languages. This results in the decay of the minor Finno-Ugric languages, especially in the Soviet Union. The greatest impact of Indo-European languages is in the language of science and technology, whereas there is a remarkably strong resistance to this influence in other language varieties (especially everyday speech and fiction).

As regards internal language structure it can be observed that while there are practically no restrictions on lexical borrowing, morphological interference of Indo-European in Finno-Ugric practically does not occur.

Typologies of Language Contact in Early Times and the Present-Day with Reference to the Romance Languages

H. Goebel

Univ. of Regensburg, West Germany

The present paper will discuss some examples from French, Occitan, and Alpine-Romance which demonstrate the value of a combined diachronic and synchronic approach to language contact. This will be done in the framework of contact linguistics which includes the older type of stratum research (sub- and superstratum) as well as modern research on interference.

A typological list which covers various linguistic layers in the Romance-speaking areas just mentioned will show how the knowledge acquired from bilingual and multilingual studies can be used to explain how new languages and new linguistic areas come into existence. The scientific modalities in carrying out this type of research will also be dealt with.

The zones of linguistic contact which are in focus here have been studied intensively for the past few years. My paper is to be seen as a contribution to an overall typology of language contact in the three Romance areas which are in turn a piece of the contact-linguistic map of Europe.

The Role of Latin as a Superstratum in German and other Germanic Languages

H. Munske

Univ. of Erlangen-Nürnberg, West Germany

This paper deals with the main areas of linguistic research (the lexicon and the syntax), where Latin loan-words have influenced the German language historically, comments on characteristics of the Latin superstratum (diglossia, acculturation) and deals in more detail with the change in the German language system, caused by contact with Latin, in the graphemic system, vowels in unstressed syllables, stress rules in foreign words and the structure of morphemes.

I will claim that the requirements of an adequate and complete linguistic description are not met by the one-sided preoccupation with "central systems" in historical linguistics and also in synchronic language description by relegating the characteristics of "foreign words" to so-called "peripheral systems".

I want to show (referring also to Dutch and Scandinavian examples) that—as a result of contact with Latin—patterns for the integration of Romance elements have been created, which have also been applied to more recent French, Italian and English transferences, with the result that the latter frequently appear synchronically to be Latinisms.

Linguistic contacts with Latin led to a kind of convergence of the Germanic languages, which overlay the historical heritage of Germanic language relationships as a kind of European-Atlantic 'Sprachbund'.

External and Internal Factors in Language Change

B. Panzer

Univ. of Heidelberg, West Germany

The subject of the paper will examine whether it is possible to distinguish between loan relationships caused by language contact and internal development which have lead to identical or similar language structures in different places. Or to put it differently: how can one prove what are the results of borrowing from other languages and what are independent parallel developments? It is the question as to the monogenesis or polygenesis of language phenomena: monogenesis, where a phenomenon spreads out from a central point; polygenesis, where it spontaneously develops independently in different places in different languages.

It seems to be time to examine which type of linguistic arguments must be advanced to prove internal development and which to prove the influence of external factors?

The paper is intended as a contribution to the discussion on the methodological principle of research on both language change and language contact. Linguistic material from various European languages and from different levels of the language systems will be discussed inter alia: the 'have'- and 'be'-perfect in Germanic, Romance and Slavic, the 'have'-construction in Russian and Finno-Ugric, the postpositive article in Balkan languages, Scandinavian and North Russian and vowel reduction in Russian and other languages.

Language Contact in Northern Scandinavia (in particular in Northern Norway)

E. H. Jahr

Univ. of Tromsø, Norway

My paper surveys the different linguistic results of language contact in Northern Scandinavia, and more particularly in Northern Norway. For several hundred years the linguistic situation in this area has been one of multilingualism. Here one could hear and observe not just one or two languages being used but many: Norwegian, Lappish, Finnish, Russian, Swedish, Danish, English, German and French. My paper focuses on three of these languages: Norwegian, Lappish and Finnish. The different types of linguistic results which emerge from an investigation of this multilingual situation can be summarized as follows: 1. Various kinds of primitive languages without lasting effect, created on the spur of the moment to overcome the communication barrier. 2. A long-lived Russo-Norwegian pidgin, Russonorsk, which was used for about 150 years in the seasonal trade between Russians and Norwegians. 3. Adstratum elements from Norwegian assimilated into Lappish and the Finnish of Northern Norway, and from Lappish into Finnish. 4. "Lapp Norwegian" with many evident Lappish substratum elements. 5. Language death, affecting especially the Finnish of Northern Norway, with many examples of analogical formations and reductions in morphology. 6. Reductions and generalizations in the morphological system of Norwegian as a result of language shift (i.e. from Finnish and Lappish to Norwegian). These different types of results make Northern Scandinavia a most interesting area for discussing languages from the point of view of contact typology.

Organizer: P. H. Nelde

Research Centre on Multilingualism, Bruxelles, Belgium

Processes and Patterns of Language Shift

Richard Trim

Research Centre on Multilingualism, Bruxelles, Belgium

The reasons for the initiation, rate, and process of language shift are complex and can vary greatly from one region to another. Moreover, language shift can produce a variety of different code patterns according to the sociolinguistic structure of the population.

This paper will examine the ways in which language shift progresses in a language-contact area of Eastern Belgium, and how the progression can affect different diglossic code patterns in the community. Here, different sociolinguistic factors such as age, sex, socio-economic background and the politico-historical development of the region have all contributed to the rate of language shift.

The analysis will therefore attempt to find answers to the question of how and why language shift is progressing and to find out if there are any universal reasons which could be applied to areas of language shift in general.

Ethnic Minorities and Language Contact

Rudolf Kern

Université de Louvain (UCL), Belgium

Although the concept "minority" has already been analysed from many points of view, they have not yet led to a universally recognised definition. As an introduction, a definition of this concept will be proposed which, together with the attempts of interpretation in the scientific literature, takes the contributions of national and international institutions into consideration.

The question of the position and relations of ethnic minorities within national or multinational societies is the further topic of this paper. On the one hand, the topic of the relationship between minority and state-nation(s) or other societies within multinational states, are investigated.

The linguistic and socio-cultural consequences of the contact in question for the smaller ethnic communities of a multinational state such as Belgium will be examined at a deeper level. In this way, special attention will be paid to the symptomatic phenomena as well as the aspect of scientific comparability.

On the Use of Pantomime and on the Teaching of Kinesics to Foreign Workers

Walburga von Raffler-Engel

Vanderbilt University, USA

Several German language sessions for guest workers sponsored by the Goethe Institute in Munich, Germany have been observed. It was most interesting to this writer to notice the use of pantomime as part of the teaching technique.

The paper will focus on two topics: The use of pantomime in the multi-cultural context and the need for the teaching of culture-specific nonverbal behavior to foreign workers and immigrants.

The purpose of this brief presentation is to generate extensive discussion on the subject.

Working Group 14:

A Survey of Non-Linear Phonology

Organizer: M. Y. Liberman

Bell Laboratories, Murray Hill, New Jersey

1251

During the past decade, various researchers have introduced into generative phonology a variety of representational devices that may be called "non-linear," in the sense that they posit entities and relations beyond those inherent in a string of phonemes or feature-matrices. In some cases, these representational devices formalize traditional categories such as "mora," "syllable," and "foot;" in other cases, they formalize traditional devices such as suprasegmental diacritics, or notions such as "prosody" (Firth), "long component" (Harris), or "prosodeme" (Hattori); sometimes, there are no corresponding concepts in earlier work.

The proposed representational innovations are sufficiently diverse, both in form and in substance, that it is difficult to categorize them. The most important categories seem to be (1) the idea of tree-like phonological structures, with relations defined over subconstituents in such trees; (2) the idea of parallel descriptions (often called "tiers"), which are combined by a process of "association," or derived by a process of "projection;" and (3) the idea of a hierarchy of levels of phonological description, where the levels may be akin to proper analyses of a tree, or may be directly expressed as a so-called "matrinal grid."

Each of these ideas has arisen as a natural response to certain descriptive problems. Depending on how they are formalized, some of the categories I have given may be collapsed together, or split apart into more refined primitive ideas. Furthermore, their expressive capacities overlap to some extent—for instance, syllabification has recently been treated as association of an autosegmental tier of syllabic features (Kahn), or as construction of syllabic trees (Kiparsky, Halle-Vergnaud). Attempts to construct an overall theory that would rationalize the set of non-linear phonological devices, and specify the appropriate domain of application for each of them, have not achieved either stability or general acceptance.

Despite some uncertainties, these non-linear methods have made some solid contributions. The typology of phenomena such as stress, syllabification, tone and vowel harmony has been clarified and (to some extent) explained. An illuminating description has been provided for some otherwise puzzling morphological patterns, especially in the Semitic languages. It seems possible to eliminate phonological "action-at-a-distance" in a principled way—that is, to eliminate variables from the structural description of phonological rules. Much

of the work previously done by phonological rules can now be accomplished by local well-formedness conditions on non-linear representations.

Whatever the future of phonological theory may be, it is likely that the substances of these accomplishments will remain intact.

An Approach to Syllabification

Paul Kiparsky

MIT, Cambridge, Massachusetts

In this paper I will assume a hierarchical representation of syllable structure and address the question of how this representation is to be assigned to strings of signals in the phonological derivation. Certain general principles are beginning to emerge from recent work on the variety of languages. I shall formulate and defend a specific set of these principles, which includes the following:

1. Syllabification is universally cyclic in many languages. It *must* be cyclic because the syllabification of complex words depends on the syllabification on their parts, and/or because other cyclic rules (such as stress) must operate on syllabified structures. The prediction is that there is no language in which the optimum grammar *cannot* have cyclic assignment of syllable structure, though its effect may, in some ways, be vacuous or overridden by independently motivated post-cyclic resyllabification processes.
2. Cyclic syllabification is universally structure-preserving. I adopt McCarthy's proposal that phonological rules are blocked when their output cannot be syllabified. Further, I adopt the idea from auto-segmental theory that segments that cannot be incorporated into a syllabification structure remain phonetically unrealized by convention. The output of phonological rules is automatically resyllabified in accordance with a "minimal readjustment principle."

Within the format work of lexical phonology, cyclicity and structure-preservation do not have to be stipulated specifically for syllabification. Rather, they are derivable from more general principles governing the phonological processes of the lexicon. Furthermore, this approach allows syllable structure to function as a filter on lexical representations, making possible the desirable elimination of the problematic category of "morpheme structure conditions" from the theory of grammar.

An Autosegmental Analysis of Japanese and Its Theoretical Implications

Shosuke Haraguchi
The University of Tsukuba

This article is concerned with an autosegmental analysis of Japanese segmental and tonal phenomena.

A disguised language in Japanese, called the Babibu language, will first be analyzed within the framework of autosegmental phonology, and it will be demonstrated that this word play and its analysis are important in that they both make it possible for us to gain a better understanding of the nature of language and offer a number of interesting theoretical implications.

It will then be argued, on the basis of an examination of certain segmental/tonal phenomena dependent on vowel height, that the notion of sonority hierarchy plays a crucial role in several Japanese dialects.

Finally, it will be suggested, with some illustrative examples, that the basic ideas of the autosegmental phonology can be extended to some areas of syntax and semantics.

Working Group 15:

Speech Production

Organizers: Osamu Fujimura¹⁾ and James H. Abbs²⁾

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Abbs, who played the leading role in organization, could not attend the congress, and therefore this group meeting was conducted under Fujimura's leadership. Among the proposed presentations, the abstracts of which were printed in the official book, Abbs and Ohman withdrew their papers by absence. Fujisaki and Liberman volunteered to forego their announced presentations and to contribute substantive comments, thus expanding the time available for coherent discussion. The time available was approximately 5 hours for seven papers, about 30 minutes each. The session was divided into four parts, moderated by Fujimura, Liberman, Fujisaki and Fujimura. There were approximately 50 participants.

Dinnsen discussed the phonetics of phonological neutralization. He gave some background of relevant work in recent years including, in particular, that by Robert Port and his coworkers on German final tenseness. Then he discussed the result of his own speech production experiments on putative neutralization in Catalan phonology. The paper basically pointed to the general lack of quantitatively precise description of phonetic facts, which can undermine theoretical arguments on phonological rules. The separation of phonological rules from phonetic realization was challenged. C. J. Bailey discussed the issue of monitored (as in this study) vs. unmonitored (spontaneous conversational) speech. Ilse Lehisté discussed the importance of listening tests by the native speakers, and also the relevance of vowel color variation and word boundary problems.

Ohala's presentation was retitled as "Evaluation of Phonetic Explanation of Phonology." The presentation replied to previous criticisms by S.R. Anderson about the distinction between phonology-internal vs. external explanations and by Dinnsen and R. Lass that phonetic explanations in phonology do not achieve deductive nomological status. Problems concerning voicing/devoicing and retroflexed apicals were discussed, arguing that the best possible goal for linguistic studies, as for other sciences, was to achieve deductive probabilistic explanations.

Harris discussed the organizational units of speech production. She observed some physiological evidence for breath-group length effects on the early syllables of an utterance, as well as effects of segmental and suprasegmental environment

on articulatory events. She remarked that some rather simple rules may be used to specify relational invariance over context change, as revealed by electromyographic and x-ray microbeam data. Liberman mentioned relevant timing data by R. Port. Eva Gårding raised the question of pre-planning. J. t'Hart, in this connection, discussed the issue of "read" vs. spontaneous speech.

Kiritani discussed a simultaneous microbeam and EMG study of velum movements in Japanese. He showed results of interpreting the observed velum height time function as a second-order linear response to a step function. Liberman noted some conflict of this result with Harris'. A new finding was reported that vowels, when surrounded by nasal consonants, required a short period of velum elevation in spite of the continuously lowered velum. Fujimura mentioned that there was difference between these new data and previous results concerning velum movement from a vowel to a syllable-final nasal, raising a question of deliberate "syllabification" of the final nasal by the subject.

Fujimura discussed his "iceberg" model of articulatory movements. Showing preliminary data that demonstrated relatively invariant local patterns, he observed that the method of comparing two segmentally similar sentences using icebergs would provide us a new tool for studying exact timing patterns with respect to phrasal effects. Fujisaki commented on the possibility of interpreting such patterns as consequences of simple physical mechanisms.

Hirose discussed a differential characterization of motor control dynamics in three types of articulatory dysfunction using microbeam data. It was argued that such quantitative studies of pathological phenomena would reveal basic facts about the neural mechanisms of motor control.

MacNeilage discussed three major impairment types in aphasia, paying particular attention to the likely targets and intrusions in substitution errors. Blumstein's early conclusion about the role of distinctive features and markedness was criticized, and the role played by phonemic segments in substitution errors was emphasized. Liberman asked about the consideration of basic statistics of the language in use, and the metric to be used for phonetic similarity. Ohala commented on the significance of the new view of phonological markedness. Fujimura emphasized the need for a comprehensive model that would predict the error patterns probabilistically.

Liberman discussed what is called pitch declination. He pointed out that there were possibly several different effects that might be alluded to as pitch declination. He suggested that a "soft" preplanning may exist which reflected a gross qualitative idea about the length of utterance, as well as purely local down stepping with reference to discrete phonological units. He also elaborated his earlier comment on the validity of step function inputs to motor control mechanisms, and emphasized the need for appropriate segmental units.

Fujisaki expressed his view on pitch declination, saying that in Japanese data, declination was phenomenologically there. He mentioned, however, that he was hesitant to directly correlate aerodynamics to down drift, since the resetting time was observed to be too short.

Lehiste discussed, about the domain of duration determination, whether the length of the preceding or the following string (of syllables), or that of the total sentence, affected the duration values. Her previous work had demonstrated the latter to be true. A. Abramson commented on Dinnsen's paper, and discussed neutralization with respect to "articulatory persistence."

It was the general feeling that very basic questions of linguistics are being raised as the result of studying quantitative data with various new tools. Specifically, the interrelation between experimental phonetics and phonological theory has entered a new era provoking serious reconsideration of basic theoretical concepts and frameworks.

The Neuromotor Translation of Phonological Features and Units: Implications for the Underlying Representation

James H. Abbs

Speech Motor Control Laboratories, Univ. of Wisconsin, Madison

Historically, theories of speech production have assumed a more-or-less direct nervous system translation from the cognitive-linguistic representation of overall speech system intended goals to peripheral articulatory movements and/or muscle activity. This assumption is reflected in continued descriptions of muscle activity in sound productions in contrasting phonetic environments, observations & renewed discussions of the so-called phenomenon of coarticulation, etc. Increasingly, however, it has become apparent that due to the operation of motor equivalence that *the speech motor translation of speech system intended goals is a multi-level programming process*. That is, recent evidence indicates that (1) a particular speech movement is achieved by variable combinations of synergistic muscles and (2) a particular speech sub-gesture (e.g., labial closure, tongue elevation, etc.) is achieved by variable combinations of individual articulatory movements. Obviously, individual speech muscle actions & movements are programmed quasi-independently from overall speech system intended goals. That is, as noted previously by the present author (Abbs, 1979; Abbs and Cole, in press), it is unlikely that there is more than a general probabilistic relation between hypothesized linguistic features or units and activity in a given muscle and/or movement of a given articulator. The implications of this hierarchical speech motor programming process for articulatory feature descriptions and for the underlying nervous system representation of phonological features and units will be discussed.

Temporal Organization of Speech and Phonological Representation

Osamu Fujimura

Bell Laboratories, Murray Hill, NJ

Recent studies using advanced instrumental techniques have revealed characteristics of articulatory time courses that escape traditional accounts based on concatenated and smoothed (coarticulated) phonetic (segmental) units. A model is proposed which assumes (1) relatively invariant articulatory movement patterns (icebergs) for initial or final part of a syllable (demisyllabic transition), (2) loose timing relations between such events of different articulators, (3) possibility of repulsion as well as smoothing between consecutive gestures by the same articulator, and (4) strong effects of stress and phrase boundaries on variable parts between icebergs.

Representation of Coarticulation in Motor Plans

K.S. Harris

Grad. School, City Univ. of NY, Haskins Labs, CT, Fredericka Bell-Berti.

St. Johns Univ., Haskins Labs, CT and Betty Tuller, New York

University Medical Center, Haskins Labs, CT

A central aim of speech production research has been to understand how the units of communication maintain their identity over such broad changes in overall plan as changes in speaking rate, differences in emphasis, or rearrangement of phonetic elements.

We have taken two approaches to this problem: first, we have examined the spread of the articulatory features of a given phone as the surrounding phones are varied, and have evaluated the results within the context of current theories of coarticulation. Second, we have examined the temporal interrelations of articulatory aspects of phone sequences as stress and speaking rate vary. Results for the first experiments show evidence of the temporal spread of features of a given phone, but not feature capture from phone to phone. Results for the second series of experiments show that although gestures for individual phones vary with changes in suprasegmental context, temporal relationships among articulations remain constant. Taken together, these findings suggest a model of speech articulation in which units are coproduced, but this coproduction is temporally determinate.

On the Phonetics of Phonological Neutralization

D.A. Dinnsen

Indiana Univ., Bloomington, IN, Bell Laboratories, Murray Hill, NJ

A fundamental construct of phonological theory is neutralization. This construct serves as one basis for relating phonology & phonetics. Surprisingly, however, the phonetic details of neutralization have only been assumed. The assumption is that forms which are distinguishable phonetically & phonologically in certain contexts and/or levels of representation are under certain other well-defined circumstances totally indistinguishable at the level of phonetics. Very little experimental work is available that would contribute to establishing the facts of neutralization. Despite the paucity of empirical evidence on this point, a number of important theoretical proposals are founded on this assumption. Recently, a number of studies from diverse research concerns point to the incorrectness of the conventional assumptions about neutralization.

This paper reports the results of an experimental investigation which examined the phonetic details of several putative neutralizations in Catalan. This is done in an effort to establish the facts of, at least, certain neutralizations in a particular language. These results will also be related to other research and general issues of phonological theory with suggested revisions of the theory.

Temporal Control in Dysarthria

Hajime Hirose

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Dysarthria is generally defined as a motor disturbance of articulation secondary to dysfunction of the neuromuscular system related to speech production. Although dysarthria can be classified into several subgroups, the general feature of dysarthria is imperfect articulation in terms of both segmental & prosodic aspects of speech. In order to investigate abnormal articulatory dynamics in dysarthric speech, patterns of the articulatory movements of various types of dysarthric subjects were analyzed by means of pellet tracking techniques using an X-ray microbeam system. The result indicated that there were different patterns of abnormality in both spacial and temporal domains of articulatory movements depending on the type of basic neurological disorders. In particular, the rhythmic structure in the speech flow process was often found to be affected in the case of cerebellar ataxia or Parkinsonism, the pattern of which was characteristic to each pathological condition. It was suggested that an analysis of the dynamic aspects of dysarthria should be a promising approach for elucidating the nature of the motor control of speech.

Modelling Prosodic Effects on Vowel Production

Mark Y. Liberman

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Changes in speech rate and emphasis, both local and global, affect not only an utterance's time pattern, but also the set of articulatory and acoustic states through which it passes. As a result, rate and stress changes are not well modelled by time-axis warping. At least in English, the emphasis effect cannot be reduced to the rate effect, although local changes in emphasis cause local changes in rate. In addition to rate and emphasis effects, a stylistic parameter that one might call "precision" causes systematic variation.

The magnitude of the variation due to these effects is large. Viewed either in articulatory or in acoustic terms, such systematic variation in (for instance) a given vowel (in the same word, in the same sentence, by the same speaker) may easily cover a third of the speaker's entire vowel space. Vowel quality perception, even by phoneticians, appears to compensate for much of this variation.

Production models that permit separate control of rate, emphasis and precision, and that make qualitatively correct predictions about their interaction, will be discussed.

Towards a Non-Segmental, Non-Linear Phonology: Phonetic Considerations

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In contemporary phonology & phonetics, segments & features are usually understood to be, respectively, (sequential) *units* of utterances and (qualitative) acoustic *properties* of these units. Using Aristotelian terminology one could say that this point of view subsumes the segment under the category of Substance & the feature under the category of Quality. And the identification of a segment with the set of distinctive features that it carries therefore presupposes the existence of the segment at the point where the feature is introduced, for, qualities presuppose the substances having them. This notion has stimulated phoneticians & others to attempt to detect, isolate, and describe segments in the continuous acoustic representation of speech. But insofar as the expectation has been to discover 'segment substances', e.g., vectors of acoustic parameter values, results have been negative. We conclude from this that segments *are not* substances and that features *are not* qualities (properties, attributes). Instead we propose to subsume the features under the (non-Aristotelian) category of

(acoustic) fact. i.e., a feature is always an acoustic fact (not an attribute of an object/substance) *to be described by a proposition* of the form 'so-and-so is the case'. One may distinguish simple facts from complex ones and describe the latter by means of the propositional operations of negation, disjunction etc. And the segments (phonemes) may be understood as complex features ('bundles' to use Jakobson's term). This construction therefore reverses the logical relationship (of presupposition) between segments and features. This is, moreover, an essential step toward a *non-segmental phonology*. In a second step the phonological notions of sequentiality ('precedes', 'follows', 'is simultaneous with') are analyzed, and the idea is advanced that these relations are *not* to be understood as operations on segments of the same truth-functional type as the formation of phonemes. Instead we perform an analysis of the language in which feature descriptions are made. And using the results of this we propose a distinction between absolute and contextual features. The sequentiality relationships are expressed in the language used to describe the latter. The empirical conclusions from these constructions seem to support certain ideas of current work by Halle, Liberman, Goldsmith, Hayes, Vergnaud and others.

Phonological Implications of Studies of Aphasic Speech

Peter F. MacNeilage

University of Texas at Austin

Linguistic theory and speech science share a conception of speech which includes a surface (phonetic) level of context-sensitive articulations and an underlying level of relatively context-free entities. A functional approach to the explanation of aphasic segmental speech errors is outlined in terms of three terminal stages of speech output: lexical access, formation of the motor program (underlying level), and generation of speech movement (surface level). Central to this approach is the reinterpretation of the concept of markedness in terms of surface (articulatory) constraints. This accounts for the fact that Speech Apractics, who are known to have articulatory problems, show error tendencies interpretable in markedness terms, while Conduction and Wernicke's who do not have articulatory problems, do not show such error tendencies. Another aspect of the approach is the interpretation of the importance of phonetic similarity in aphasic speech errors in terms of a lowered signal to noise ratio in speech planning mechanisms rather than in terms of the significance of the distinctive feature for linguistic processes.

Simultaneous X-ray Microbeam and Emg Study of Velum Movements for Japanese Nasal Sounds

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The movements of the velum were observed by the x-ray microbeam method simultaneously with the EMG recording of the levator palatini muscle, & the pattern of the muscle activity & its relationship to the velum movement were analyzed. It was confirmed that velum lowering for nasal sounds was characterized by a temporary suppression of the levator EMG and the difference in the velum height between the syllable initial /m/ and the syllable final /N/ was correlated with the difference in the duration of the EMG suppression. It was observed that there was definite activation of the levator muscle for the vowel between the nasal sounds (such as /e/ in /memee/), although the velum stayed at a low position during the vowel period. This would indicate that the nasalization of the vowel was due to mechanical smoothing of the movement. The levator activity for the stop consonants in the condition of /Np/ and /Nb/ was characterized by a markedly higher peak value compared to that in non-nasal environment. It was assumed that there were context dependent adjustments at the motor command level to achieve a full elevation of the velum for these consonants, whereas there appeared no such adjustment for the vowel.

Articulatory and Acoustic Causes of Retroflexion of Apical Implosives

John J. Ohala

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Greenberg (1970) documented a cross-language tendency for apical voiced implosives (=glottal ingresses) to be articulated as alveolars, post-alveolars, or retroflexes even though other non-implosive consonants in the language in which they appear may be dental or even though they may have originated from earlier non-retroflexes, e.g., Sanskrit *dántā* > Sindhi *dandu* "tooth". This tendency can be explained by reference to data from the phonetic literature and from original acoustic, aerodynamic, and dynamic palatographic studies which show that 1) the primary articulatory correlate of retroflexion is an enlargement of the cavity immediately behind the apical constriction, 2) as a class, implosives are characterized by an active enlargement of the oral cavity and apical implosives achieve this in part by lowering and hollowing the tongue behind the constriction, and therefore, 3) apical implosives tend to *sound* retroflexed. Presumably, since they sound retroflexed they may eventually be interpreted as and articulated as retroflexes. This explanation of a common sound pattern,

often manifested in sound change, will be discussed in relation to the recent criticism of Dinnsen and Lass that such phonetically-based accounts fail to achieve the status of *deductive nomological explanations* which are common in other sciences. These authors' characterization of what they perceive as the "shortcomings" of phonetic explanations in phonology are, to a certain extent, accurate. However, deductive nomological explanations in the "successful" sciences, e.g., physics, chemistry, share the same shortcomings when they are applied to *real world* (and not just *textbook*) problems where all distorting factors cannot always be controlled. Taking into account the discipline-specific possibilities for controlling all relevant variables, phonetic explanations in phonology compare favorably with explanations in other scientific disciplines. [Supported by the National Science Foundation and The Committee on Research, Univ. of Calif., Berkeley.]

Analysis of Timing Control for Plosive and Nasal Consonants in Connected Speech

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Clear definition for the timing of articulatory and acoustic events is indispensable for the proper understanding of the timing relationship of segments in connected speech. The work to be reported here is based on the analysis of utterances of both real and nonsense words containing plosive and nasal consonants and vowels of Japanese, uttered by a speaker of the *Tokyo* dialect. Acoustic characteristics that reflect the timing of closure and opening of the vocal tract were determined on the basis of observations of the physiological processes of producing bilabial and alveolar consonants, using the artificial palate and other techniques. These characteristics were then used for the acoustical analysis of the timing of consonant articulation in a number of speech samples containing also velar consonants. Quantitative analyses were made of the durational relationships between a vowel and its neighboring consonants, as well as on the influences of speech rate and word length upon segmental duration. Based on these results, a quantitative model was constructed to predict segmental durations in words consisting of an arbitrary sequence of syllable types under study.

Working Group 16:

Intonation

Organizers: Hiroya Fujisaki¹⁾ and Eva Gårding²⁾

¹⁾University of Tokyo and ²⁾Lund University

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1. Organization of the Working Group

Planning for the working group started as early as in April 1981 when Fujisaki visited Gårding at Lund University. In order to hold condensed and fruitful discussions among specialists actively involved in research, a preliminary invitation was sent out to about 15 people, most of whom agreed to contribute a paper and participate in discussion. Eventually the working group meeting took place with the participation of 12 speakers but opened to other participants of the congress whose number totalled about 110. Eleven papers were contributed in advance and were distributed among the speakers, but were also made available as a preprint booklet to all who participated in the meeting. A more formal publication is being planned for the final form of the papers and discussions. The speaker's names and the titles of their contributions are listed below.

John J. Ohala (Berkeley): Physiological mechanisms underlying tone and intonation

Arthur S. Abramson (Storrs): Intersections of tone and intonation in Thai

Johan 't Hart (Eindhoven): The stylization method applied to British English intonation

Gösta Bruce (Lund): Experiments with the Swedish intonation model

Nina Thorsen* (Copenhagen): Sentence intonation in Danish

Keikichi Hirose (Tokyo): Modelling the dynamic characteristics of voice fundamental frequency with applications to analysis and synthesis of intonation

Philippe Martin (Toronto): Prosodic structures in French

Eva Gårding (Lund): A comparative study of intonation

Zong-ji Wu (Beijing): Rules of intonation in standard Chinese

Hiroya Fujisaki (Tokyo): Word accent and sentence intonation in foreign language learning

Ilse Lehiste (Columbus): Some temporal and tonal characteristics of declarative sentences in Estonian

Karen Kvavik (Madison): Acoustic and linguistic aspects of Parkinson intonation

(*Thorsen could not attend the meeting, but her paper was summarized by Bruce.)

The meeting was opened by an address by Fujisaki, stating the aims of the working group and presenting a list of topics which he and Gårding considered to be appropriate for discussion. The papers, which dealt with various aspects of intonation, were divided into two groups: those dealing with the analysis and quantitative description/formulation of the F_0 contour, and those dealing with the analysis and formulation of the rules of interaction between intonation and other factors, both linguistic and extralinguistic. These two groups of papers were discussed in two sessions, followed by a third session for overall discussion. A brief summary is given of each of these sessions.

2. Session I—Mechanisms, Analysis/Synthesis, and Modeling

This session was chaired by Gårding. Fujisaki as moderator stated briefly his broad definition of intonation, as those features of the spoken language that are expressed by the subjective pitch over a message. Although the time course of the fundamental frequency (the F_0 contour) is the main acoustic correlate, it may also be influenced by such factors as the intensity and the duration. He then showed a schema of how intonation is realized. Information of a spoken message is coded by language-specific rules of intonation into neural commands, which drive the physiological and physical processes of controlling vocal cord vibrations. Referring to the distinction between these two stages, the former as the "software" and the latter as the "hardware" for producing intonation, he emphasized the need for a clear formulation of the latter process as a quantitative model. By incorporating the physiological/physical constraints (which are more or less common to speakers of various languages) into such a model, it would become possible to separate and extract the underlying international structure of an utterance representing the linguistic constraints (which are considered to be more or less specific to a particular language or even to a dialect).

Following Fujisaki's introductory remark, *Ohala* summarized his paper in which he reviewed several aspects of speech physiology relevant to the production of tone and intonation: 1) universals of question/declaration intonation, 2) phonetic manifestation of stress, 3) segmental influence on pitch, 4) duration of pitch fall vs. pitch rise, and 5) language differences in style of pitch control. The review provided evidences for some of the basic properties and constraints of the laryngeal mechanism, and also for the influence of these constraints on the phonological form of intonation.

Abramson described his investigation with Svastikula on the interactions of lexical tone with sentence intonation in Thai, a language with five phonemic tones. The speech materials were recordings of both three-word simple declarative sentences and spontaneous conversation. Declination was found to be more evident in longer, more complex sentences than in shorter sentences where the effect of lexical tone was found to be dominant.

Hart then described his study on stylization of F_0 contours, i.e., approximation of an F_0 contour by piecewise linear curves in the domain of logarithm of F_0 as function of time, as applied to intonation of British English. Perceptual experiments were conducted to compare four versions of stylization rules and the original speech analyzed-resynthesized, and to determine their placement on a psychological scale. It was found that both fully-standardized and partly standardized stylizations are equally acceptable as the original speech which has undergone analysis-resynthesis.

Liberman described a study conducted jointly with Pierrehumbert on the interaction of four factors in determining F_0 contours in English: tune, prominence, declination and pitch range. Speech materials were collected by manipulating these factors independently, and were used to construct a model, in which a local scaling of F_0 values is established by pitch range and phrase position. No clear evidence was found for phrase-level planning of F_0 implementation and phrasal position effects seemed to be limited to a lowering of final pitch accents.

Bruce described results of experiments designed to implement his and Gårding's model of Swedish intonation. He examined 1) the nature of F_0 downdrift, 2) the effect of utterance length, and 3) the effect of attitude. The course of F_0 downdrift was found to be stepwise at the accents rather than gradual over the utterance. Utterance length was found to be signalled by raising the F_0 peak and F_0 values of succeeding accents in proportion to the number of upcoming accents, and the speaker's involvement was found to be expressed by an increase in the overall F_0 range.

Thorsen's contribution on sentence intonation in Danish was briefly summarized by Bruce. In this work, sentence intonation is defined solely in terms of stressed syllables. In short utterances, the intonation contours were found to be quasi-rectilinear, with slopes varying according to syntax and function of the utterance. In longer utterances, on the other hand, the intonation contour was decomposed into phrase group with partial resettings. Phrase group boundaries were found to be only indirectly related to syntax.

Hirose described a joint paper with Fujisaki on a quantitative model for F_0 contours. Unlike most other scholars, they make a distinction between the model of the laryngeal control mechanism and the rules of intonation that controls the mechanism. The entire time course of the logarithm of fundamental frequency is represented as the sum of phrase components and accent components, corresponding to prosodic phrases and lexical accents, respectively. The model itself was shown to be capable of closely approximating F_0 contours not only of Japanese, but also of English and Estonian. On the other hand, the input commands to the model, as inferred by analysis-by-synthesis, were found to indicate the language-specific rules of intonation.

Following these presentations, discussions and comments were made by the speakers and other participants.

Abramson raised the issue of the domain of declination, and suggested the

need for separating the natural downdrift and the linguistically controlled downsteps. He also mentioned that the domain may extend over a number of sentences.

Fujisaki commented that at least in Japanese, and presumably also in other languages, the declination can be observed even within a single vowel segment or a word, and it is considered to be the consequence of the basic physiological/physical properties of the larynx. He also emphasized the need for a quantitative model for the analysis and interpretation of F_0 contours, in order to be able to separate the effects of various factors and to make objective statements on their contribution to an F_0 contour.

Bailey commented on 't Hart's stylization of British F_0 contours. His impression was that 't Hart by doing one of the stylizations had created a contour with a different meaning from the original one.

Martin then posed a question as to the linguistic importance of declination, if the latter is such a universal characteristic of any F_0 contour. Responding to *Martin*, *Lieberman* emphasized the need for separating several different factors that may contribute to declination, such as control of the initial and final values of F_0 , downstepping of F_0 during the utterance, and the autonomous decay process, etc.

Fujisaki commented that, even though there could be many models to describe a single phenomenon within an acceptable range of accuracy, these models have to be evaluated in terms of their relevance to the linguistic structure of the input and the physiological/physical relevance of the transfer function.

Gårding then stated that it is counterintuitive to have the same declination line for both declarative and interrogative sentences. *Fujisaki* in reply commented that one should not confuse the sentence intonation and the declination. He added that even though the characteristics of declination are determined mainly by the autonomous characteristics of the physiological/physical mechanisms of the larynx, and hence follow the natural law of motion and vibration, different languages provide different ways of combatting this natural downdrift, in order to let the speaker use an F_0 range wide enough to express phonemic tones, lexical accents, emphatic accents, etc.

3. Session II—Rules of Intonation

This session was chaired by *Fujisaki*. *Gårding* gave a short introduction. Although the contributors cover different aspects of intonation in a wide range of prosodic systems, they all try to separate different interactive factors, lexical effects and sandhi effects, the influence on intonation from syntax, semantics and pragmatics, including tempo.

Zong-ji Wu as the first speaker showed how the intonation of Standard Chinese varies with sandhi, syntactic grouping, modality and tempo. The four tones form 15 distinct disyllabic tonemes, called ditonemes, which combine into syntactic groups with primary or secondary intonation contours. Different modes affect the ditonemes. Finally tempo has an effect on the modal contours. The

interaction between these factors is most conveniently described as a successive process. The order of the rules expressing this process is the following: sandhi rules, grammatical rules, attitudinal rules and tempo rules.

Eva Gårding described some features in the intonation model presented in her preprint. Phrase boundaries and modality determine the tonal grid which is a set of rising, falling or level lines. This grid shows the general trend of the phrase and sentence intonation and marks the bounds within which local and accentual pitch movements can develop. By expanding and compressing the grid and shifting it up and down the frequency scale we can also accommodate for pragmatic effects including the impression of coherence between sentences in a text. Accents (tones) are expressed as Highs or Lows which retain their positions in relation to the segments even with strong compression and expansion. The model can be used for comparison of intonational systems.

Hiroya Fujisaki summarized the paper he had written together with Miyoko Sugito. In this paper his intonation model has been used to analyse prosodic transfer in the English speech of some Japanese subjects. In terms of the model the Japanese speakers used too many phrase and accent components. They also had an incorrect use of prominent accents on words which should have been unaccented. It could also be shown how the transferred pattern retained dialectal characteristics of the speakers.

Ilse Lehiste gave a summary of her and Fujisaki's analysis of Estonian intonation contours in terms of the latter's intonation model. She commented on two sets of sentences with contrastive F_0 patterns derived from sentences in which a noun is modified by a compound adjective in one set and by two independent adjectives in the other set. The analysis shows additional phrase components and accent components in the longer sentence as compared to the shorter one. The cue most consistently used to disambiguate the sentences is preboundary lengthening which is considerable in the non-compound case.

Karen Kuvavik reported on an analysis of intonation in a group of speakers suffering from Parkinson dysarthria. Some Parkinson subjects appear to have an inappropriately small final F_0 fall from a consistently higher than normal F_0 peak. This may explain the frequently reported clinical observations of monotonicity in connection with Parkinson dysarthria.

Philippe Martin, as the last speaker, presented his model for the syntactic function of intonation. In this model an utterance has two structures, a syntactic structure and a prosodic one. Both may be represented by trees. The elements of the prosodic structure are called prosodic words. A prosodic word has a melodic contour on the final syllable which can be represented by a set of three binary features:

- ± Rising, to express connectedness,
- ± Extreme, to express modality,
- ± Amplitude, to express level in the syntactic hierarchy.

Tests with synthetic speech indicate that listeners prefer isomorphic structures.

Speakers, on the other hand, are free to choose any prosodic configuration as long as the pause pattern agrees with the syntactic structure.

Several speakers from the audience participated in the discussion.

The existence of Chinese sentence intonation was questioned by *Jeng, Wu* amplified his previous statements. The monotonemes and the ditonemes are important basic units which remain rather stable except in fast speech. Different attitudes affect the intonation contour.

There was inconclusive discussion about the nature of French stress which emphasized the need for physiological investigation in this particular field, also mentioned in *Ohala's* paper (*'t Hart, Geimes, Martin, and Gårding*). Equally inconclusive was a discussion about French as a syllable-timed language (*Hara-guchi, Martin, and Lehiste*).

The use of *Fujisaki's* model in cross-language studies raised questions that had already been touched on in the morning session. *Fujisaki* was asked to comment on the relation of the input of his model to linguistic units (*Gårding*), and replied that one needs to make a distinction between analysis and synthesis. In analysis, he uses his model of laryngeal control to extract the underlying inputs and their relationships to the linguistic units, etc. In synthesis, this knowledge is used to convert lexical accents and syntactic boundaries into the input commands for the model.

It was also argued that a hardware model was hardly sufficient for an analysis of prosodic transfer. To understand these phenomena it seems natural to differentiate between transfer of phonological rules and transfer of phonetic characteristics (*Gårding*). *Fujisaki* replied that the hardware model was used precisely to uncover the differences in the underlying phonological rules and to find out how rules of one language are transferred into another language spoken by a nonnative speaker.

Kvavik was asked if the different age means in her normal and impaired speakers could explain the different mean F_0 peaks that had been observed in the Parkinson group (*Ohala*). She answered that she has some data to indicate that age cannot explain the difference. The discussion seemed to show that we know too little about what factors contribute to a monotonous impression in normal speech. There are several possibilities, a small pitch range versus stereotype intonation and accent patterns (*Gårding*).

4. General discussion

The predominant theme for the general discussion was the following: What have we neglected in research and discussion? *Liberman* argued that we had not tried to connect phoneticians' accounts of F_0 with phonologists' traditional accounts. This is true, but at this point it was too late to remedy the situation. *Dubois* wanted to know more about the relation of intonation and rhetoric. There was general agreement on the need for such studies. *Lehiste* was called on to describe her studies of the use of intonation in paragraph structure and *Gårding* showed a figure of intonation contours which have been used, apparent-

ly to create coherence in a text. *Lehiste* asked if we felt ready to talk about typologies of intonation. This seemed to be an interesting question suitable for future research and discussion.

The general discussion concluded with the organizers' thanking the panelists and the audience for their contributions.

5. Afterthoughts

We shall finish our summary with the topics suggested for discussion and some concluding remarks.

(1) Definition and function of intonation

Broad definitions seem necessary until we know more about the form and function of intonation in various prosodic systems. The need for more precise knowledge about the function of intonation at higher levels of speech, including speech acts like text reading and conversation is strongly felt.

(2) How are the basic units combined/extracted?

One basic unit which has attracted a lot of interest is the declination. In the contributions and the discussion there seemed to be a general agreement that at least two factors are involved: an almost autonomous, continuously decaying process, and a controlled, stepping process with communicative function. Strangely enough there is only one model which makes use of inclination (Gårding). The use of inclination seems to be a natural continuation of phrase level intonation in the hard as well as the soft intonation research.

(3) What models should be used, and how should they be evaluated?

Several kinds of models are represented in the contributions. Qualitative and quantitative models are both necessary and complement each other. The use of hard(ware) and soft(ware) should not be given the derogative connotations the words have in some cultures. Generative models focus on the nature of the input, the various stages needed to generate an output which should be as close as possible to the observed contour. A natural requirement is that the input as well as the stages in the generative process should be linguistically and phonetically meaningful. Analytical models typically start with the observed contour and try to analyse it into relevant units. An important question for both methods is the following: How accurate does the output have to be? We are looking forward to results of experiments that explore the thresholds of acceptability and intelligibility in intonation (cf. the contribution by Hirose and Fujisaki).

(4) How is intonation linked to morphology, syntax, semantics and pragmatics?

The link between F_0 and lexicon and morphology is well established by tradition. Other connections are much less known and should be given more attention by phonologists and typologists. It is expected that we will find greater similarities between languages once we leave the lexical and morphological levels.

(5) What and how important are the interactions with other features?

Investigations of the interaction of subglottal, glottal and supraglottal speech

gestures would teach us a great deal not only of the organization of speech but also help understanding different developments in related languages and dialects (tone vs. accent, tone vs. register, accent vs. quantity, etc.)

(6) What are the areas of application of intonation studies?

Apart from the ones mentioned in the contributions we should like to emphasize the role of intonation in perception and speech recognition.

Working Group on Intonation: Organizers' Remarks

Hiroya Fujisaki¹⁾ and Eva Gårding²⁾

¹⁾University of Tokyo and ²⁾Lund University

The purpose of the working group is to present and discuss recent research works on intonation, dealing with both universal and language-specific problems, especially on the formulation of the relationship among objective manifestations of intonation, the underlying linguistic information, and its perceptual consequences. Special emphasis will be placed on quantitative analysis of the fundamental frequency contours and the modeling of the processes that mediate between the linguistic units and their acoustic manifestations. Another topic that will be discussed in depth is the relationship between temporal and tonal organization at various levels of linguistic messages. Although the presentation at the meeting will be restricted to 15 scientists who have made their contributions in advance, the meeting is open to everyone who is interested in problems of intonation. It is hoped that the working group will contribute significantly to our understanding of both universal and language-specific aspects of intonation, and will also indicate directions for future research.

Physiological Mechanisms Underlying Tone and Intonation

John J. Ohala

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University of California, Berkeley

Although considerable progress has been made in the past two decades in discovering how the fundamental frequency (F_0) of voice is produced and controlled, major gaps in our knowledge still leaves us unable to account for most common cross-language patterns in the linguistic use of this parameter. This paper will provide a progress report on attempts to explain universal phonological tendencies in tone and intonation by reference to physical and physiological facts. Among the topics to be discussed will be: 1) the association of low and/or falling F_0 with assertions and high and/or rising F_0 with questions or uncertainty, 2) the greater time taken to raise F_0 than to lower it such that

there is greater tendency for rising tones to exhibit perseveratory tone spreading than for falling tones to do so, 3) the association of creaky voice with low pitch, 4) the interaction of specific consonant types with given tonal perturbations, *i.e.*, voiceless obstruents and voiced implosives with tonal elevation and voiced obstruents with tonal depression. (Supported by the National Science Foundation and the Committee on Research, University of California, Berkeley.)

Intersections of Tone and Intonation in Thai

Arthur S. Abramson and Katyanee Svastikula

University of Connecticut and Haskins Laboratories

The lexically distinctive tones of a tone language may be said to have "ideal" pitch contours that are perhaps best seen in citation forms. Strings of tones in running speech show perturbations of the ideal contours through tonal coarticulation and the effects of segmental features. These tones intersect with sentence intonation, which also makes much use of pitch. For our research we chose Thai, a language with five phonemic tones, because much analytic and perceptual work had been done on its tones. We recorded some 25 minutes of spontaneous conversation and many isolated sentences for acoustic analysis into waveforms, overall amplitude and fundamental frequency (F_0). We looked for (1) "declination," *i.e.*, a drop in F_0 from beginning to end, (2) intonational features at major syntactic boundaries, and (3) interaction between intonation and lexical tone with particular regard to (1) and (2). The frequent use of declination in Thai is well supported by our data. We describe its size and domain. Some intra-sentence syntactic boundaries show a rise in F_0 or, in sentences with declination, at least a failure to fall. The interaction between tones and intonation does not generally cause a loss of tonal contrast, although it affects the tonal contours.

The Stylization Method Applied to British English Intonation

Johan 't Hart

Institute for Perception Research, Eindhoven

The aim of this study was to answer the following two questions: 1) Is it possible to stylize F_0 curves of spoken British English (BE) utterances using interestingly small numbers of straight line segments, as it has been shown to be possible for Dutch intonation? 2) Is it possible to develop a relatively simple melodical model which can account for the most frequently occurring intonation patterns of BE?

Stylization was executed with the aid of an interactive LPC analysis-resyn-

thesis system, and took place in a number of steps, from "close-copy stylizations", which are shown (in Exp. I) to be undistinguishable (by native speakers of BE) from the originals, to "fully standardized contours". In Exp. II, the acceptability has been tested of such contours as compared to, among others, natural BE intonation and stylized contours composed on the basis of rules for Dutch intonation. The standardized contours turned out to be equally acceptable as the original versions, whereas the 'Dutch' versions were considerably less acceptable, which indicates that this favourable result is not due to lack of sensitivity in the—again native—subjects.

Intonational Invariance Under Changes in Pitch Range and Length

M. Y. Liberman and Janet Pierrehumbert

Bell Laboratories

We have studied the interaction of four factors in determining F_0 contours in English: tune, prominence, declination and pitch range. In two experiments, these factors were manipulated independently, and the patterns apparent in the resulting data were used to construct a model of intonational realization. In this model, pitch range and phrase position establish a local scaling of F_0 values. This transform may be seen as a sort of time-varying graph paper on which F_0 values are plotted. Each speaker has an invariant baseline, or floor on F_0 values; the character of the "graph paper" above the baseline depends on the phrase's overall pitch range. We find no clear evidence for phrase-level planning of F_0 implementation. All computations can be made on pairs of adjacent pitch-accents, and the parameters of the transform need not be set differently for different phrase lengths. Phrasal position effects seem to be limited to a lowering of final pitch accents.

Experiments with the Swedish Intonation Model

Göta Bruce

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A series of experiments were conducted to test an earlier model for Swedish intonation where local specifications for accentuation were inserted in a global baseline-topline structure. The course of the F_0 downdrift is found to be step-wise at the accents rather than gradual over the utterance. The speaker signals utterance length by raising the F_0 peak and the F_0 values of succeeding accents in proportion to the number of upcoming accents, rather than by varying the slope inversely with utterance length. The involvement of the speaker is expressed as an increase in the overall F_0 range with a preservation of the F_0 rela-

tions between the accents within the utterance. The F_0 bottom of the speaker's range appears to be fixed, while the F_0 maximum is variable. These findings indicate that a more adequate account of the relationship between accentuation and intonation should give a more explicit role to their interaction than is achieved by merely adding accentuation to intonation. It is suggested that the overall F_0 course of an utterance is essentially formed by the relations between successive, local excursions for accentuation.

Sentence Intonation in Danish

Nina Thorsen

Institute of Phonetics, University of Copenhagen

A definition of sentence intonation in Standard Danish solely in terms of the stressed syllables in the utterance probably reflects a productional as well as a perceptual reality and has obvious descriptonal and pedagogical advantages. In short utterances, containing no more than four stressed syllables, the intonation contours are quasi-rectilinear, with slopes varying according to the syntax and function of the utterance: most falling in terminal declarative sentences at one extreme, and horizontal in syntactically unmarked questions at the other extreme. The only instance of asymptotical declination is found in the "topline" (*i.e.* the connection of local fundamental frequency maxima, which are constituted by the first post-tonic syllable in each stress group) above non-falling intonation contours.

With four and more stress groups, the intonation contour is decomposed into phrase group contours, with partial resettings between them, still preserving an overall downdrift, however. There is a tendency for fundamental frequency range to increase with increased utterance length, but in a non-linear and seemingly random fashion. The increase is brought about by higher starting points as well as by lower ending points. Concomitant with the range increase we find a decrease in overall downdrift in the longer utterances, but degree of downdrift is not simply inversely related to utterance length.

The boundaries between phrase groups are only indirectly determined by the syntactic structure: In syntactically unambiguous non-compound sentences the phrase groups will cut across any syntactic boundary, and the boundary location bears no simple relation to surface syntactic structure. Furthermore, the semantic content of the syntactic constituents may influence the prosodic structuring.

Modeling the Dynamic Characteristics of Voice Fundamental Frequency With Applications to Analysis and Synthesis of Intonation

Hiroya Fujisaki, Keikichi Hirose and Noboru Takahashi

Faculty of Engineering, University of Tokyo

The intonational information of a message is realized through the speaker's phonatory mechanism which generates and controls the contour of the fundamental frequency (F_0) of voice. Thus the F_0 -contour reflects not only the prosodic structure of an utterance, but also the physiological and physical properties of the phonatory mechanism. For a precise analysis of the intonational phenomena, therefore, the two factors ought to be separated. The present paper describes a quantitative model of the control process of F_0 , originally proposed by Öhman (1967) and further elaborated by Fujisaki *et al.* (1971, 1982), which can be used to separate these two factors and thus to extract the underlying intonational structure in the form of input commands to the model. Analysis of utterances of three quite different languages, *i.e.* Japanese, English, and Estonian, indicates that the model is capable of expressing the dynamic characteristics of the F_0 -contour in all three languages. The timing and the amplitude of the input commands, on the other hand, are much more language-specific and represent essential differences that exist among the intonational structures of these languages. Problems such as the F_0 -declination the number of accentuation levels, and the intra- and inter-speaker variability are discussed mainly based on analysis of the Japanese utterances.

A Model for French Intonation

Philippe Martin

Experimental Phonetics Laboratory, University of Toronto

This model describes F_0 movements (contours) on stressed syllables of the sentence, in order to account for the syntactic function of intonation. It postulates that utterances have a syntactic structure (SS) and a prosodic structure (PS), and that the two are related to each other in nontrivial way.

The SS is considered only in its geometrical properties, *i.e.* as a geometrical classification of syntactic units, which can be connected or disconnected, planar or non-planar. Connectedness refers to the possible partitioning of a syntactic unit by another unit, planarity deals with the possible crossing of branches in the tree representing the SS. The PS organizes a hierarchy of units called prosodic words. Each prosodic word contains one stress, whose melodic (F_0) contour is described by language-specific phonological features. These features are in turn generated by a language-specific grammar. In French, this grammar accounts for

the observed contrasts in slope (rising or falling), amplitude variations, and duration. Unlike the SS, the prosodic structure correlated with those contours is always connected (*i.e.*, no discontinuous prosodic words are allowed) and planar.

The two structures are related to each other by the property of congruence, which stipulates that the grouping of elements in one structure cannot contradict the grouping of elements in the other structure. There is therefore no one to one correspondence between the SS and the PS, and more than one PS can be derived from the same SS.

This model describes accurately observed F_0 contours in French, and has been widely used in speech synthesis. Its validity has been established for many speakers in various conditions (spontaneous speech *vs.* reading, *etc.*). Actual quantitative aspects of its prediction will be discussed, as well as its compatibility with other descriptions of French intonation.

A Model for Comparison of Intonation

Eva Gårding

Department of Linguistics and Phonetics, Lund University

I shall contribute a description of a generative intonation model, originally developed for Swedish dialects (Bruce and Gårding, A prosodic typology for Swedish dialects) but in its latest version made applicable to other prosodic systems as well (Gårding, Botinis and Touati, A comparative study of Swedish, Greek and French intonation). This model generates syllable structure, syllable durations and an intonation contour from abstract markings representing syntactic boundaries, accentuation and mode.

There are several stages in the model. The rules for syllable structure and syllable duration come first, followed by intermediary phonological rules, which delete *e.g.* accent marks after focus. The abstract input symbols are then exchanged for concrete ones, namely Highs, Lows or combinations of these. Finally there is an algorithm giving the pitch curve. Here the Highs and Lows are distributed on a tonal grid representing sentence intonation, and the final pitch curve is obtained by interpolation between these points over the voiced segments.

The use of an input which is similar in pragmatic, syntactic and phonological structure, and the same generative scheme for all compared languages gives us a tool for comparison and description of prosodic similarities and dissimilarities which is of typological as well as pedagogical interest.

In the three languages compared so far, statement intonation is similar but accentuation differs. There are also differences in syllable structure and hence in syllable duration and overall length. The model is also being used to describe prosodic transfer in a foreign accent and to clarify aspects of learning difficulty and acceptability.

Rules of Intonation in Standard Chinese

Zong-ji Wu

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The sentence intonation in Standard Chinese (SC) varies in contour pattern with four levels, interfered one by one from intrinsic to extrinsic, as a series of complex functions, *i.e.*

phonological	v.w.*	grammatical	v.w.	attitudinal	v.w.	tempo of
tone sandhi		modification		compensation		speech.

Phonologically, monosyllabic tonemes and disyllabic ditonemes which form a definite number of lexical contourems play a role of basic units in intonation. Grammatically, lexical contourems may be modified more or less according to different sense groups, emphatic words and deep structures to form a sequence of primary contours and secondary ones. Attitudinally, in addition to the declarations, there are three kinds of attitudes which interfere with the intonation contours to cause more compensations. In the interrogations and the exclamations, the overall sentence contours may change their average registers, spreading ranges or ending drifts. In a hastened speaking, the more the tempo is speeded up, the more the spreading contour is flattened. In other words, the increasing of the amount of syllables within a definite interval inversely proportions to the spreading range, and *vice versa*.

Since the sentence intonation in SC being a compound varies with many features of modifications, compensations, *etc.*, it seems rather complicated but can be predicted by rule.

*v. w. = varies with

Word Accent and Sentence Intonation in Foreign Language Learning

Hiroya Fujisaki¹⁾, Miyoko Sugito²⁾, Keikichi Hirose¹⁾ and Noboru Takahashi¹⁾.

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It is well known that a speaker tends to transfer the intonational features of his/her native language into utterances of a non-native language. Objective and quantitative analysis of such "intonational transfer", however, has been rather scarce because of methodological difficulties. The present paper describes an approach using a model of the control process of voice fundamental frequency (F_0) proposed by one of the authors. The analysis allows one to estimate not only the parameters of the control mechanism of F_0 , but also the parameters of the underlying commands that are assumed to give rise to changes in F_0 . Utterances

of the same English sentences were collected from three native speakers of American English and four native speakers of Japanese. While the dynamic characteristics of F_0 can be closely approximated by the same model for all seven speakers and suggest the similarity of the control mechanism itself, the estimated underlying commands are markedly different in a number of features, such as the presence/absence of a phrase command at a syntactic boundary, the way in which several lexical items form a prosodic word, and the use of the accent command to generate interrogative intonation. The analysis also revealed dialectal differences among the Japanese speakers, two from the *Tokyo* dialect and the other two from the *Osaka* dialect. These differences in the tonal features, together with differences in the temporal features and in their coordination, may constitute major difficulties in foreign language learning.

Temporal and Tonal Characteristics of Declarative Sentences in Estonian

Hiroya Fujisaki¹⁾ and Ilse Lehist²⁾

¹⁾University of Tokyo and ²⁾The Ohio State University

The lexical and syntactic information of an Estonian sentence is reflected both in its temporal and in its tonal characteristics. While the temporal characteristics, manifested as changes in duration of syllables and pauses, lend themselves to precise acoustic measurement, the tonal characteristics are difficult to extract and analyze without the use of a quantitative model. For this purpose, the present study adopts a model of the fundamental frequency contour that has proved itself valid for describing the tonal characteristics of Japanese and English [Fujisaki and Sudo (1971), Proc. 7th ICA, vol. 3, 133-136. Fujisaki and Hirose (1982), JASA, vol. 71, Suppl. 1, S7]. Utterances of 30 declarative sentences were collected from seven native speakers of Estonian, and were analyzed. In particular, investigations were made into the acoustic characteristics that serve to discriminate a set of two adjectives independently modifying a noun, from the case where the two adjectives form a compound. The changes in temporal characteristics are found to be almost always accompanied by changes in the tonal characteristics, such as termination/continuation of an accent component and presence/absence of a phrase component. The relative perceptual importance of these features is also discussed.

Acoustic and Linguistic Aspects of Parkinson Intonation

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Speech Motor Control Laboratory, University of Wisconsin

Acoustic and linguistic data are presented for normal and parkinson intona-

tion. Fundamental frequency, length, and intensity are examined in sentence-final intonations for declaratives and non-declaratives. Of interest are: to what degree the parkinson subjects have reduced or less variable pitch range; whether there is a lack of falling (or rising) terminations; whether the parkinson subjects differ linguistically in accent placement and use of tones; and to what degree syllable intensity and length differ from the normal subjects. The subjects are male, age 65+, and of comparable dialect and education. The F_0 analyses were made by means of a computerized pitch tracker. Our results are based on a language-situation protocol, designed to elicit more linguistically natural responses, *vs.* the customary reading or repetition protocol used in the evaluation of impaired speech.

A systematic and fine-grained linguistic and acoustic study of parkinson intonation has been undertaken in order to elucidate conflicting claims in the speech pathology literature. The literature is mainly based on listener evaluations and generally characterizes the speech as "bizarre", with monotonic pitch, reduced stress, and monotony of loudness (Darley, Aronson, Brown, 1975:193-195). It has been claimed that expressiveness is lost and that final falls may disappear completely (Grewel, 1957:444). Some researchers report that pitch levels are too high, some too low. On the other hand, Canter's study reports that intensity was comparable to normals: pitch ranges were restricted, and that duration did not differ systematically (1963:228).

Working Group 17:

Shared Knowledge in Language Use

Organizers: Jeanette K. Gundel¹⁾ and John Hinds²⁾

¹⁾Minnesota and ²⁾Pennsylvania

1236

Shared Information in Japanese Conversation

John Hinds

Pennsylvania State University

A distinctive characteristic of the Japanese language is the extent to which ellipsis occurs in normal conversational interactions, as well as in formal writing styles. The elements which may be ellipted include, but are not limited to, nominals, postpositional particles, and main verbals. Descriptions of this phenomenon vary according to theoretical framework, but adequate descriptions typically state that ellipsis is possible when the speaker assumes the addressee can fill in the missing information based on linguistic or extra-linguistic knowledge. The major concern of researchers into this phenomenon is the specification of conditions under which ellipsis is possible.

Native language authors frequently 'explain' the pervasiveness of ellipsis in the Japanese language through appeals to the homogeneous character of Japanese speakers. The experience of being Japanese, in this sense, provides a wealth of shared information which does not have to be conveyed overtly for successful communication.

This paper reports on an experiment which is currently being designed to measure the degree to which native speakers of Japanese alter their speaking styles vis-à-vis their conversational partners. Six brief narratives are being written in Japanese, with controlled length, number of propositions, cultural relativity, and story complexity. Male and female subjects will be asked to read these stories one at a time and then immediately retell the stories to (a) their spouse, (b) a close friend of the same sex, (c) an acquaintance of the same sex, (d) an acquaintance of the opposite sex, (e) a stranger of the same sex, (f) a Japanese-speaking foreigner of the same sex. The hypothesis is that as the conversational partner becomes less 'homogeneous', there will be less information ellipted. If this hypothesis is borne out, it will provide a direct correlation between shared knowledge and linguistic forms.

'Shared Knowledge' and the Cooperative Principle

Jeanette K. Gundel
University of Minnesota

Various explanations have been proposed to account for the fact that a person who uses the (a) sentences in (1) and (2) treats certain aspects of the meaning of those sentences as non-informative to the addressee which are not treated as non-informative in the corresponding (b) sentences.

- (1) a. The woman with a British accent called this morning.
b. A woman with a British accent called this morning.
- (2) a. What was stolen was a tape recorder.
b. A tape recorder was stolen.

Attempts to deal with such facts within a theory of presuppositions (logical or pragmatic) or strictly in terms of Gricean implicatures have all been shown to be inadequate in one respect or another (see, for example, Wilson 1975 and Wilson and Sperber 1979 for discussion).

Wilson and Sperber propose to account for such facts within a modified truth-conditional semantics in which entailments are 'logically ordered' in a way that makes it possible to "distinguish, for a given utterance, those entailments which are centrally important, or focalized, from those which are peripheral." The basis for this ordering is the syntactic form and phonological prominence of the constituent with which the entailment is associated.

I will argue in this paper that Wilson and Sperber's proposal is also inadequate. As it stands, it can not distinguish between sentences like (1a) and (1b) since the entailment that there is a woman with a British accent is associated in both sentences with a noun phrase that normally does not receive primary stress, a fact which would put it equally low on the relevance scale for both sentences. Furthermore, since (3), unlike (1a), does not entail the existence of a woman with a British accent, Wilson and Sperber's model would incorrectly predict that what is treated as non-informative in these two sentences is different.

- (3) The woman with a British accent didn't call this morning.

I will try to show in this paper that the above facts are most naturally handled within a theory of language use that incorporates, with some refinements, independently needed principles concerning the topic-comment structure of sentences, as put forward in Gundel 1974 and elsewhere.

Since it is the function of topics to relate a speech act to some entity familiar to both speaker and addressee (cf. Grice's Maxim of Relation), variations in the 'strength of presuppositions' associated with a particular constituent will be a function of the likelihood that that constituent is interpreted as the topic of the sentence. Thus, for example, given the fact that unstressed definite subjects in

English are normally interpreted as topics, it follows that the existence of a Queen of Peru is more likely to be treated as non-informative in (4) than in (5).

- (4) The Queen of Peru read the report.
- (5) The report was read by the Queen of Peru.

A similar explanation can be given for differences between *wh*-clefts like (6) and corresponding *it*-clefts like (7), which were pointed out in Prince 1979.

- (6) What made her decide to leave was the climate.
- (7) It was the climate that made her decide to leave.

Since the embedded *that*-clause is less likely to be interpreted as the topic in (7), (7), unlike (6), can be used to assert the information in the embedded clause as new.

Fancy Syntax and 'Shared Knowledge'

Ellen F. Prince

University of Pennsylvania

First, one must distinguish between two phenomena that have been called 'shared knowledge': (1) information that the speaker hypothesizes the hearer has (and hypothesizes the speaker has . . .), akin to Clark and Marshall's (1981) 'mutual knowledge', and (2) information that the speaker hypothesizes the hearer is, or could appropriately be, currently attending to, akin to Chafe's (1974) notion of 'givenness'. It is the second type, which I shall henceforth call 'Chafe-given', that is at issue in this paper.

As is well-known (Halliday 1967, *inter alia*), sentences with an intonationally marked focus treat some (open) proposition as Chafe-given, the intonationally marked focus supplying the 'new' information, i.e. the value of the variable in the open sentence. For example, 1 marks the proposition represented in 2 as Chafe-given and adds to the discourse model the value of the variable represented by the English word *something*:

- (1) John gave a BOOK to Mary.
- (2) John gave something to Mary.

In addition, as is well known (Chomsky 1971, among others), certain marked syntactic constructions are 'presuppositional' in that they, too, mark some open sentence as Chafe-given and 'close' the sentence. Thus, the stressed-focus *it*-cleft in 3 likewise marks the proposition in 2 as Chafe-given:

- (3) It was a BOOK that John gave to Mary.

It is the thesis of this paper that a number of marked syntactic constructions not standardly thought to be 'presuppositional' in fact are, i.e., mark some open

proposition as Chafe-given, and that such constructions have two other nonobvious similarities: a fixed stress pattern and a perceptible 'gap'. Two such constructions to be examined here are Topicalization and Gapping, exemplified in 4 and 5, respectively:

- (4) To John I gave a BOOK.
- (5) Sam ate fish and Harry meat.

Topicalized sentences, it will be argued, have a two-fold function: they mark the entity represented by the leftmost NP as being in a particular, highly constrained relation to entities already introduced; and they mark the open sentence, arrived at by replacing the tonically stressed constituent by a variable, as being Chafe-given. (See Prince 1981).

Gapped sentences, or rather the conjunction of a canonical sentence with a gapped sentence, it will be argued, mark a *single* proposition with *n* variables as Chafe-given, where *n* = the number of constituents in the gapped conjunct, the constants in the Chafe-given proposition representing the nonrepeated material in the first conjunct. (See Kuno 1976). Thus 5 marks 6 as Chafe-given:

- (6) Someone ate something.

Of course, a sentence marking some open proposition *P* as Chafe-given will be felicitous in a particular discourse only to the extent that the speaker's assumption that *P* is or could appropriately be in the hearer's consciousness is warranted. The unacceptability of many Topicalizations and Gappings will be seen to follow directly from this, e.g.

- (7) (discourse-initial)

Someone interesting/Two guys I met yesterday.

- (8) Q: Why did Harry eat meat?

A: Well, you know, whenever Sam eats fish, Harry eats meat, so yesterday Same ate fish and Harry meat.

In contrast, I shall try to show that other marked syntactic constructions do not have the function of marking propositions as Chafe-given, e.g. Left-Dislocation, Passive, There, but have rather functions relating to processing, to the rearrangement of entities for thematic reasons, etc.

Finally, I shall suggest that Wilson and Sperber's (1979) ordered-entailment model, with some slight modification, provides a natural and intuitive way of describing and distinguishing among the phenomena discussed.

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Modality and Conversational Background

Dieter Wunderlich

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It has often been said that the modal verbs in languages like English and German can be used either to express factual conditions on a process as permission, obligation or ability, or in the epistemic sense to express degrees of (subjective) probability. Consider, for example, the famous pair of sentences of Halliday's:

- (1) You must be very careful.
- (2) You must be very careless.

where (1) most likely expresses a requirement and (2) a probability. Moreover, to account for these two senses it has been claimed that sentences like (1) and (2) will arise from two distinct deep structures.

This ambiguity thesis has seriously been challenged by Kratzer's work. Her assumption is that each modal verb has a unique meaning, with the requirement that any utterance of it is related to some conversational background. The differences between (1) and (2) then arises from the different backgrounds most likely associated with these sentences.

More specifically, the modal verb in an utterance indicates an inference the speaker wants to make. Hence, the relevant conversational background can be reconstrued as a hidden premise of this inference. Consider

- (3) a. You want to go mountaineering at night.
b. Everybody who goes mountaineering at night, is very careful.
c. = (1) You *must* be very careful.
- (4) a. You want to go mountaineering at night.
b. Everybody who goes mountaineering at night, is very careless.
c. = (2) You *must* be very careless.

The inferences the modal verb here indicates (itself not being part of the inferred sentence) are obviously alike. The difference in the understanding of (1) and (2) goes back to the different roles which maxims like (3b) and (4b) play

in our system of knowledge: (3b) says what a good mountaineer behaves like, whereas (4b) says how mountaineering of this kind is generally judged.

If the conversational background for utterances with a modal verb is not yet established as shared knowledge, the hearer is nevertheless given some hints for reconstructing it, for instance, in a shift of contrast of modal verbs.

In a project on the discourse functions of modal verbs in German, we investigated many instances of factual discourse to find out the principles by which speakers relate to a particular kind of conversational background (as given by the type of discourse, social relationship and daily knowledge). We also investigated the verbal constructions which serve for establishing a relevant background. Consider, for example, the antecedent of a modalized conditional:

(5) If you want to visit Hungary, you must have a visa.

Here, the conversational background for the use of *must* is determined by both the desire and its content, i.e. the entry regulations for Hungary.

If the use of modal verbs is based on conversational background, it is by principle vague. At the same time it is very efficient. For understanding can be secured by the shared knowledge without verbal means.

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Bias, Individual Differences, and Shared Knowledge in Ambiguity

Joseph F. Kess and Ronald A. Hoppe

University of Victoria

There are two types of shared knowledge germane to the question of ambiguity detection and resolution. First, there is the fact of bias in the detection of and resolution for ambiguous structures in natural language. Despite the care that many psycholinguistic experiments have exhibited in selecting sentences which have an even possibility of either interpretation being given, there are obviously an astounding array of sentences within natural languages that have more than one reading as their possible interpretation set. Yet many of these sentences pose little or no problem in ambiguity comprehension; in psycholinguistic tasks requiring subjects to detect multiple readings for such sentences, there seems to be an ordered hierarchy of possible readings which must be the reflection of a priori shared knowledge. Indeed, some readings seem scarcely possible in the light of such prior knowledge constraints, so much so that possible linguistic arrangements are never processed as having meaning

because their real-world knowledge possibilities are either limited or nil. Moreover, our research has found that such biasing constraints operate even in the presence of context, so that the plausibility of certain readings is not exclusively the result of context, but rather of the interaction of context and the sentence's inherent bias.

Secondly another interpretation of the notion of shared knowledge revolves around the basic concept of individual differences in language comprehension and language use. Linguistic theory must posit an ideal speaker/hearer, to whom the entire array of linguistic abilities which define his membership in the speech community are identically available. Yet when one looks closely at performance considerations in several areas, as for example, the ability to detect and resolve ambiguity or to paraphrase, it is obvious that there is inherent variability among speaker/hearers. Ambiguity detection and ambiguity resolution, for example, are not linguistic abilities which are exhibited in the same fashion or in the same degree by all subjects in psycholinguistic tasks; rather, one finds considerable individual differences in this area. This paper also offers some commentary on the role of individual differences and its interaction with the notion of shared knowledge, specifically in the area of ambiguity.

Direct and Indirect Speech in Japanese

Florian Coulmas

University of Düsseldorf

One of the functions of the Japanese complementizer *to* is to mark a sentence as a quotation. *To* is preceded by both direct and indirect speech and followed by a verb of communication. While in indirect speech *to* is thus similar to and usually translated as English *that*, this particle lacks an English equivalent in its function of marking direct quotation.

Indirect speech is a form of paraphrase where the speaker translates someone else's utterance into his own words. In so doing he makes appropriate switches in deictic reference, such as tense, location, honorifics, etc. Owing to the fact that the word order is the same in direct and indirect quotation in Japanese and that both forms of reported speech are marked by the particle *to*, many Japanese sentences are ambiguous in the sense that, if seen in isolation, they could represent either direct or indirect speech. The focus of this paper is on sentences of this kind, and on the question of how shared knowledge is involved in deciding whether an utterance is reported directly or indirectly.

Inferring Mutual Belief

Jerry R. Hobbs

Typically, a speaker S tells a hearer H what S believes H doesn't know in terms of what S believes H and S mutually believe, and H interprets the utterance in terms of what H believes H and S mutually believe. It is thus not enough to specify procedures whereby S and H can generate and interpret utterances using some knowledge base. We must also give an account of how S and H are able to tailor the knowledge base specifically to each other. That is, for any fact P they must be able to infer whether they mutually believe P. Examples of three ways such inferences could be drawn are given. (1) The knowledge base includes facts which tell what other facts are about, thus effectively indexing facts by who else knows them. (2) It includes general facts related shared experience and mutual belief. (3) Fairly complex inferences can sometimes be computed "on the fly". This account is then applied to problems raised by the notion of "levels" in stories—a real author becomes an implied author and uses a narrator who reports something that was told to him in the distant past. We may view the move from one level of a story to another as a transformation on the knowledge base we use for interpretation. Such transformations can be effected by the same processes that one uses for tailoring a knowledge base for a co-conversant.

Shared Knowledge and Communication

G. P. Boguslavskaya

Mordovian State University

An important feature in any real life situation is shared knowledge. Nearly all our conversations involve unspoken assumptions, unconscious prejudices, or shared knowledge.

Shared knowledge may be understood in the *narrow* sense of the word, namely shared experience and hence the absence of any necessity to speak about the facts known to the interlocutors on the one hand and the easiness in establishing contacts on the other hand.

Shared knowledge may be understood in the *wide* sense of the word, i.e. the same cultural level (standard of culture) of the interlocutors. Rather often people of different standards of culture find it difficult to communicate.

And at last *shared knowledge* may be understood in still *wider* sense of the word meaning people coming from different countries representing different cultures (civilized habits) which can lead to different misunderstandings.

Working Group 18:

The State of the Art in Language Typology

Organizer: Christian Lehmann
Universität Köln

Discussants: Ch. Lehmann¹⁾, Eugenio Coseriu²⁾, Gilbert Lazard³⁾ and Edward Keenan⁴⁾

¹⁾The role of grammaticalization in linguistic typology"

²⁾Tübingen, "Typologie des langues et typologie des procédés linguistiques"

³⁾Paris, "Typologies et types linguistiques"

⁴⁾Los Angeles, "Universal grammar and the passive"

Summary

Preparing the Working Group, a questionnaire containing 25 questions on general problems of linguistic typology had been predistributed among participants, and an abbreviated version of the answers obtained was included in the preprints. This enabled the Working Group to presuppose background information on the present state of linguistic typology and to concentrate rather on more specific themes. In accordance with the papers presented, the discussion centered around three main themes: 1. the possibility of a (holistic) language typology; 2. grammaticalization; 3. parameters of universal grammar.

Ad 1: Proceeding inductively, typology starts from a cross-linguistic analysis of a single category or construction. Combining such analyses of different features, it then seeks connections among them. The crucial concept here is typological clustering. However, the more comprehensive such clusters are, the looser the connections within them become, the more we must allow for variation. Meillet's notion of 'la Langue' as a 'système où tout se tient' has to be modified in the light of typological work. The hope to arrive at a global typology by combining ever more features in an inductive way appears to be utopic. The language type must be conceived of as a set of interrelated principles which govern the structuring of languages. These can only be found by supplementing the inductive by the deductive method. Only then can the language type become an explanatory concept.

Ad 2: The functional domain, as the common denominator of a typological comparison, is structured by various principles and parameters. One of these is grammaticalization. This accounts for the scalar nature of the domain and for the graduality of the differences among the categories united by it. Functionally similar grammatical categories form a continuum, whose focal instances mark the points preferably selected by languages.

Ad 3: Implicative or absolute generalizations state the distribution of Properties across languages. Such statements are derived from statements of

universal grammar, with its variables, parameters and constraints. The different forms which the passive takes in various languages are provided by universal grammar, and forms which do not occur are excluded by it. However, such an approach relies on the possibility of providing a crosslinguistic definition of the grammatical categories involved in such statements.

Questionnaire

Cross-references between questions are meant to insure that your answers are mutually consistent. All questions except 5, 19 in part, and 20 ff. are intended as theoretical questions; i.e. they ask not so much what is currently happening in linguistic typology, but which provisions linguistic theory should make, on the basis of available evidence.

1. What is a language universal?
2. What are the tasks and methods of linguistic typology?
3. What is the difference between linguistic typology and universals research? (→ 1, 11a)
4. A holistic language type comprises all linguistic levels from semantics to phonology. Is there (the possibility of) a finite set of holistic language types?
5. If 4 = yes: What empirical evidence is there for a finite number of holistic types?
6. If 4 = no: Is there (the possibility of) a finite set of morphosyntactic (grammatical) types?
7. If 4 = no and 6 = yes: How are semantics and phonology connected with such morphosyntactic types? (→ 12, 13)
8. - 10. If 4 or 6 = yes:
8. As to the substance, the constitutive element of a language type:
 - (a) Does the type center around a specific set of grammatical features which are basic to language structure (and to typology)?
 - (b) If a = yes: Which are these, and how is their special status justified? (→ 19)
 - (c) If a = no: What is the unifying factor/principle of the language type?
9. What is the formal structure of a language type? Please comment on the notions of implication, clustering, equilibrium and hierarchical layering of properties.
10. (a) Are language types disjunct or do they overlap?
 (b) What is the relation between a language and a language type? Does every language belong to just one type, or to several types?
 (c) If types are disjunct: Please argue with W. von Humboldt ("Über die Verschiedenheiten des menschlichen Sprachbaues", § 33): "einer... solchen Klassifikation, wo auch die gar nicht stammverwandten Sprachen nach allgemeinen Ähnlichkeiten ihres Baus zusammengestellt würden [i.e. a typological classification], widerstrebt, wenn man den Begriff genau nimmt und fordert, daß die zusammengestellten wirklich als Gattungen in allen wahrhaft charakteristischen Merkmalen einander ähnlich und von anderen

verschieden sein sollen, die tiefer erörterte Natur der Sprache selbst. Die einzelnen Sprachen sind nicht als Gattungen, sondern als Individuen verschieden ... Das Individuum, als solches genommen, füllt aber allemal eine Klasse für sich."

(d) If types are not disjunct: What is the difference between a language type and any empirically confirmable combination of properties?

11. If 4 and 6 = no: (a) How is linguistic typology distinct from general comparative (or universal) grammar? (\rightarrow 3)
(b) How can we avoid typologizing on just any feature? How can the preferential status of certain grammatical properties in present-day typology be justified?
12. (a) Is there (the possibility of) a finite number of phonological types? (\rightarrow 7)
(b) If a = yes: What are the criteria?
(c) If a = no: Should phonological typology (as distinct from general comparative phonology) be pursued?
13. Can there be a semantic typology (distinct from syntactic and morphological typology)? What would it deal with? (\rightarrow 7)
14. Is there, within morphosyntactic typology, a genuine difference between form-oriented and content-oriented typology (as G. Klimov claims)?
15. (a) Is there any need for quantitative typology?
(b) If a = yes: What must it contribute?
(c) If a = no: \rightarrow 10 a/d
16. (a) Can the typological perspective shed any light on language change? I.e. is there a necessity for a diachronic typology as distinct from general diachronic linguistics?
(b) What is the use of typology in historical reconstruction?
17. What is the *tertium comparationis* for types (no matter whether in whole-system (holistic) or part-system typologies)? Is there any such thing as an *étalon* language (B. Uspensky)?
18. Which are the explanatory principles for the limits of linguistic diversity and for typological clustering? How are they related to language universals?
19. Please comment on the actual merits and possible prospects of the following typologies currently promoted (\rightarrow 8b, 11b):
—fundamental relations (ergative, accusative, active (etc. ?));
—basic order (SOV, VSO etc.);
—role and reference (subject-prominent, topic-prominent etc.);
—morphological typology:
 degree of fusion, degree of synthesis;
 concentric vs. excentric construction (i.e., roughly, personal affixes vs. case affixes, T. Milewski);
 —other.
20. In which regards are the present questions biased; which significant aspects/problems of present-day linguistic typology have been overlooked?
21. Please indicate up to five of your recent publications which are most

- directly relevant to linguistic typology.
22. Please indicate up to five recent publications by other researchers which have significantly advanced linguistic typology.
 23. What are you currently working on within linguistic typology (topic(s), paper(s) in preparation)?
 24. Which typological theme(s) do you plan to work on when your present work is finished?
 25. Personal bibliography
 26. Alien bibliography

The Role of Grammaticalization in Language Typology

Ch. Lehmann

Universität Köln

It is not functionally indifferent whether a concept such as number is expressed by fusional suffixes or, e.g., by juxtaposed nouns of multitude; this correlates with a (gradual) semantic difference.

A typologically relevant order among the various phenotypes of such grammatical categories as number is effected by grammaticalization. This is a process which subjects lexemes to desemanticization and phonological attrition, until they become grammatical formatives. In this course, they become increasingly dependent and obligatory, their paradigmatic and syntagmatic variability decreases, and the cohesion of the construction integrated by them increases. As elements exceed a certain degree of grammaticalization, they are renovated in periphrastic (analytic) constructions. Since grammaticalization affects both the signans and the signatum of linguistic signs in a parallel manner, the phenotypes of grammatical categories in individual languages may be compared as to their degree of grammaticalization. To the extent that semantically related categories are similarly expressed in a language, several grammatical categories will show the same degree of grammaticalization, which leads to a certain typological clustering. Within each language, only a limited number of degrees of grammaticalization are employed, so that a language type may be partially defined by the distribution of grammatical categories over such levels of grammaticalization. Finally, since grammaticalization determines not only a certain synchronic distribution of functional variants of a grammatical category, but is also a diachronic process which changes the constitution of grammatical categories, it may also be used to describe typological change.

On the State of Art in Typology

L. Dezső

Kossuth University, Debrecen, Hungary

I am looking at typology from the point of view of its integration and possible application in the description and comparison of languages. Typology is a more or less autonomous branch of linguistics; here, I am going to concentrate on its specific features. My approach can be characterized as "functional", "structural" and "operational" from the point of view of methodology. My views are in constant change: the role of function is growing. According to them the universal functions of languages are expressed by formal means relevant to the analysis according to types. I think that the development of the functional aspect and the existence of an enormous empirical base of morpho-syntax will give new impetus to the study of the structural, especially paradigmatic component in a unified framework connecting universal and typological aspects. At present, we are far from this because the fundamental questions of typology are formulated and answered in different ways, and behind this there are differences in general or specific theories, in the empirical base and the substantive foundations underlying the views of typologists working according to their research programs often unconnected with those of others.

Typologie et types linguistiques

G. Lazard

Sorbonne, Paris

La typologie, comme la recherche des universaux, a pour but de contribuer à la connaissance fondamentale du langage en décelant dans les langues entre des faits apparemment hétérogènes des liaisons résultant de la nature même de l'activité linguistique.

Les groupements typologiques de langues (types) peuvent s'appuyer ou prétendre s'appuyer sur de vastes ensembles de faits intéressant la totalité de la structure linguistique (typologie "totale") ou sur des ensembles plus restreints (typologie "partielle") ou sur des faits isolés (typologie "élémentaire").

L'expérience montre qu'on ne trouve pas de "solidarités" entre les faits linguistiques, mais dans le meilleur des cas un plus ou moins haut degré de corrélation. D'autre part les typologies globales (ex.: langues isolantes/agglutinantes/flexionnelles/polysynthétiques, ou: accusatives/ergatives/"actives") sont peu convaincantes parce que fondées sur d'excessives simplifications. Plus fécondes sont les typologies partielles, surtout si elles considèrent les types comme des choix faits dans des cadres démontrés universaux (ex: projet de

Cologne). Les typologies élémentaires sont en elles-mêmes inaptes à servir les buts de la méthode, mais, aidées par l'intuition et la chance, elles peuvent être le point de départ de découvertes de corrélations.

Working Group 19:

Structural Transitions and Typological Diversities in Sino-Tibetan and Their Neighboring Languages of East and Southeast Asia

Organizer: Mantaro J. Hashimoto
Tokyo University of Foreign Studies

In the tradition of modern linguistics since the last century, structural developments of a group of languages are normally treated as the ones caused and brought about, if not triggered, by the internal forces of their linguistic systems, once the cognate relationship among these languages is proved by establishing the so-called sound laws observed between these languages. These sound laws are based exclusively on the sound correspondences between cognate words of these languages. The cognateness of languages here should then be applicable to the sound makeups of these words, and hence does not or should not necessarily imply the cognateness of other aspects of these linguistic structures.

In the development of East Asian languages, we witness some consistent interactions of heterogeneous linguistic systems, once we draw our attention to linguistic aspects other than the sound makeups of words. Since the structural changes in cognate languages are normally regarded as originated internally, and since involvements of non-cognate languages are often ignored or at most ascribed to the so-called substratum languages, descriptions of structural changes have mainly been restricted to "how's" of these developments, seldom to "why's."

Surrounded by variety of languages in the vast expanse of Eurasian continent and spoken mainly by agrarian populations, Sino-Tibetan languages have experienced relatively static developments since the very beginning of their history. Naturally these speakers have never been completely free from some kind of migrations, but the scale was at least not as drastic as those of Indo-European speakers—almost a millenium had elapsed before the Indic people, for instance, realized that their ancestors could have come from the same homeland as that of the Celtic or Germanic people. Thus Sino-Tibetan languages offer ideal data for examining such interactions of heterogeneous linguistic systems.

As a case study of such inquiries, at this working group meeting we would like to analyze structural developments and typological diversities found in Sino-Tibetan languages. Sino-Tibetan here will be interpreted in its wider sense, and participation and contribution by specialists of neighboring languages or language families will be most welcome.

"Tonal Fall Deletion in spoken Lhasa Tibetan"

Willa Dawson

Kuwait University

This paper discusses the loss of falling tone during compound formation in spoken Lhasa Tibetan. Compound formation involves the combining of two independent morphemes, each bearing semantic content, by affixing them to each other to form an independent lexical item with a new meaning related to the meanings of the two independent morphemes. *E.g.*: *phöö* 'Tiget' + *qEE* 'language, voice' becomes the compound, *phööqEE* 'spoken Tibetan (language)'. 26

During compound formation, and in other instances as well, generally the first syllable retains the tone closest to the tone of the morpheme in independent form. However, when a morpheme that takes a falling tone—high falling or low rising-falling—is affixed as a first syllable to another morpheme during compound formation, the fall of the tone is lost, and the tone is realized as either level or rising. *E.g.*: *čhaa* (52) 'hand' (Hon.) + *ñüü* 'money' → *čhääñüü* 'money' (Hon.); *phöö*^{3 2 1} 'Tibet' + *qEE*^{2 3} 'language' → *phööqEE*. That is, the high falling tone (52 or 51) is realized as high level (55) in this position, and the low rising-falling tone (231) is realized as low rising.

The discussion introduces several formalisms to account for this process. Contour tone is viewed as a concatenation of level tone segments. Thus a falling tone is represented as a sequence of High-Low. The loss of the fall is analyzed as a deletion of the Low element, thus allowing for the statement of this tone sandhi process as a unitary phenomenon.

"Why Did 蟲 *D'oiŋ Change from 'Animal' to 'Wug'?"

Michael Carr

Otago University of Commerce

Sometime between the Zhou and Han dynasties, the OC word 蟲 *d'ioŋ changed in meaning from 'animal' to 'wug' (insects, worms, spiders, reptiles, amphibians, etc.). In addition, it is defined in the *Erya* as 'legged animals', as opposed to 𧈧 *d'ioŋ 'legless animals'. Why did this unusual semantic specialization take place?

The lexicological history of 蟲 *d'ioŋ is summarized in respect to: (1) contextual meanings in pre-Han texts, (2) relations and confusions with 虫 *xjwər 'snake' and 蛭 *kwən 'insect', (3) OC associations with word families for 'numerous; herd', 'winding; extended', and insect names, (4) ST roots of *s-brong 'reptile; insect' and *dyuŋ 'insect', and (5) universals in the development of folk-zoological vocabulary.

Understanding the diachronic semantics of 獸 *d'iong will provide an important piece in the puzzle of the OC system for animal nomenclature. A speculative attempt is made to integrate the facts and proposals about this word, and additional information is requested.

Working Group 20:

Patterns of Language Impairment in Aphasia

Organizer: Sumiko Sasanuma
Yokohama National University

Aphasia represents a neuropsychological condition where the use of spoken or written language is disturbed due to brain damage, producing a diversity of impaired language behaviors.

Evidence has accumulated in recent years indicating that attempts at the application of linguistic concepts and methodology to the study of aphasia can provide insight not only into neuropsychological mechanisms underlying language pathologies but also into the organization of normal language behavior.

The five papers to be presented and discussed in this working group represent attempts of this kind. The topics of the papers span a wide variety of issues both theoretical and and practical and yet are closely interwoven with each other: the nature of impairment in agrammatism among Japanese as related to Mary Kean's recent proposition of agrammatism as a uniquely phonological impairment (Bisazza); the patterning of Japanese onomatopoeia in aphasia (Coulmas, Bisazza, and Sasanuma); linguistic competence vs. performance as related to language deficits in aphasia (Kamio); universal and script-specific features of reading impairment in aphasia (Sasanuma); and the production of discourse in aphasia (Ulatowska).

It is hoped that further insights will emerge from a productive interchange of ideas and information among the discussants, contributing to the development of further explanatory theories of language deficits due to brain damage.

Nouns, Verbs and Syntactic Deficits in Agrammatism among Japanese

John A. Bisazza
Meiji Gakuin University

Agrammatics and other types of aphasics (as well as normals under some conditions) sometimes find it easier to process (e.g., read aloud, repeat, etc.) nouns than verbs. The causal basis of this phenomenon and whether the "noun facilitation" symptoms shown by different types of aphasics are really the same—i.e., have the same causal basis—are at issue. Also at issue is the cause of agrammatism itself. Mary Kean has argued that agrammatism represents a uniquely phonological impairment. She claims that agrammatics retain a rela-

tively spared processing for phonological words versus nonphonological words (where a "phonological word" is any string #—# where—contains no #). Kean claims that agrammatism involves *no* syntactic impairment. If it can be shown that agrammatics have a noun facilitation effect even when phonological structure is held constant Kean's theory of agrammatism will have been in part disconfirmed. Noun-verb pairs in Japanese (e.g., *urami-uramu* "resentment-resent") make possible such a test since the members of such pairs do not differ in terms of phonological structure. Moreover, the results of such a test (for *or* against Kean's theory) will be useful for comparing noun facilitation across different types of aphasia and in normals, since evidence (based on the same noun-verb pairs) for a syntactic basis of noun facilitation in Japanese normals and one dyslexic aphasic is already available (Bisazza 1980, unpublished dissertation).

The Patterning of Japanese Onomatopoeia in Aphasia

Florian Coulmas¹⁾, John A. Bisazza²⁾ and Sumiko Sasanuma³⁾

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³⁾Yokohama National University

To what extent individual speakers of a language have mental associations to natural sounds for onomatopoeic expressions is largely unknown. An answer to this question would have relevance to the linguistic question of how arbitrary linguistic conventions and natural sound imitation interact in establishing onomatopoeic words as a distinct class of expressions. To shed some light on the mental associations that people may or may not have for onomatopoeic expressions, we are looking at whether brain-damaged persons exhibit any differences in processing onomatopoeic and non-onomatopoeic expressions. Our assumption is that the processing of expressions with more non-linguistic sensory associations will be relatively spared in cases of linguistic impairment due to brain-damage. To test this assumption, both a listening comprehension and a speech production experiment are being conducted with Japanese aphasics. The Japanese language is particularly suitable for this study since it has a rich stock of commonly used onomatopoeic expressions.

Linguistic Competence in Aphasia

Akio Kamio

The University of Tsukuba

Since generative grammar began to influence work in neurolinguistics, one major conceptual distinction of generative grammar has had a significant relevance to neurolinguistic research: Linguistic performance vs. linguistic com-

petence. The relevance of this distinction is apparent in the question: Whether it is part of linguistic competence or performance or both that is damaged in aphasia. In 1970, Weigl and Bierwisch claimed that it is linguistic performance and that the mechanism of competence is kept intact in aphasia. Along the lines suggested by their thesis, a number of research works have been published. Some of them have argued against the thesis. For example, Zurif and his associates have claimed that in agrammatism, linguistic competence is also destroyed to a noticeable degree. On the other hand, Kean recently presented a detailed analysis of agrammatism, arguing that it is in fact a phonological deficit and the basic knowledge of phonological structure and other parts of grammar are preserved in agrammatic patients. Also, the results of a close examination of Japanese aphasics, both of the fluent and the non-fluent types, performed by the present author revealed that the syntactic knowledge of Japanese is kept intact and that it is only by the breakdown of performance mechanisms that their speech appears to be syntactically defective. At this Working Group, I would like to further advance the thesis proposed by Weigl and Bierwisch, claiming that the knowledge of language is basically unaffected in aphasia. The results of an experiment dealing with a detailed syntactic structure of Japanese will be presented as evidence. Some consequences and implications of the thesis will also be discussed.

Patterns of Reading Impairment in Aphasia: Universal and Script-Specific Aspects

Sumiko Sasanuma

Yokohama National University

The paper is concerned with possible neurolinguistic explanations for the patterns of reading impairment observed in aphasic users of different types of orthographic symbols, i.e., alphabetic, syllabic, and logographic scripts. Word reading performance data obtained from Japanese aphasic patients, users of both syllabic and logographic scripts combined, as well as comparable data obtained from aphasic users of alphabetic scripts, will be analyzed and compared to each other in terms of the similarities and differences in the overall patterns of reading deficits, characteristics of paralexical errors, types of dissociations between phonological and semantic processings, and other relevant variables.

Production of Discourse in Aphasia

Hanna K. Ulatowska

University of Texas at Dallas

The paper focuses on two main concerns:

1) How the emergence of new analytical constructs from discourse grammar allows us to reveal which structures are preserved in discourse produced by aphasic subjects and 2) What possible explanations may be offered for the patterns of disruption of these structures. The constructs applied to the analysis of aphasia discourse are narrative superstructure, spectrum, profile and procedural superstructure. The population studied includes twenty-five aphasic subjects with mild and moderate degrees of impairment and twenty-five normal controls. The results show preservation of discourse structure in aphasics with selective reduction of information in discourse, as revealed through application of discourse constructs to the data.

Working Group 22:

Morphosyntax or Semiosyntax?

Organizer: Claude Hagège, Paris

Discussants: C. Hagège; Susumu Kuno, Harvard; C. P. Boguslavskaya, Saransk, USSR; Pr. Dasgupta, Pune, India; L. Deszö, Kossuth University, Debrecen; G. Lazard, Paris; C. Lehmann, Köln; A. Lemaréchal, Poitiers

The Working Group Session was opened by Prof. C. HAGEGE, who gave a general overview of the papers by participants and proposed an organization of the WG taking into account the time necessary for the audience to participate in the discussion, and for the discussants to comment on one another's talks.

Following the alphabetic order, Prof. G. P. BOGUSLAVSKAYA's paper, since the author was absent, was read by the organizer: this paper emphasizes meaning as the most important criterion for classifying morphemes; the author also insists that "there is no absolute borderline between the plane of expression and the plane of content".

In an interesting data-oriented paper, Prof. Pr. DASGUPTA presents many facts of Bangla which constitute an apparently chaotic set: first, he shows that it is not possible to predict, either on phonological or on morphological criteria, which one of three allomorphs of a suffix on Bangla nouns will appear; semantically, the situation is even more complicated, since these allomorphs, or the unique morpheme which underlies them, are used to express either the locative case, or the nominative (but only for the first and second persons singular of the personal pronoun), or the nominative, and furthermore with many restrictions in each of these cases; Prof. DASGUPTA further demonstrates that a comparable case of polysemy is the one observed in the use of the classifiers, which not only present, like the above-mentioned morphemes, a difficult case of allomorphy, but also have widely diverging uses. The organizer then proposes a semantic and diachronic interpretation of this apparent chaos: parameters such as the oppositions 1st & 2nd pers./3rd pers., animate/inanimate, pronoun/noun, and various kinds of semantic conditionings can help to account for the complicated situation which is observed in Bangla.

Prof. C. HAGEGE, the organizer, then presents his paper on "Syntax as a non-autonomous domain". He shows that three important branches of linguistic studies, i.e. language acquisition, diachronic syntax and creolistics, together amount to showing the intermediate status of syntax between morphology on one side and semantics on the other: 1) before 4-5 years. French

children tend to express semantic contents and syntactic relations by word order and intonation, and also, of course, natural environment: then there is meaning, but practically no morphology, to the extent that the latter is defined by the variation of forms as a means of expressing functions. 2) As for diachronic syntax, there are many facts showing the role of meaning in the genesis of morphological categories. Three sets of facts may be mentioned in this respect: a) *The Noun/Verb distinction*: it is preposterous to debate whether it was verbs or nouns which appeared first: believing that one or the other is original is completely circular, since it amounts to saying that the opposition already existed! It seems more reasonable to assume that the determiner, which represents the participant(s), was the first to acquire distinctive marks, because it had to be identified as opposed to the determined, i.e. nuclear, part of the sentence, representing the predicate, i.e. the elements that expresses the relation between the participants, a given text (oral as well as written) containing by necessity more participants than relations between these participants. Thus the Noun/Verb distinction cannot be properly accounted for short of having recourse to semantic considerations. b) *The genesis of spatial and dynamic relators and of "that" complementizers*: in many languages, especially West-African, Austronesian, North-American, relators historically derive from verbs or nouns: if they come from nouns, they will be prepositions in languages whose noun-phrases have the order of succession determined noun + determining noun, since these relators most often derive from names of body parts, the complement of the relator having originally designated the possessor of the body part: thus for example in Mbum (Central Cameroon),

"head"	has yielded	"on"
"foot"	"	"under"
"chest"	"	"in front of"
"back"	"	"behind"
"belly"	"	"within"
"waist"	"	"near"

In Mande and Kpelle (Liberia and Sierra Leone), the same thing happens, but with the reverse order: since the order of succession within the noun-phrase is opposite to the one of Mbum, the corresponding relators, with the same meanings, are postpositions. So much for spatial, i.e., non-dynamic, relators, which, as one can expect, come from nouns. As for dynamic relators, i.e. the ones designating the direct participants in the process (agent, patient, beneficiary, sociative, instrumental, etc.), they come, as can also be expected, from verbs, and here a comparable kind of situation can be observed: verbal series, in languages which present $V_1 + V_2$ series whose first element (i.e. V_1) is the main, i.e. the determined one (these languages often being the very same ones that also have the order determined noun + determining noun within the noun-phrase), will be the framework within which a relator will be produced in V_2 : e.g. in many Austronesian languages, one has "go" (V_1) + "reach" (V_2) =

"go to"; and, symmetrically, in various North-American Indian languages, "pass by" (V_1) + "arrive" (V_2) = "arrive by means of". Evidently, V_1 will be a postposition in this case, since this second type of language is generally SOV, and what becomes the complement of the postposition was originally the complement of the verb from which this postposition comes; in the former case, symmetrically, the V_2 ("reach" in the example given above) yields a preposition, since this kind of language is most often SVO, and the complement of the relator has originally been the complement of the verb from which this relator has been produced. Just as relators have come from verbs, in many languages, "that" complementizers also come from a verb "to say" or "to speak". c) The genesis of modals, auxiliaries and negation from semantically frozen verbs: this is a well-known phenomenon: in many languages, to mention only that, "finish" yields "past", "continue" or "be with" or "sit" etc. yield "progressive", "want" yields "future", etc. 3) As far as creolistics is concerned, we are lucky enough to be, here, the very witnesses of a process by which the cycle from semantics to semantics through syntax, then morphology, then resyntax takes place: the genesis of a morphology is evidenced by such examples as, for instance, Haitian Creole *ap̃s̃ate* "will sing", which is the last current step of the following evolution: Classical Latin *cantā-bo* (*bo* = future marker, 1st person singular) → Low Latin *cantare habeo* → Romance *cantāryo* (restituted) → Classical French *chanterai* → Haitian Pidgin *mo ap̃Re š̃ate* (I/ will/ sing) → Haitian Creole *m'ap̃s̃ate* "I will sing". To conclude, one can say that SHORT OF HAVING RECOURSE TO SEMANTIC AND TO DIACHRONIC CONSIDERATIONS (which often amounts to the same), MANY SYNTACTIC FACTS REMAIN UNEXPLAINED, to say nothing of "autonomous syntax".

Prof. S. KUNO's paper presents a very interesting study of Japanese honorific forms, in which he demonstrates that a generative of purely lexical account is untenable for the complex verbal expressions which are formed when one uses an honorific of the form O + VERB STEM + NI NARU. In fact, in at least three kinds of structures, i.e. causatives, passives, and verbal compounds, the insertion of NI NAR-I within the structure, yielding a sense which is different from the one which is obtained when this insertion does not take place, is by no means accountable by lexical rules, and must be handled transformationally.

The organizer then reads Prof. L. DEZSÖ's paper, since the latter was not able to come. In this short abstract, the author argues that "the time has come to reconsider the status of morphology in grammar, taking the experience of the past and the results of linguistic theory of the present into account".

Then, Prof. G. LAZARD reads an English translation of his paper, originally written in French: here, he shows that there is a continuum from morphology to syntax, and semantics should be kept apart. Prof. HAGÈGE objects that syntax without semantics is untenable.

Prof. C. LEHMANN, having his own Working Group at the same time, was not able to come. However, he sent an interesting paper, in which, under the title "On the grammaticalization of syntactic functions", he shows how one can

have a reduction of the syntagmatic variability of the constituents in given constructions. "This correlates with... a desemanticization which reduces the semantic function to a syntactic one, e.g. that of the indirect object. The relation may then become part of the meaning of the verb (government), and its segmental expression may become zero, the reduction in syntagmatic variability resulting in the fixed order of the object vis-a-vis the verb".

The last paper, by Prof. A. LEMARÉCHAL, very clearly shows, using good examples from Palauan an Indonesian language of Micronesia, that formal changes can also be sequential, and prosodic, not only segmental, and the amount of information which can be communicated by language can also concern the relation between the real world and the speaker, and not only the semantic content in itself. He also shows, on the basis of Palauan examples, that a polysemic account as one and the same morpheme, when we have a big amount of allomorphy or of diversity in the syntactical uses, is to be preferred over an homonymic account as diverse morphemes which, as if by mere chance, would happen to be formally identical.

The comments and questions by the audience show that for most people, syntax cannot remain as an autonomous domain; nor can it be excluded in favor of pure mechanical accounts or lexical listings, in other, complementary, cases. In other words, neither too much esteem for syntax, nor too little.

Syntax as a Non-autonomous Domain

Claude Hagège

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There are three connected fields whose study shows that syntax has an intermediate status between morphology and semantics: language acquisition, diachronic syntax and creoles. The morphological intricacies of flexional languages like, e.g., French, are not mastered by children until they are 4-5. Before that stage, semantic contents tend to be expressed by juxtaposition of lexemes, and order of succession, as well as natural environment, are essential. Thus, there is meaning, some syntax, and practically no morphology. As for diachronic syntax, its study shows how semantic features combined with functional specialization result in noun-verb distinction, how spatial relators are produced from names of body parts, or auxiliaries, negations, modals from semantically frozen main verbs, or "that" complementizers (and, often, other clause markers) from a verb "say". Finally, creole languages are excellent witnesses of the steps on the cycle from semantics to semantics via syntax → morphology → re-syntax: optional adverbs become obligatory modals (ex. Haitian *ap* (<Fr. *après*) → imperf. marker), dynamic verbs produce spatial relators, 3rd pers. plur. pron. yield affixed plur. markers on nouns, etc. While the pidginization of European languages has led them from a complex morphology to

a positional syntax, the creolization of pidgins and further evolution of creoles go the opposite way: what we have here is the genesis of a morphology.

It follows from the above that a completely autonomous study of syntax cannot defensibly be maintained unless we limit ourselves to a narrow synchronic approach. In dynamic terms (and languages are dynamic entities), there is (on very long periods, of course) a constant flow from semantics to syntax and from syntax to morphology, followed by a new cycle from morphology to syntax and so on. But the relationship of synchronic syntax itself to morphology on one side and semantics on the other side is so tight that even synchronically, there is no such thing as autonomous syntax. A proper account of incorporated nominals, for example, in Eskimo, Iroquoian, Algonquian, Paleo-Siberian languages, etc., and even in English (cf. *man-made*, *clam-digging*), although this phenomenon seems to belong to the chapter of word-formation as a part of morphology, is not possible short of having recourse to syntactic considerations. There is, on the other hand, a remarkable parallelism between syntactic and semantic complexity, as is evidenced, for example, by the fact that in most languages, subordinate clauses, just as they are semantically dependent, are syntactically submitted to much more constraints than independent clauses. Furthermore, the syntactical notions of noun, verb, etc. (categories) and of subject, predicate, complement (functions) are, in themselves, a participation to meaning: it is not semantically irrelevant for a verb to be a verb, for a noun to be a noun, for an element functioning as subject to function as subject, etc. Finally, the relationship between functional notions and semantic roles is often regular across languages: the subject (a morphosyntactic notion) corresponds in many cases to an agent or patient (semantic-referential notions) and the predicate corresponds to a process (for more details on all the above, cf. Hagege 1982). We can thus say that synchronically as well as diachronically, syntax is not an autonomous domain, if only because the rules which we call syntactic all contribute to one and the same task, which is the task of language: to convey meaning.

Reference

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The Honorific Forms of Compound Verbals in Japanese

Susumu Kuno

Harvard University

Japanese forms the subject honorific form of a verb by adding *O-...NI NAR-* to the verb: *kak-* 'write': *O-kak-i NI NAR-*. Since adverbial particles can appear inside such an honorific form, as in *O-kak-i NI SAE NAR-* 'to even write', the honorific form formation cannot be handled by word formation in

the lexicon, but must be dealt with as a syntactic process.

Japanese forms the causative form of a verb by adding *-sase-* to the right of the stem form of the verb: *ibar-* 'boss around' + *-sase-* = *ibar-ase-* 'make boss around'. It has been proposed by some generative grammarians that this process should be dealt with as a word formation process in the lexicon. However, this position is untenable because the verbal stem can appear in the honorific form under certain conditions. For example, *ibar-ase-te ok-u* 'boss around—Cause—to leave; let (someone) boss around': *O-ibar-i NI NAR-(s)ase-te o-ok-i su-ru*. When such an honorific form can be used with deference for who is completely predictable if we take a transformational approach to the generation of causatives, but would be hard to account for if one takes a lexical word-formation approach.

Japanese allows more than one honorific form for "compound verbals" such as *kak-i-hazime-* 'write-begin; begin to write': *O-kak-i NI NAR-i hazime-* and *O-kak-i-hazime NI NAR-*. These alternative forms have been regarded as stylistic variations. I show that they are different in semantics, and that this difference can be captured automatically only in a transformational approach to compound-verbal formation.

On Structure and Semantics of Grammatical Categories

G. P. Boguslavskaya

The Mordovian State University

Linguistic theory must reflect systemic structural organization of language. In the structural organization of grammatical categories the principle of unification of grammatical classes and units constituting this category is most essential. The basis for such a unification is the most general meaning (for instance, the meaning of time) uniting the meanings of the components of the category. Semantic opposition builds up the relation subordinated to the principle mentioned above.

The linguistic fact that must find its reflection in linguistic description is the one registered in the given language as "the result of selection" which yields the system of certain grammatical forms (a series of forms) used for the expression of meanings connected with the given category (paradigm or a complex of paradigms).

Grammatical selectivity does not always follow formal logical rules such as, for instance, the subdivision into semes.

The schemes and principles of systemic structural organization inherent in the grammatical category often ascribed to the language may be violated by the fact that the expression of potential grammatical meanings is distributed among morphology, syntax, lexical system, context and different combined means of expression including "hidden grammar."

The unification of the forms constituting a grammatical category is the most essential constant quality of systemic structural organization of grammatical categories. This unification is at the top of hierarchy. The relations of opposition between the components of the category are merely variable individual qualities of a lower rank.

One should also bear in mind that there is no absolute borderline between the plane of expression and the plane of content. The intermediate link is grammar which is a constituent part of the plane of content and which serves the purpose of building up the remaining part of the plane of content, e.g. the word (he) *drives* denotes the action because it possesses the grammatical categories of person, number, tense, aspect, voice and mood. Grammar serves the plane of expression by grouping phonemes into the basic combinations, i.e. morphs.

Alain Lemaréchal

Poitiers

Many a structural homology is observed within a given language as well as between different languages. In order to explain such morphological peculiarities (which are often stable within a language, if not widespread all over the world) it is necessary to search beyond the mere morphological and syntactical description for a more refined description of the distribution of meaning.

Semantics is the only way firstly for preserving the logic of the system of each language and even for identifying a certain kind of information communicated by the languages (an abstract, logical one such as categorization of the elements of the real and of the relations between these elements), secondly for founding a typology capable of explaining and not only stating the homologies.

G. Lazard

Sorbonne, Paris

Traditionnellement la "morphologie" étudie les variations des mots, fixées dans la langue et limitées en nombre; la "syntaxe" étudie leurs combinaisons, soumises à des règles, mais en nombre infini. Cette distinction n'a qu'une valeur relative et pratique: des mots nouveaux peuvent être formés indéfiniment; inversement beaucoup de combinaisons syntaxiques sont stéréotypées. Mais il est vrai que les combinaisons d'unités dans les séquences courtes ("morphologie") ont beaucoup plus de latitude que celles qui ont lieu dans les séquences longues ("syntaxe"). "Morphologie" et "syntaxe" sont les pôles du continuum que forme l'étude de la chaîne dans la double perspective paradigmatique et syntagmatique et qui est à la base de la construction des systèmes.

La sémantique concerne l'ensemble de ce continuum: elle cherche à pré-

ciser le signifié de chaque unité et à montrer comment se constitue la signification des combinaisons d'unités. Les "fonctionnels" ont un signifié, si ténu soit-il: lexèmes et morphèmes forment aussi un continuum, du plus riche de sens au plus vide. L'ordre des termes, s'il est significatif, a aussi par définition un signifié.

Donc morphosyntaxe et non sémiosyntaxe.

Morphology and Syntax in Typology

L. Dezső

Kossuth University, Debrecen, Hungary

In the 19th century and in the first decades of the 20th century morphology was in the centre of grammatical studies. Syntax was approached through morphology, especially in typology. Later syntax became the major field of linguistic investigation, not only in grammatical studies, especially in grammatical theory, but also in typology. The study of syntax via morphology as well as the approach to morphology from the point of view of syntax comes up against serious difficulties. The time has come to reconsider the *status* of morphology in grammar, taking the experience of the past and the results of linguistic theory of the present into account. In general that is important for typology and in particular for the typological characterization of languages with "strong" morphology. The theoretical and typological investigation can be based on the Typological Bank of the Kossuth University stored in the computer on the basis of the huge Soviet project "Encyclopedia of the Languages of the World."

MAB

Working Group 23:

Origin of Japanese

Organizer: Kazuo Mabuchi
Tokyo

The working group on the origin of Japanese met on August 30, 1982 at Room A of the Nippon Toshi Center and discussed the problem with Kazuo Mabuchi as organizer, three invited specialists (Lee Ki-Moon, Seoul; Shichiro Murayama, Tokyo; Susumu Ohno, Tokyo) and five volunteer speakers (Kim Kong-chil, Cheju; László Szabó, New Brunswick; Pon Kothandaraman, Madras; Takao Kawamoto, Jōetsu; Kōji Atarashiya, Sapporo). Out of the invited specialists, Prof. Roy Andrew Miller (University of Washington) declined to attend the Congress, Prof. Karl H. Menges (University of Vienna) and Prof. Nikolaj A. Syromjatnikov (Moskva) sent us summaries but could not attend the Congress.

Main points of discussion

1. Prof. Murayama asked Prof. Szabó a couple of questions about Uralic and Altaic specialists;
2. Prof. Yoshimachi gave some comments on the examples given in Prof. Kim's handout;
3. Prof. Ohno gave supplementary explanation about the CVC stem-structure of Japanese and Tamil, and answered the questions raised by Professors Murayama, Murasaki and Krishnamurti;
4. Prof. Murayama proposed taking ancient accent into consideration, on which Professors Ohno, Hirayama and Komatsu gave comments;
5. Prof. Matsumoto raised a question about the word order, the grammatical gender and the number in Tamil and their relation with Japanese. Prof. Ohno replied that the answers were given in his published books. Prof. Susumu Shiba said that comparison should be made separately between Dravidian, Austronesian and Mongolian. Prof. Kawamoto wished to recognize the CVCV structure as the stem of Japanese.

Summary

A number of theories on the origin of Japanese have been proposed during the discussion. The organizer asked each discussant which age (chronology) of Japanese he had in mind when he gave his theory.

Prof. Lee Ki-Moon: It is not yet time to define the chronology;

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- Prof. Kim and Syromjatnikov (northern theory): ca. 2000 years ago;
Prof. Kawamoto (southern theory): 5000-3000 B.C.;
Prof. Kothandaraman: prior to 500 B.C.;
Prof. Ohno: 400-300 B.C. (prior to that date there might have been a language with the CVCV structure);
Prof. Szabó: Japanese belongs to Altaic, Hungarian belongs to Uralic, the two families having been separated from each other about 6000 B.C. Possibilities of relationship between Indo-European and Uralic/Altaic cannot be denied either.

The organizer does not necessarily agree with all discussants on all points, but each theory has some possibility in view of the fact that the theories proposed are not necessarily exclusive against one another despite methodological varieties. The theories proposed can be synthesized into the following graph.

(1) Korean theory; (2) Dravidian theory; (3) Austronesian theory; (4) age in which relationship between Altaic and Uralic can be assumed; (5) Indo-European (?).

	6000 B.C.	3000 B.C.	500 B.C.	0 A.D.	→ 日本語
(5) ビ イ ア ン ド 語 ・ ユ ア ロ	(4) れがルア るあ両ル 時ッ語タ 代た系イ との・ 思関ウ わ係ラ	(3) ア オ 語 ー ス ト ロ ネ シ	(2) ド ラ ゲ イ ダ 語 説	(1) コ リ ア ン 語 説	

Problems in the Comparative Studies on Japanese

Ki-Moon Lee
Seoul National University

Recent comparative studies on Japanese have made it clear that an overall examination of their methodology is required. The most fundamental problem is related to the basic nature of comparative work carried on *only two languages*. When relying on only two reflexes, it is extremely difficult to judge the validity of proposed comparisons.

Let me cite just one example. The Korean word *pal* 'foot' has been compared with Old Japanese *fagi* 'shin', Ryukyu *fagi* 'foot', Hachijojima dialect *hagi* 'foot' on the one hand, and with Old Japanese *asi* 'foot, leg' on the other. Since these are based on the comparison of only two languages, it is impossible to decide which of the above pairs are real cognates. Fortunately, however, we have Tungus words such as Goldi *palgan* 'foot', Evenki *halgan*, *algan* id. which have also been compared with Korean *pal*. These Tungus forms clearly show

that the *fagi: pal* pair is much more probable than the *asi: pal* pair. If we take into consideration the fact that Old Japanese *f*- goes back to **p*- and that original medial consonant clusters were simplified in Japanese, we can safely assume that the earlier Japanese form was something like **palgi* and this corresponds to the above-mentioned Korean and Tungus words.

One of the most serious problems in the comparative studies of only two languages comes when one attempts to establish rules of sound correspondences, because it is almost impossible to distinguish between actual and accidental ones. What have been called "rules of sound correspondences" in Japanese comparative work are, in fact, so powerful that they can be used to justify lexical comparison of any two languages.

Finally, I would like to touch upon the problem of borrowing. It is difficult to distinguish between cognates and loan-words, especially with languages such as Korean and Japanese which have been in contact from the earliest period of their history. Although there is no general criteria to distinguish between cognates and loan-words in early periods, we can postulate some conditions in which lexical borrowings took place. It is rather safely assumed that Old Japanese *kī* and *sasi* 'castle', *kisi* and *kimi* 'lord' are loan-words from the Paekche and Silla languages.

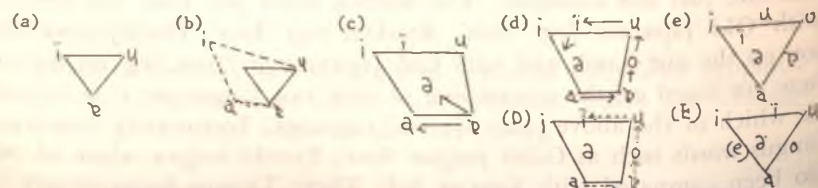
The Korean and Japanese Languages About 20 Centuries Ago

Kim Kong-chil

Cheju National University

About 20 centuries ago there lived the races of Buyo, Koguryo, East, Okjo, Ye, Hann and Wa in an extensive area covering South Manchuria, the Korean peninsula, and Northern Kyushu and these races belonged to the same cultural and linguistic cycle on the basis of different clan society system.

By means of the word examples picked up from regional and place names in the Korean peninsular and the Japanese area, as the result of reconstructing the vowel inventory and system of both languages, it is found out that the common system reconstruction in both systems was originally /*e-u-i*/ in the Korean peninsular. And other vowels were formed and adjusted with in these basic vowels as a base, so that in the course of time the vowel system of two



languages had become different. The developing courses can be illustrated as follows. Only where the Wa races lived transitional systems as seen in (b) and (c) were transplanted and, as a result, a retroactive systems as seen in (d) was developed.

The two primitive languages of Korea and Japan had the same vowel system by nature, so the correspondence of the system is very simple. Consequently the transition from the "primitive" to the "ancient" had brought about complex correspondences of vowels between the two languages. The reason of this movement seems to be its own specific reasons in the vowel specializing and developing process. But on observing the course of the formation of these vowels, we can discover the casual relationship in the course of moving and regularly limited correspondence among vowels of two languages coming into being.

For example;

Jap.	Kor.
{ asa 'morning'	{ *aze (azək-ac'em) 'morning'
{ osō 'evening, late'	{ *əzə (əzətkuī) 'yesterday'
{ kata 'shape'	{ kat 'thing'
{ kōtō 'thing (abstract)'	{ kət 'thing'
{ padara 'frost falls'	{ ptaradida 'drop'
{ pōdōrō 'snow falls'	{ ptərəzida 'fall'
{ karu 'cut, crop'	{ karda 'crop, plow'
{ kōru 'cut, take in'	{ katta 'gather in'
{ taru 'enough'	{ ta'eda 'complete'
{ tōru 'take off'	{ tərda 'take off'
{ tahusu 'bring down'	{ tahēda 'exhaust'
{ tōhōsu 'pass through'	{ tēhēda 'do it still more'
{ sita (*sata) 'lower part'	{ sta 'earth, land'
{ sōtō 'back, outside'	{ thə 'area'
{ parahu 'suppress'	{ parada 'depend upon'
{ porōbu 'destroy'	{ pərīda 'abandon'
{ pakaru 'devise'	{ pargida 'make a trial'
{ pokōru 'progressive'	{ pergida 'dispose'
{ tadasi 'rightful'	{ tatta 'arrange'
{ *tōdōsi (tōtōnōhu) 'arrange'	{ tattēdi 'in order'
{ *karama/kōrōmō 'cloth'	
{ asa/ōsupi 'cloth'	

As mentioned above, the fact that the correspondence in alternate of developing vowel a-ō exist in Old Korean and Japanese languages is a proof that Korean and Japanese languages are cognate languages.

Historical-Comparative Observation of the Japanese Language

Shichiro Murayama

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Starting from the Old Japanese (OJ) we try to reconstruct the pre-OJ vowel system and to examine whether the so-called "Onsetsu-ketsugō no hōsoku", i.e. OJ law of vowel combination, has anything to do with the Altaic vowel harmony. On the other hand we examine whether the reconstructed pre-OJ vowel system shows any correspondence to that of the Austronesian. The remnants of the morpho-phonological prenasalization of Austronesian type in OJ will be investigated.

Further we treat the problem of OJ verbs, their conjugation and their morphological connection with the Altaic, especially Tungusic languages.

Our analysis will show the polygenetic origins of the Japanese language, which makes the problem of the genesis of the Japanese language extremely difficult to solve.

Comparison of Japanese and Tamil

Susumu Ohno

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I will give a brief account of my study on the comparison of Japanese and Tamil words. The result of my study was published in book form *Sound Correspondences between Tamil and Japanese*, Gakushuin 1980.

Tamil roots take the form of CVC- or CVCC-. Accordingly, I consider Japanese words only up to the third phoneme. I have collected correspondences in the first, second and third phonemes. Grammatically, Tamil and Japanese belong to the agglutinating type. Structurally, both languages show considerable similarity.

Further consideration will be given to Tamil intervocalic stops. Since Caldwell there have been two theories on Tamil intervocalic stops, whether they are voiceless or voiced. In recent times, studies by Bh. Krishnamurti and K. Zvelebil have shown the theory of voiced is more likely. If Japanese is added as a new material for the study of Tamil, the theory of voiceless will have to be examined anew. I will also touch upon the problem of voicing of intervocalic *ka-* and *ta-* series in Northeast Japanese dialects. I will distribute the list of Tamil-Japanese correspondences.

The Phonetic Laws due during the transfer from Proto-Japanese to pre-Japanese

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"Proto-Japanese" is dialects of Altaic tribes, invading Tukusi 2000 years ago before their contacts and phonetic changes under the influence of the substratum Malayo-Polynesian dialects. Proto-Japanese had clusters of consonants as other Altaic languages also. Pre-Japanese under the influence of substratum lost clusters of consonants according to the following laws:

1. If the first consonant had been voiceless sound or a sonant (l, r, n, m, ŋ), it disappeared without any result: *kalta(s) > kata.
2. If the first consonant of a cluster has been a voiced sound, the following voiceless one became a voiced consonant: Mongol *tobti- 'a button'—AJ todi- 'to close'; Mongol kabsa- 'to blow'—AJ kaza-/kazē 'wind'. Thus, Jap. o:gi < apuki < Evenk. arpu-ki 'fan', 'a fin' (-ki is a suffix, denoting that the doer does this action constantly. cf. usagi ~ Evenk. tuksaki 'a hare': tuksa- 'to run').

The common law is that the last consonant of a syllable was dropped: Evenk. inamu-hta > AJ *namu-hta > nami-hta (under the influence of nami 'wave') > namida (-hta is a suffix of names of little, but a multitude of things).

Japanese and Altaic

Karl H. Menges

Orientalisches Institut, Wien

Apart from agnostic and solipsistic tendencies to consider Japanese as having no genetic relationships with other languages, there is a well-founded general agreement that the languages most closely related to and cognate with Japanese are the Altaic languages. Recent research on this problem achieved considerable expansion and consolidation of the theory linking Japanese with the Altaic languages. Of these, Turkic is the group, with the possible exception of Korean, possessing the oldest written documents, from the VIIIth century A.D. on; Mongolian has written documents from the earlier XIIIth century on, and Tungus (or Manžu-Tungus) is documented latest, with some fragmentary Zürčen texts of the XIV-XVth centuries and the vast Manžu literature from the XVIIth century on, while the other Tungus languages, none of them possessing a literature, have become known from the middle of the XIXth century on, most research having been done in this century. It is just

the latter, the non-literary Tungus languages, especially those of the Northern branch, Ewenki and Lamut, which are the most conservative and archaic Altaic languages, evolutionally being much older than either Turkic or Mongol, and thus they hold a key position in the comparative-historical research on Altaic.

For historical and ethnological reasons the assumption is cogent that the Kiba Minzoku, the "Horse-Riding Peoples", were early Altaians, no Uralians or Arians; history knows of no Uralic nomads of the steppes of Central Asia in the largest sense.

Japanese-Hungarian Relationship: an Old and New Problem

László Szabó

University of New Brunswick

Mutual sympathy between the Japanese and the Hungarians has been a contributing factor to the investigation of the possible relationship between these two languages. Hungarian is an Uralic language, Japanese is supposed to belong to the Altaic language family. Are the Uralic and the Altaic languages related to each other? This question was already being discussed by linguists in the nineteenth century, for instance by Schott, Castrén and Munkácsi. The idea of the Uralo-Altaic relationship has never been generally accepted in the linguistic literature, but the problem has emerged again, to mention only a few of the great twentieth century scholars: Wiedemann, Ramstedt and Fokos-Fuchs. My list is far from being complete, even if I add the name of a linguist from our time, Kazár.

In the paper, instead of giving a complete survey of Japanese-Uralic comparative studies, I present just a few ideas of the above listed scholars, adding to them my own comments, being a speaker of several Uralic languages and a student of Japanese.

Even though Turkish loanwords in Hungarian as well similar borrowings in other Uralic languages might cause difficulties, it is worth continuing the pioneers' work on Uralo-Altaic Linguistics.

Linguistic Affinity between Dravidian and Japanese

Dr. Pon Kothandaraman

University of Madras

The purpose of this paper is to explicate the linguistic affinity between Dravidian and Japanese. Dr. Susumu Ohno of Tokyo has already collected about five hundred words in Tamil which are phonetically and semantically similar to the corresponding words in Japanese. After making a brief survey

of the works done by Dr. S. Ohno, Dr. Susumu Shiba, Dr. Akira Fujiwara, Dr. Minoru Goh, Dr. Muneo Tokunaga and others, I would like to place certain relevant facts which reveal the fact that the Japanese language is genetically related to the Dravidian family.

The paper makes an attempt to explain why the reconstructed Proto-Dravidian forms are not directly considered for the comparative study and why in most of the cases the Tamil forms are chosen for comparison with the Japanese forms.

The phonological, syllabic, morphological and syntactic aspects of both languages are carefully considered from the view of an historical and comparative study. The phonological aspect is given more attention since the author does not have sufficient data for a deeper study of the other aspects.

Two Sets of Correspondence Relating Japanese to Austronesian

Takao Kawamoto

Joetsu University of Education

Considerable efforts have been made so far to uncover the definite sound laws between Japanese and Austronesian without success. A recent comparative work on various aspects of the two languages reveals that we have been treating the two different groups of words without distinction. The one group is what Bruce Biggs calls a direct inheritance composed of basic words (eye, mouth, fire, &c.) and words of Oceanic origin, whereas the other, an indirect inheritance, consists of culture words (ricefield, blacksmithing, weaver's sword, &c.) and Hesperonesian words. I have assigned over a hundred words to each of the groups, and found the following two sets of correspondences:

PA/H	p/b-	-p/b-	gp/gb	t/T	C	gt/gT	d/D-	-d/D-	l/r	gd/gD	R-	-R-
PO	p	p	mp	t	t	nt	d	d	l/r	nd	R	R
I	f	f/w	m	t	t/s	s	t	r/d	r	t	s	r
II	f/w	f/b	m/b	t/d	t	s/z	t	t/r/d	t/r	z	ø	r
m	n/N/ñ	s/c/z-	-s/c/z-	j	gs/gc/gz	k/g	gk/gg	g	w	q-	q-	y S H
m	n/ñ	s	s	s	ns	k	gk	g	w	q	q	y ø ø
m	n	t/s	s/t	?	t	k/g	g	k/g	w	ø	f	? ø ø
m	n	s/t	s/t	s	t	k	g	k/g	f	k	k	y ø k

Japanese y also reflects *gt, *d, *l, *r, *R, *s, *z and *ñ under conditions which cannot be stated at present.

[Abbreviations and Symbols: PA/H=Proto Austronesian or -Hesperonesian; PO=Proto-Oceanic; g=ŋ; Ø=nothing; ?=no examples].

The Etymological and Comparative Studies on Japanese Words and Indo-European Roots

Kōji Atarashiya
Hokkaidō University

We know from the previous studies that Indo-European /*l-/ and /*sk-/ regularly correspond to Japanese /k-/. The present studies on comparisons between the homophonic Jap. words *туру* and IE roots demonstrate that their sounds correspond. The results described below leave no doubt that IE /*ste-/ corresponds to Jap. /tu-/.

The Jap. word *туру* meaning "vine" is cognate with the English *stalk* which is derivative from the IE *stel- meaning "to put, stand". The Jap. *туру* meaning "to cramp" is cognate with the Eng. *stark* from the IE *ster- meaning "stiff". The Jap. *туру* meaning "bird's name, crane" and Eng. *stork* are cognate with the Eng. *stark*. The etymology of *туру* and *stork* is "the stiff movements of the bird". The Jap. *туру* meaning "strain, race" is cognate with the Eng. *strain* from the IE *ster- meaning "to spread".

And the comparative words described below are similarly obtained. (1) IE *stebh- (post, stem), Eng. *staff* (stick, rod), Jap. *tuwe* from *tupe (staff) after MILLER, Alt. *čibiq* from *čibik (staff) after POPPE, (2) IE *steg- (pole, stick), Eng. *attack* (offense), Jap. *tuku* (to attack), Drav. *tukk- (to push, stab) after FUJIWARA, Eng. *attach* (to touch), Jap. *tuku* (to attach), (3) IE *steg- (to cover), Gk *stegein* (to cover), Jap. *tuku* (unknown meaning, after the TAKE-TORI MONOGATARI, but probably cover, thatch, roof),

On the basis of these data, it seems reasonable to assume that Japanese belongs to the Indo-European family of languages.

Principal Works on the "Origin of Japanese" by discussants

- 李 基文 (Lee Ki-Moon): 韓国語の歴史 1962 (藤本訳) 1975 大修館
村山七郎 (Shichiro Murayama): 日本語の起源 (大林と共著) 1973 弘文堂
日本語の誕生 1979 筑摩書房
大野 晋 (Susumu Ohno): 日本語の成立 (日本語の世界 I) 1980 中央公論社
日本語とタミル語 1981 新潮社
金 公七 (Kim Kong-chil): 原始韓日両言語の母音体系について 1982 国語国文学
川本 崇雄 (Takao Kawamoto): 日本語の源流 (講談社現代新書) 1980 講談社
Roy A. Miller: The Japanese Language 1967 (小黑訳) The University of Chicago
1972 三省堂
Origins of the Japanese Language 1980 University of Washington
Press; (村山・山本・下内訳) 1982 筑摩書房

Working Group 24:

Modality

Organizer: Frank R. Palmer
University of Reading

Only three of the discussants were present—Walter A. Cook, Thomas R. Hofmann and Co Vet. Disappointingly as a result, the discussion was largely concerned with modality in English. Although Vet's paper was concerned with French, many of the points he raised were equally valid for English.

Cook provided a very general framework within which to place modality. Taking Fillmore's distinction of Modality and Proposition he further divided the first of these into Tense, Aspect and Modality; these are universals, with different manifestations in different languages. This was challenged by David-sen-Nielsen who pointed out that modality is further removed from the proposition than either tense or aspect and, more generally, it was challenged by the whole of Vet's paper which showed the close relation between tense and modality in French.

Hofmann provided a neat framework of modality in terms of (1) Possible, Impossible and Necessary and (2) Epistemic, Deontic and Capacity (= my Dynamic). But it was generally felt that this was too narrow. When does probability come in and how does he deal with *will* (Yamada)? Is there a clear distinction between Deontic and Capacity (Miyahara).

Vet's paper showed the close relationship between the temporal and modal use of sense tense forms in French and an attempt to explain this in terms of situational semantics.

My own view, which motivated the suggestion for the working group is as follows:

- 1) Language does not consist entirely of 'categorical assertions', though these are given privileged status in a great deal of linguistic and philosophical discussion. There are languages, e.g. Hidatsa (G.A. Matthews *Hidatsa syntax*) in which it is not possible produce a non-modalised utterance. (I am indebted to John Lyons for these ideas.) (It was argued that all languages have modality indicated at all times (Reesink) even if one form is unmarked (Ferris). But this seems to me to miss the point—the absence of formal marking of unmodalised forms is highly significant—it distinguished them from all the others (unlike Hidatsa).)
- 2) The issue is one of the grammaticalisation of notional/semantic categories. How do languages grammaticalise modality? The greatest difficulty here is in defining modality—it is far more difficult to establish then, say, tense

(with reference to time) or number (with reference to enumeration). But many languages have formal systems that seem likely candidates, e.g. the English modals which are formally very different from the 'semi-modals' such as *be going to*, *have to*, *be able to*. But should we distinguish mood and modality (Davidsen-Nielsen)?

3) There are problems.

- (a) Is tense separable from modality (Vet's paper or the functions of *will*)?
- (b) Is it coincidental that the same forms are often used for epistemic, deontic and dynamic modality? Is possibility/necessity more basic than these distinctions? (This is, perhaps, what Hofmann is recognising when he says that the modals are unambiguous—a very strange claim in view of their very different functions.) Or is this just a feature of English and some familiar languages?

On the whole the exercise was a disappointing one. We learnt little about the typology of modality. There was, however, one interesting proposal from the audience—that of Tsunoda who suggested that there is a close connection between modality and case with examples from a number of languages and in English *He shot him/He shot at him*. But this is, surely, 'casting the net' too widely.

Mood and Context—A Brief Outline

G. P. Boguslavsaya
Saransk

The current opinion runs that finite verb phrases have Mood, which indicates the speaker's attitude to the predication. The speaker may wish to represent an action as a real fact or as a command/a request/or as something that does not exist in reality.

The relevance of context to the significance of the verb-forms merits close attention. Some verb-forms exhibit a specific modification of their primary function and develop special connotation and modal force due to the context.

The Russian linguistic school singles out a special part of speech named *modal words*. Finite verb-phrases combined with modal words acquire subtle shades of meaning.

A complete study of the likenesses and differences of means of expressing modality between any two languages alone is a most difficult thing to do. No real attempts have yet been made to tackle the problem and give a picture, accurate and exhaustive in detail. Contrastive grammar is still in its beginnings. There is no lack of promising directions along these lines of linguistic research. It is certain that the differences could be largely classified and reduced to rules.

We endeavour to achieve it by analyzing the peculiarities of the use of different means of expressing modality in two languages belonging to two different linguistic groups, such as English and Russian.

Modality in Generative Semantics

Walter A. Cook, S. J.
Georgetown University

The logical structure which represents the literal meaning of a sentence in generative semantics includes three layers of structure which differ in function. The performative layer specifies the type of speech act, the modality layer specifies the mode of existence of the predication, and the propositional layer specifies the objective content (Langacker, 1975). Logical operators, including negatives, quantifiers and connectives, belong to the propositional layer.

The modality or T-A-M layer includes tense, aspect and mood. Tense relates the time of the predication to the moment of speaking as present/past/future. Aspect indicates different ways of viewing the internal temporal consistency of the predication as perfective/imperfective, habitual/continuous, progressive/nonprogressive (Comrie, 1976). Mood indicates factual, theoretical or hypothetical meaning (Leech, 1971). Modal verbs are a subset of mood indicating possibility/necessity, or permission/obligation.

In a universal theory of modality tense, aspect and mood are present in every predication but manifested in different ways. This manifestation may be inherent-radical, in the verb root, derivational-thematic, in verbal affixation, or adverbial-compositional, in the surrounding context (Friedrich, 1974). The modality question, then, is not whether tense, aspect and mood occur but rather how they are manifested in any given language.

Semantic Analysis of Modal(ity)s

Th. R. Hofmann
Toyama University, Japan

Taking the notion of modality of modal logics & expressed in most of the English modals, we find contrasts of this nature in probably all natural languages & in most modal logics, as exemplified by the English epistemic modals *may* : *can't* : *must*, or the deontic modals *can* : *shouldn't* : *should*. There is moreover a strong tendency for natural languages to have lexical forms expressing all 3 of these terms & no others (though a 4th term is not so uncommon in alethic modality). This dimension of "logical modality" accounts for some but not all of the contrasts in natural language forms identified as modal expressions.

Of the remaining contrasts, we can extract an other dimension found in most of the languages examined. It is orthogonal to the above, but often also has 3 terms: epistemic : deontic : capacity, though French for example does not distinguish epistemic & capacity in its modal forms, & English appears to have made the distinction between epistemic & deontic only recently.

English alone among the languages examined makes a further systematic distinction, between "strong" (e.g. *must*) & "weak" (e.g. *should*) expressing a tolerance or not for exceptions or counter-examples. Further, though non-systematic, distinctions are found in both Chinese & Japanese, though on very different bases.

Additional points are 2: A new type of data, a technique of controlled paraphrase, generated the basic analysis of 2 dimensions ("logical" vs other), & is promising for application to other areas of semantic analysis. Also, & the proof of these results, is the discovery that, except for special idiomatic usages, & *will* & *shall* not included here, the English modals are unambiguous. Each has a single & general, but unitary, meaning.

Shifting of Modality in Spanish Discourse

Beatriz R. Lavandera

CIAFIC-CONICET (Argentina)

I attempt to establish the set of meanings that are distinguished in the Spanish system of EPISTEMIC MODALITY (Palmer 1979 and 1981) and the way in which such meanings are employed as purposive strategies that are shifted within discourse. Corresponding to the English predominance of the MODAL VERBS in the expression of MODALITY, the inflection of verbs as indicative/subjunctive is shown to be the main grammatical means in Spanish, where MODAL VERBS almost always occur as QUASI-MODALS. The system of TENSE is also used to 'modalize' and, as Halliday (1970) has pointed out for English, MODALITY in Spanish is not located either at any special point in the clause, but runs throughout it.

I will show that even the whole utterance or the 'information unit' is an insufficient context to study contrast and variation of modality and that the unit of analysis must be extended at least to a semantic discourse unit, externally bound and internally structured, which usually runs over several sentences.

Furthermore, I will show how the paradigmatic analysis must be completed on the syntagmatic axis, that is, adding to the data on alternations, contrast and substitutions, the investigation of shiftings.

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Illocutionary force and Modality

Daniel R. Vanderveken

Université du Québec

I will explore some consequences about modality of the following hypothesis about language: it is part of the meaning of each sentence that its literal and serious utterance in certain context constitutes a performance of illocutionary acts. All elementary sentences are of the form $f(\bar{p})$ where f is an illocutionary force indicating device such as the mood of the main verb and \bar{p} is a clause expressing a propositional content. The illocutionary force indicating device and the clause of a sentence may be syntactically complex.

Consequently there are two kinds of modalities in natural language, one peculiar to force and the other peculiar to propositional content. Modifiers of illocutionary force markers such as for example the adverbs "please" and "frankly" in the sentences "Please come!", "Frankly he won" express operations on illocutionary force such as a restriction of the mode of achievement and an increase of the degree of strength of the illocutionary point. But temporal adverbs or propositional attitudes express operations on propositions.

I will give a list of the various operations on illocutionary force and show that often to the modality on propositional content that is expressed at the level of the literal speech act corresponds a modality on force at the level of implicature or indirect speech act.

From Tense to Modality

Co Vet

University of Groningen, Netherlands

In this paper the idea will be defended that there is a narrow relationship between the temporal and the modal use of some tense forms in French (e.g. the epistemic and deontic modality expressed by the Simple future). Within a somewhat modified Reichenbachian framework, it will be shown the semantic structures of these temporal and modal meanings are to a large extent the same and that the modal uses can be explained by a transposition of a future (or past) reference point to the point of speech. This is possible only in some well-defined pragmatic circumstances. An explanation will be put forward for the fact that the auxiliary *devoir* ('must') can express the same modal values as the Simple future. It will be argued, finally, that possible world semantics

cannot account for all the modal uses of tense forms and that situational semantics seems to offer a better instrument for their analysis.

References

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WG 24: Modality

Working Group 25:

The Present State of Proto-Indo-European Studies

Edgar C. Polomé

University of Texas at Austin (USA)

1284

The workshop was convened on Thursday, September 2, at 5 p.m. The participants presenting papers for discussion were: Professor Giuliano Bonfante (Rome, Italy), Professor Thomas V. Gamkrelidze (Tbilisi, USSR), Professor Edgar C. Polomé (Austin, USA), Professor Haiim Rosén (Jerusalem, Israel), Professor Wolfgang Meid (Innsbruck, Austria), and Professor Bernfried Schlerath (Berlin, Germany). Professor Antonio Tovar (Madrid, Spain) took also part in the panel discussion, but did not give a paper. Papers were received from Professor Françoise Bader (Paris, France), Professor T. L. Markey (Michigan, USA), Professor Alexandr S. Melničuk (Kiev, USSR), and Professor Robert Schmitt-Brandt (Heidelberg, Germany), but their authors were unfortunately not able to attend the Congress. All the papers presented and received will be edited by Edgar C. Polomé and published by the *Journal of Indo-European Studies* in its 1984 volume.

The workshop bore witness to the vitality of Indo-European studies and the renewed interest in this field of historico-comparative linguistics, which was also evidenced in other sections of the Congress. It indicated the new avenues of research that have been opened by recent work in phonology, in syntax, in areal studies, in lexicography, and other fields, and illustrated the leadership that Indo-European studies continue to give in methodology and innovative thought in diachronic linguistics.

Dr. Haiim B. Rosén, in his description of *Some more noteworthy features of 'primitive' Indo-European syntax*, attempted to define the oldest types of sentence patterning in IE languages on the basis of the 'markers of sentencehood': (1) 'finiteness'; (2) concord; (3) adverbiality. Illustrating this situation with examples from an array of IE languages (Hittite, Sanskrit, Greek, Latin, Germanic, etc.), he indicated that, with the non-required presence of a verb, one could distinguish three primordial *verbal* sentence-patterns and three *non-verbal* ones, but that preference was soon given to a single pattern with more than one 'marker' (nl. concord and 'finiteness', i.e. a person-marked form). The grammatically redundant 'verbal' sentence thus created became the 'normal' pattern in most of IE, entailing the use of the self-sufficient 'markers of sentencehood', such as participles without auxiliaries, merely in subordinate clauses or forms.

The challenging paper of Dr. Rosén, which raised a number of important

theoretical issues, was the object of a lively discussion in which various questions touching upon PIE syntax, such as the problem of the *ergative* or the origin of PIE verbal flexion, were brought into the debate.

In his presentation on *The relative position of the IE languages*, Dr. Giuliano Bonfante proposed a pattern of original distribution of the Indo-European dialects that would give Albanian and Anatolian a central position and consider Baltic, Tocharian, and Indo-Aryan as 'bulges' outside the 'oval of the peripheral dialects (Germanic, Slavic, Iranian, Celtic, Italic, etc.). He backed up his views by providing a number of isoglosses characterizing the eccentric position of Tocharian, Indo-Aryan, and Baltic.

In the discussion which followed, a number of objections were presented to the separation of Baltic from Slavic, but Dr. Bonfante was especially taken to task on the validity of his Tocharian isoglosses by Dr. W. Winter (Kiel, Germany). Dr. Bonfante defended his position by reassessing his data and adducing some additional material.

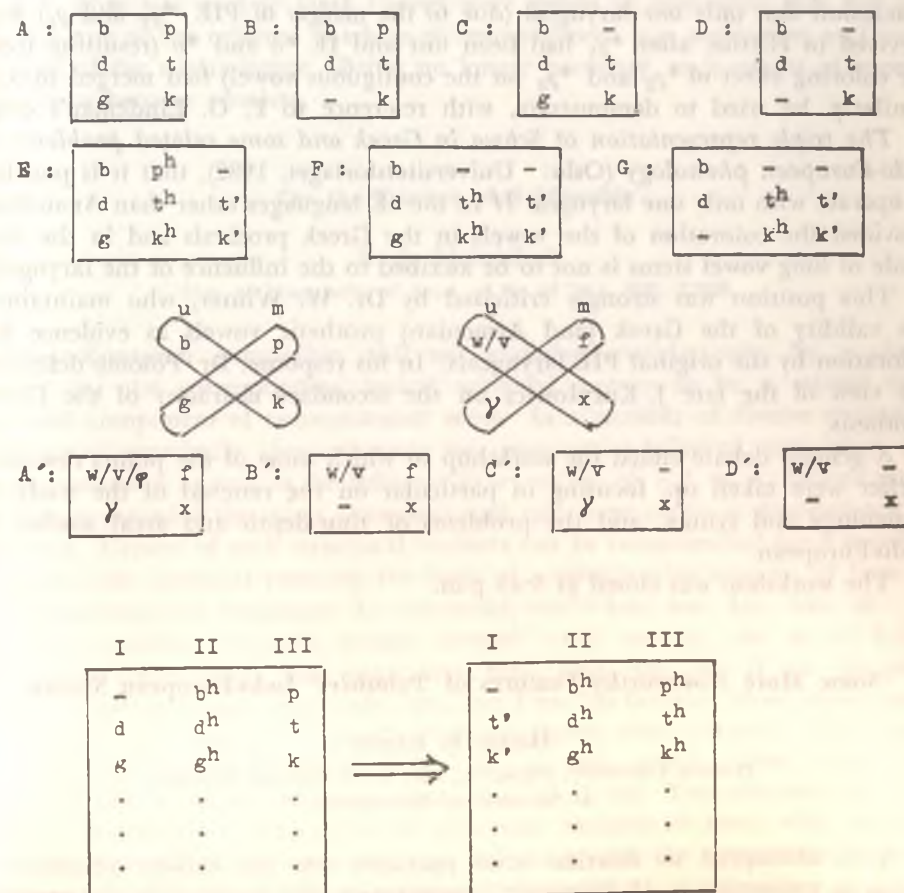
In his paper on *The Indo-European Glottalic Theory (A new paradigm in IE comparative linguistics)*, Dr. T. V. Gamkrelidze gave an updated report on the new views on the IE consonantal system that V. V. Ivanov and he himself have advocated since 1972, comparing their position with that of J. Hopper in his contribution to the volume *The Indo-Europeans. Third and Fourth Millennia B.C.* (ed. E. Polomé; Ann Arbor: Karoma, 1982). With strength of argument, Dr. Gamkrelidze indicated the typological and phonological reasons that make a system of glottalized, voiced and voiceless stops (the latter two with aspirated and non-aspirated variants) more plausible than the currently reconstructed IE system. (See p. 1361)

In the ensuing debate several discussants, a.o. Dr. W. Winter (Kiel, Germany) Dr. J. Knobloch (Cologne, Germany), Dr. I. Dyen (Yale, USA), Dr. W. Meid (Innsbruck, Austria), carefully weighed the pros and cons of the proposed system and argued in favor or against it. In his response, Dr. Gamkrelidze underlined the fact that some of the major criteria for a scientific theory are its plausibility and its ability to solve a number of problems in a simple and elegant way, which is precisely what the new approach does with the vexing questions of the quasi-absence of PIE *b or the co-occurrence restrictions of some consonants in PIE roots.

Dr. Wolfgang Meid, analyzing the *Problems in the temporal and spatial patterning of Indo-European*, restated the argument he had made in the volume *Flexion and Wortbildung* (ed. Helmut Rix; Wiesbaden: L. Reichert, 1975, pp. 204-219) about our reconstructed forms of PIE and their lack of spatial and temporal perspective. As any linguistic system that conforms to a norm sanctioned by common usage also shows archaisms as well as innovative tendencies, Dr. Meid assumes that the same state of affairs applies at any stage of the parent language and accounts for phasal differences in the system of the individual languages: the latter reflect the state of IE at the time they broke away—hence, e.g., the phonetic reflexes of laryngeals in Hittite. This

enables him to point out three stages in the development of IE: (1) early IE; (2) middle IE; (3) late IE (with an east-west contrast, illustrated, e.g. by the Greek-Aryan innovations).

Dr. Bernfried Schlerath, responding to Dr. Meid's presentation, sharply criticized the postulates on which the 'new view of Indo-European' is based and insisted on the necessity of defining what we reconstruct, i.e. an abstract structure reflecting a historical reality without direct connections to any definite time or place. We cannot assume social and dialectal differences of the parent language for every stage of its development: all we have are partial correspondences—the isoglosses, but they do not compel us to doubt the existence of a hardly differentiated parent language. What we reconstruct are elements, classes of elements (lexemes, affixes, etc.) and the rules governing them. There is no reason to deny to Indo-European the rich diversity of forms that charac-



terizes Indic, Iranian or Greek: at the time of the first documents in Germanic, Celtic or Slavic, the spoken languages of India and Iran had lost far more of the IE morphological system than these languages, and it is therefore plausible that the North-European linguistic system was originally more complex than the oldest evidence shows, as some isolated forms tend to confirm.

A very lively debate followed these two presentations, which opposed the defenders of a strictly traditional approach to the *Grundsprache* and the proponents of a concept of the parent language taking into account the accomplishments of sociolinguistics, dialect geography and other recent developments in linguistic studies.

The last paper was a survey of *Recent developments in the laryngeal theory* by D. Edgar C. Polomé, who briefly reviewed the literature of the last decade and tried to assess the validity of the hypothesis that only one IE laryngeal has to be postulated. An examination of the Anatolian evidence led him to the conclusion that only one laryngeal (due to the merger of PIE $*_{22}$ and $*_{23}$) had survived in Hittite, after $*_{21}$ had been lost and IE $*a$ and $*o$ (resulting from the coloring effect of $*_{22}$ and $*_{23}$ on the contiguous vowel) had merged into a . Similarly, he tried to demonstrate, with reference to F. O. Lindeman's essay on *The triple representation of Schwa in Greek and some related problems of Indo-European phonology* (Oslo: Universitetsforlaget, 1982), that it is possible to operate with only one laryngeal H in the IE languages other than Anatolian, provided the coloration of the vowels in the Greek prothesis and in the zero grade of long vowel stems is not to be ascribed to the influence of the laryngeal.

This position was strongly criticized by Dr. W. Winter, who maintained the validity of the Greek (and Armenian) prothetic vowels as evidence for coloration by the original PIE laryngeals. In his response, Dr. Polomé defended the view of the late J. Kurylowicz on the secondary character of the Greek prothesis.

A general debate ended the workshop in which some of the points discussed earlier were taken up, focusing in particular on the renewal of the study of phonology and syntax, and the problems of time-depth and areal studies in Indo-European.

The workshop was closed at 8:45 p.m.

Some More Noteworthy Features of 'Primitive' Indo-European Syntax

Haiim B. Rosén

Hebrew University, Jerusalem, and Israel National Academy
of Sciences and Humanities

It is attempted to describe what probably was the earliest situation of syntactic patterning in IE languages: constitution of a sentence by the presence of any one of the following 'markers of sentencehood': 1. 'finiteness' (person-

marking, not necessarily verbal), 2. gender-number concord, 3. an adverbial (circumstantial) expression. The presence of a verb not being required, one thus recognizes three primordial verbal sentence-patterns and three non-verbal ones. This situation of a multiplicity of sentence patterns was subsequently superseded by one of strong preference for a single pattern, in which more than one 'marker' was present (in particular concord and finiteness). The IE 'verbal' sentence thus created was, from the point of view of earlier syntax, grammatically redundant (*Taurus mugit* as originally appositional); but since the 'verbal' finite-concord sentence 'took over' as the principal and 'normal' pattern in most of IE, earlier self-sufficient 'markers of sentencehood' could now indicate only the existence of a subordinate sentence (clause), e.g., participles without the auxiliaries later developed function in most of IE as subordinate forms only. We must, however, inquire as to what constituted a 'clause-marker' at a period at which the later clause-markers were still 'markers of sentencehood': what was used to mark clauses were means of expression that did not involve any of the original 'markers of sentencehood', e.g. absolutes and compounds of the *wedawit*-type. Being no longer necessary, such means of expression have become obsolete.

On the Essence of S Movable

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Indo-European s movable, still not explained satisfactorily, by a closer regard for the corresponding lexical material, proves to be a second consonantal component of two-consonant roots. As a member of diverse structural variants of genetically identical roots the consonant s followed most often the other consonant as well as the infixes $i(>i, e)$, $u(>u, o)$, r , l , m , n , $H(>a)$; more seldom it preceded the other consonant and still more rarely it dropped out of the root. Groups of such structural variants can be reconstructed for a number of genetically identical roots on the basis of a considerable amount of facts of the Indo-European languages: ks - "to strike, cut"— kes -, kos -, krs -, kas -, sk -, k -; ts - "erect, standing, exposed, spread, covered"— tes -, tos -, trs -, $tās$ -, st -, t -; I ms - "to throw"— mes -, mos -, mis -, mus -, mrs -, mls -, mjs -, sm -, m -; II ms - "to pull, touch"— mes -, mos -, mis -, mus -, mjs -, sm -, m -; I ps - "to breathe, blow, strew, sow"— pes -, prs -, pas -, sp -, p -; II ps - "to trample down, step, advance"— pes -, pos -, pis -, pas -, sp -, p -; ues - "good"— su -, v -; I ls - "to walk, search, gather"— les -, $lōs$ -, las -, sl -, l -; II ls - "sticky, mucus, to slip"— $leis$ -, sl -, l - etc. The existence of many parallel formations as derivatives of structural variants of roots with initial s and of those without s has caused, in old languages, the appearance of new analogous formations mainly of emotional character.

The Indo-European "Glottalic Theory". A New Paradigm in the Indo-European Comparative Linguistics

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A typological reinterpretation of the classical system of the Proto-Indo-European consonantism as proposed by us and independently by the American linguist Paul Hopper necessitates a cardinal revision of the views on the provenience and the prehistoric developments of the consonantism of the individual Indo-European dialects. Such a reinterpretation of the Proto-Indo-European system of consonantism and a postulation of a series of ejectives or glottalized consonants for Proto-Indo-European, with all the structural consequences for the individual Indo-European dialects involved, was styled the "Indo-European glottalic theory" and compared in some aspects to the Indo-European "Laryngeal theory".

Positing for PIE a typologically natural consonant system with three series of stops specified as: (i) glottalized (corresponding to the voiced stops in the classical system), (ii) voiced (with aspirated and unaspirated allophones), and (iii) voiceless (with aspirated and unaspirated allophones) yields a picture of the origin and developments of phonemic systems of the historical Indo-European dialects, which appears to be basically different from that assumed traditionally in the classical theory.

The Indo-European Glottalic theory presents as to its methodology and inferences involved a new approach to the problem of linguistic reconstruction in general and the Indo-European comparative studies in particular. It is viewed by some scholars as a new paradigm in the Indo-European comparative linguistics.

Phonological aspect of Indo-European grammatical reconstruction

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Indo-European phonemes are determined mainly by morphophonemic alternations (in ablaut rows, laryngeal series etc.) each phoneme being a point of intersection of morphic relations and not a natural phonological phenomenon. Features of stops might be defined in terms of their possible positions in the canonical root shape. To a small subset belong those phonemes (among them laryngeals) the fall of which was connected with restructuring of verbal paradigms that are used as parts of inflexions the order of phonic elements being especially significant for the vocative (older casus indefinitus) and imperative forms ending in a vowel. The rules of syntactical accentology define the

obligatory unstressed features of the enclitic/proclitic particles occupying the second place in a sentence and of other syntactical word classes having no separate accent (as the finite verb in a final position) the low-tone character of unstressed syllables being able to give the acoustic explanation of the voicing according to Verner's law. The grammatical evolution is accompanied and partly influenced by the recording of phonemic elements (laryngeals in old nominal collective plurals, verbal inflexions etc.) into accentual (tonal) characteristics. The Hittite types of barytonic and movable accentual paradigms help to reconstruct Proto-Indo-European tonal schemes.

Aspect Categories in PIE?

Robert Schmitt-Brandt

Heidelberg

Following a comprehensive definition of the terms "aspect" and "PIE", arguments are put forward for the development of aspect categories within the IE language community during the period of the spread of the IE tribes. Thus some tribes were never affected by the innovation (e.g. the Proto-Anatolians), while others adopted it through the mediation of intervening tribes which had only partly adopted their verbal system to the new distributional classes, so that some of them probably never possessed a fully developed aspect system (e.g. the Proto-Germanic tribes).

In an attempt at internal reconstruction, the aspect system in question is traced back to a stage where the vanishing differentiation of verb forms with regard to the manner of action (such as sk-suffixes, n-infixes, reduplication, etc.) resulted in an opposition between verb forms characterized by these affixes and those not so characterized. Thus in the former the distinction of how the action progressed gradually gave way to a simple expression of progression *per se*, while the latter began to state the action as such.

This development reached its highest state in Greek and in the older stages of Indo-Iranian, and, probably influenced by these languages, in the pre-historic stage of Slavic as well. There an entirely new type of aspect crossed the older one. This new system had its origin in the opposition of simple and composed verbs, the latter consisting of adverbial prefixes plus the verb stem. Here the specific semantic value of the prefixes vanished to the point where their common characteristic of being "terminative" prevailed (cp. drink : drink up). Under the influence of the older system, this new opposition developed into a mixed aspect-Aktionsart system. Although this particular development in Slavic is irrelevant for PIE reconstruction, it is instructive for Indo-Europeanists because of its obvious parallels in development with PIE.

Les présents hittites en -hi

Françoise Bader

Ecole Pratique des Hautes Etudes. Paris

On dissocie trois objets d'études:

- 1) la flexion, à singulier d'origine moyenne, pluriel (3e p.) d'origine active, comme au parfait grec, aux présents du tokharien B (sauf "aller"), aux présents dits thématiques du type grec;
- 2) la structure thématique, plus ou moins étendue (type *arhi*; *sak-hi*/*sagg-a-hhi*; *wast-a-hhi*);
- 3) le vocalisme, essentiellement: zéro (*uhhi*), -o- (*wastahhi*), c'est-à-dire provenant d'une intégration temporelle, respectivement du moyen (atemporel) à degré zéro initial, et du parfait, le vocalisme -e- marque radicale des présents les plus anciens, à flexion moyenne, comme les formes à degré zéro et le parfait (type *kešmuat*), reste cantonné en hittite dans son foyer d'origine (type *ney-a-hha*, à flexion moyenne), n'étant qu'exceptionnellement étendue à l'actif (*nehhi*). Dans les présents actifs du hittite, le vocalisme -e- n'apparaît qu'au singulier, alternant à l'origine avec le zéro du pluriel, et ces présents, ont, au contraire des précédents, la flexion des formes actives (atemporelles) ainsi intégrées au temps (type *kuenzi/kunanzi*).

The Semiotics of Enumeration: the Case of Indo-European

T. L. Markey

The University of Michigan

As a grammatical category and cognitive prime, number is shown to be derivationally related to, yet significantly different from, the category and cognitive prime of deixis. Ultimately, there is never a plurality of *hic et nunc*, though deictically derived pronouns are frequently used to signal number. This is universally so in stratified creoles where suffixation of the 3rd pers. pl. anaphoric pron. denotes nominal plurality, e.g. Papiamentu *buki sg.* 'book': *buki-nan pl.* 'books', where *nan* = 'they'. Assuming that the creole strategy, which clearly relates deixis and number, is not merely typologically distinctive, but also historically primitive, the relationship between number and deixis, as well as the primary locus (nominal vs. verbal) of deictically derived number marking is then traced in Indo-European. It is thereby shown that, as an associative construction, deictically derived nominal number marking (e.g. in stratified creoles) is typologically complementary to deictically derived verbal number marking (e.g. -t, -n, -r) in Indo-European.

Problems in the temporal and spatial patterning of Indo-European

Wolfgang Meid

University of Innsbruck

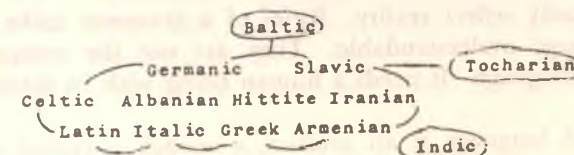
This paper explores the possibilities of establishing spatial and temporal distinctions within "Indo-European" in so far as this can be reconstructed by comparative linguistics. Broad chronological distinctions between Early, Middle and Late IE are proposed, the last being geographically divided into an eastern and a western type. Late IE was already dialectally differentiated, and certain segments had already broken away, such as Hittite. The archaism of Hittite, and its crucial role in the reconstruction of IE, are discussed. Hittite, as a representative of Early or Middle IE, is not to be subsumed under the Greco-Aryan type which usually serves as the basis for the reconstruction of the proto-language. Nor are the innovations of the western IE languages innovations vis-à-vis the Greco-Aryan type. The basis of these innovations, both of West and East Late IE, are the preceding Middle and Early IE phases. In the course of the argument the Present-Aorist system, the complex of the Medio-Passive, Perfect and Hittite hi-conjugation, and the problem of the 3-Gender-System are reviewed.

Indo-European Areal Distribution

Giuliano Bonfante

Accademia dei Lincei

According to the Wellentheorie of Johannes Schmidt, every Indo-European language is connected with two neighbors, the whole forming a circle. I follow on the whole this theory, with the exception of Baltic, Tocharian and Indic, which emerge from the circle as three half-isolated bulges:



The position of Albanian (= Thracian) and Anatolian has to be discussed. They are probably in the centre of the circle.

Recent developments in the laryngeal theory

Edgar C. Polomé

University of Texas at Austin

In the seventies, a considerable number of studies have been devoted to the laryngeal theory, surveying the whole problem (Keiler, Lindeman, Jonsson) or concentrating on their reflexes in a definite language such as Greek (Beekes, Peters). Moreover, it has been integrated into new approaches to the study of Indo-European phonology (Schmitt-Brandt) and made to play an important role in the explanation of Indo-European morphology (e.g., in the reconstruction of pronominal inflection G. Schmidt). The influence of laryngeals on Balto-Slavic accentuations has received renewed attention (Kortlandt), and a number of papers, articles and dissertations have been devoted to reexamining critically and reanalyzing thoroughly some of the processes ascribed to the presence of laryngeals. On the other hand, the staunch opposition to the laryngeal theory has not disarmed: some of its major tenets such as the interpretation of the Greek prothetic vowels as reflexes of laryngeals have been utterly rejected (Wyatt), and the dispute over the number of laryngeals to be posited in the protolanguage has been lingering on (e.g., Szemerényi).

This paper endeavors to assess the present state of the theory, especially in the light of recent work on Anatolian. It also tries to establish which of the assumed reflexes of laryngeals in other Indo-European languages are reasonably acceptable and why, while rejecting those for which the degree of plausibility remains much more disputable (e.g., the Old High German *-r*-preterites).

The reality of linguistic reconstruction

Bernfried Schlerath

Berlin, BRD

1. All grammars only *reflect* reality. Rules of a grammar make some mechanisms of language understandable. They are not the computer program which governs language. It needs a human being with its intentions, experiences, feelings.
2. A reconstructed language is an artefact, a model, a system of hypotheses based on different operations of different evidence.
3. In the reconstruction of the phonology of a proto-language 3 successive steps are necessary: *a*: experimental stage of etymology, *b*: finding out the rules of phonological correspondences *c*: phonetic/phonological interpretation.—This is also a quantitative problem (completeness).—*b*: = *firm base*; *c*: phonetic probability/likelihood of the system (typological evidence).—Although

the changing of the sounds of the IE→individual languages took place in different times, we put the sounds in the abstract line of the oldest change. So Hitt. *h* and the hypothesis of laryngeals the whole IE phonology moves back in the past.

4. The reconstruction of the morphemes follows the reconstruction of the phonemes.
5. Reconstruction has no dimension of time or space.
6. The assumption of IE dialects or varieties contradicts logically the reconstruction, which leads always to a unit.
7. W. Meid overestimates the possibilities of detecting the history of IE. Mere speculations!!

Working Group 26:

Nonverbal Communication for Linguists

Organizer: Walburga von Raffler-Engel
Vanderbilt University, U.S.A.

The Working Group n. 26 *Nonverbal Communication for Linguists* is designed to introduce linguists to the idea that the analysis of language is far more complex than is generally assumed. More precisely, the complexity is not to be sought primarily in derivations from intricacies of deep structures but in the intricate relationship of verbal language with the other components involved in human communication. The Workshop is based on the theory that verbal language is only one of the components of human communication and must be analyzed accordingly. The tacit assumption that verbal language conveys the full message has led to theories of linguistics that make language too powerful by attributing to it that which pertains to the other four components, paralanguage, kinesics, the ethnography of speaking, and pragmatics. Language, paralanguage and kinesics are overtly present in face-to-face interaction and can be separately identified in most instances although there are many overlapping areas, particularly between language and paralanguage. There are also mutual-conditioning features, such as obtain between posture and phonation. Features of pragmatics, like the presence of external factors, such as objects that are not fully described because visible to all the parties concerned may explain certain verbal constructions. Transcripts from sales transactions without mention of the physical artifacts that were present in the real life situation are unintelligible because of constant references to "the same color", "a bit larger", and like utterances (F. Weisberg and W. von Raffler-Engel, *The function of repetition in the sales transaction*, paper presented at the International Conference on Social Psychology and Language, University of Bristol, England, 1979, ERIC ED 203 405). The ethnography of speaking is among the shared/unshared presuppositions which have been given attention during the last twenty years but are not yet adequately incorporated into most linguistic theories.

It is essential that linguists become familiar with the components of the unit of communication other than verbal language. This Working Group concentrates on the nonverbal component which appears to be the one least studied by linguists. This lack of concern is probably due to the tendency of linguists to reduce language to writing before it is analyzed. Historically, philologists and epigraphists studied only written records. Eventually, anthropological linguists took notes or transcribed from tape with little attention to filled pauses

and generally none to empty pauses. Linguists failed to recognize that writing is not just a different style of language but based on a different modality. A corpus that originates in spoken form cannot be reduced to its verbal component only. In material that is meant to be in writing from the start, the additional components of live communication have to be skillfully incorporated into the text. When only verbal language is available the wording has to be more extensive than when the complete array of components can be perceived by the receiver. The reader must be empowered to reconstruct the natural form of communication. Research by A.F. von Allmen and W. von Raffler-Engel (*The relationship of verbal cues to the image of the speaker*, paper to be presented at the Xth World Congress of Sociology Mexico City, 1982, Main Session, Sub-session IV) has shown that people associate verbal and non-verbal features at above chance level.

Before the start of the working session the participants will observe a minute of silence in memory of HELMUT ESAU who was tragically killed in a car accident six months ago. He was scheduled to present his research on *The Watergate tapes in relation to nonverbal behavior* and had been very much looking forward to being with us. We all miss his warm personality and competent scholarship. His research is of paramount importance to the theme of this Workshop and it is to be hoped that his colleagues at Texas A & M University will complete and publish his unfinished manuscript.

The Workshop will open with a paper by the convenor, WALBURGA VON RAFFLER-ENGEL (Vanderbilt University, U.S.A.) on *The inadequacy of linguistic theories which exclude the nonverbal component from analysis*. The paper is critical of current linguistic theories because they analyze verbal language as if the latter were solely responsible for conveying meaning in human communication. It suggests that much that is posited as part of the deep level is actually present in the surface when one includes the other components that comprise the act of communication. The persistence of inadequate linguistic theories is thought to be possible because these theories are not tested against necessary applications, as they would be in fields such as medicine or chemistry.

To document the impact of nonverbal behavior in particular on the production and the perception of a message, the paper proceeds to report the results from a series of research projects conducted by the author dealing with the relationship of the verbal component to the nonverbal component in the act of communication. The description of the research projects is divided into two parts, first mentioning those that deal with production and then those that deal with perception.

In the message production series, the first report centers on multiparty communication. It is possible to address a new conversation partner without breaking contact with a previous interlocutor by maintaining one part of the body in the old direction while positioning another part of the body towards the new interlocutor, e.g. turning only one's head in the new direction. This is tantamount with saying "Please be patient; I'll be back with you in a minute." Other pro-

jects deal with the parallelism of the acquisition of language and kinesics in children and with observations that the most talkative children are the ones that also are kinesically the most active.

Both speech production and perception are involved in metakinesics. When talking about other people's gestures, the person who will notice most gestures will also tend to be the one that most frequently enacts that same gesture when describing it. Research dealing exclusively with perception indicates that variation in nonverbal behavior while the verbal component remains invariant will cause significant differences in the evaluation of the speaker. One further experiment compared the reaction to inadequate verbal expressions combined with perfect nonverbal behavior against the opposite, perfect verbal expressions combined with inadequate nonverbal behavior. Michael Gold, a student in our Research Program, demonstrated through a test involving 86 raters that in 1982 the reaction to the Kennedy-Nixon U.S. presidential campaign debate of 1960 was similar to the one of the time. When heard over the radio, the two candidates tie but when viewed on television, the evaluation is 81% in favor of Kennedy.

FERNANDO POYATOS (University of New Brunswick, Canada) gives support to the theme of the Workshop by showing that in what has traditionally been termed "fluency", verbal language is only one aspect. In his paper on *Linguistic Fluency vs. Verbal-Nonverbal Fluency*, and *Redundancy vs. Complementarity*, the author aims *Towards a Revision of Concepts*. The author clarifies the concept of "redundancy". Often what is termed "redundant" is in reality complementary, adding information about the intention of the speaker or serving to define his cultural or social background. "Fluency" entails a series of conscious and unconscious behavioral choices which mainly depend on an individual's "fluency quotient". According to the author, age is the main factor determining a person's "fluency quotient". To achieve "interactive fluency" the speaker must understand the self-regulatory functions of intra-personal as well as inter-personal behaviors. Of these, spoken language is only one component.

Two papers examine literary works and show how a writer utilizes the description of nonverbal behaviors to depict his characters. FORREST D. BURT (Texas A & M University, U.S.A.) in *The Basis of Somerset Maugham's Sensitivity to Nonverbal Communication* shows that, particularly in his autobiographical works, Maugham's novels are heavily dependent upon the narrator's observations of the nonverbal communications of the other characters. According to the author, it was Maugham's unhappy childhood and his speech impediment which fostered his strong sense of observation and his liking for dramatic techniques.

KATHERINE FELL (Texas A & M University, U.S.A.) examines *The Unspoken Language of Edward Driffield*, a character in Maugham's second autobiographical novel, "Cakes and Ale". Facial expression—smiles, winks, looks, and gazes, unmask Driffield who usually hides behind his clothes and clean-

shaven face. Only the narrator detects Driffield's facial mimicry and eye gaze while all the other characters see only the clean-shaven mask behind which he hides. Thus the nonverbal cues serve both to mask and unmask Driffield. Fell shows how Maugham's willful use of nonverbal communication portrays the difference between what is meant and what is merely said verbally. Language alone is insufficient to convey the full meaning.

The next two papers deal with nonverbal behavior as an integral part of the acquisition of communicative competence.

BATES HOFFER and JEANNE CALLIHAN (Trinity University, U.S.A.) present the *Development of Verbal/Nonverbal Interactional Features* based on their study of 15 children who were videotaped over a period of 9 months to research their acquisition of interactional behavior with non-family adults and children. The study showed the correlations and non-correlations between the development of verbal and nonverbal competence. In particular, the authors researched the correlation of illustrator gestures and higher syntactic features as a model for the rest of their research. Their presentation is accompanied by statistical charts, graphs, and still photos.

CECILIA K. YODER (South Oklahoma City Junior College, U.S.A.) focuses on the presyntactic period. In *Learning to Play the Parts: Language Development as Orchestration* she presents the results of a comparison of the communicative behavior of a boy at age 16 months, 3 weeks and at age 17 months, 3 weeks.

For this author, language development is conceptualized as a process of increasing differentiation and integration of verbal and nonverbal features. The child exploits intonation, voice quality, gaze, gesture, and body movement for regulating interaction with his mother during the transition from idiosyncratic to conventionalized language.

Yoder views the young child as a highly competent organism, with the means for regulating initiation and engagement as well as disengagement and defense. After the month long interval the little boy had progressed further and the researcher observed significantly increased ability to shift gaze while vocalizing both toward and away from his interactant.

ELIZABETH KEIL and ANN HEINS (Vanderbilt University, U.S.A.) researched the other end of human development. From videotapes of natural settings and questionnaires the authors concluded that the question *Do Older People Need More Personal Space?* has definitely to be answered in the negative. It is young and middle aged persons who shun the elderly while old people themselves sit close to each other and enjoy the proximity of younger people. The needs for personal space seem to diminish with old age after they have held a plateau during middle age. The authors will show their tapes and statistical charts comparing the seating arrangements of young adults, middle-aged people, and the elderly.

To forcefully conclude the session's theme, NICHOLAS CLARK (Vanderbilt University, U.S.A.) will show the videotape and comment on a commercial

of Sharp Copier produced by David Kaye of Rosenfeld, Sirowitz and Lawson, Inc. *Nonverbal and Artifactual Communication in a Commercial* explains how a thirty second commercial can be extremely effective when it supplements what is said verbally with paralanguage, kinesics, and the artifacts of clothing. One actor is shown in three roles growing in wealth and stature as he adds features which increase the power of his copier to satisfy his extended business needs. Colored slides made from the tape will show the progression from a young working class business man in a blue shirt extolling the merits of his copier to a successful manager in a grey suit with eye glasses giving advice to the public that sharp people buy Sharp.

An extensive discussion will follow the presentation of the papers. The discussion will focus on how the papers contributed to the theme of the working group. Tentative plans for publishing the Proceedings are underway.

The inadequacy of linguistic theories which exclude the non-verbal component from analysis

Walburga von Raffler-Engel
Vanderbilt University

Research conducted by this author over a twenty year period to see whether it was possible to adequately describe the conceptual base of a spoken utterance exclusively in terms of verbal language disproved that hypothesis. The years of research were also involved in testing this same hypothesis in regard to speech perception. Here too, the hypothesis that the perception of a spoken utterance can be described using only verbal language was disproved.—Neither the base nor the process of speech perception can be accounted for without recourse to paralanguage, kinesics, and pragmatics. Conversation in face-to-face and telephone interaction must also include audience response which affects speech programming at various stages of development. (It will be shown how kinesics affects telephone conversations.) Linguistic and psycholinguistic theories that do not include nonverbal aspects are erroneous by attributing too much power to verbal language both in its expression and in its perception. First and Second Language Acquisition cannot be understood if confined to the restricted framework of verbalization.—The data to be presented in this paper deal with adult perception and interpretation. The results are based on my research with 100 subjects rating a dyadic interaction and the research conducted by my student Michael Gold with 100 subjects rating monologs on radio and television.

"The Basis of Somerset Maugham's Sensitivity to Nonverbal Communication"

Forrest D. Burt
Texas A&M University

Somerset Maugham's sensitivity to nonverbal communication was grounded in his own experiences of: (1) being orphaned at age nine, (2) developing a speech impediment when he was moved from Paris (where he had spoken French more fluently than English) to England (where he lived with his middle-aged uncle, a vicar and his aunt) and (3) having an unhappy life at Kings School at Canterbury. Understandably these experiences led Maugham to develop a more passive life style, to remain aloof from active life, to look on while others were active. He found himself drawn to writing as a career. Considering such a career impractical, his guardians approved his choice of medical training at St. Thomas Hospital in London. Here Maugham had the opportunity to observe human beings in moments of crisis. His first novel based upon these experiences, *Liza of Lambeth*, relies heavily upon dramatic techniques—particularly dialogue and nonverbal communication. Understandably Maugham turns to drama and has a successful career on the London stage—refining and enlarging his dramatic skills. But the unhappiness of his childhood, the burdens of his past returned to him often and he determines to free himself of it all by writing an autobiographical novel, *Of Human Bondage*, his longest and most popular work. Subsequently Maugham contributes to the war effort as a spy in the British secret service and takes on a special mission to Russia as a secret agent—all consistent with his passive life style. Following the war Maugham writes of these experiences, continues to write plays and travels extensively observing, recording, transforming experiences into fiction. Returning to the subject matter of his unhappy childhood in Whitstable and the exhilarating years in London, he writes a second autobiographical novel, *Cakes and Ale*. In it he turns his powers of observation once more upon himself and his relationship with others. The result is a novel that is heavily dependent upon the narrator's observations of the nonverbal communications of the other characters.

Development of Verbal/Nonverbal Interactional Features

Bates Hoffer and Jeanne Callihan
Trinity Univ.

The child's acquisition and development of gestures—especially illustrators, proxemics, haptics, and so on occur over several years and have been hypothesized as partly correlating with the development of certain linguistic competencies. The research reported here deals in depth with a set of 15 children

videotaped and studied closely over a 9 month period for their acquisition of interactional behavior with non-family adults and children. The study shows the correlations and noncorrelations between the development of behavior, linguistic competence and communicative competence. In particular, the relation of acquisition of illustrator gestures and higher syntactic features are studied as a model for the rest of the research. Statistics, graphs and still photos will make the presentation more understandable.

Non-Verbal and Artifactual Communication in a Commercial

Nicholas Clark
Vanderbilt University

The topic of this paper is to examine a commercial for its non-verbal content. The commercial studied advertises Sharp Copiers and was shown on ABC-WNGE, Nashville, Tennessee, September 8th, 5:48 pm. The commercial is divided into four parts. The first three parts show an actor who portrays the same man in a sequence of three stages of age and social status: a working class businessman; a small middle class businessman; and a sophisticated businessman. The fourth part of the commercial is a sign off message.

Even though this is only a 30 second commercial, one can easily notice the non-verbal and artifactual communication since there is only one actor who plays the three roles. In each of the three parts, the actor changes his non-verbal and artifactual communication to delineate and reinforce the role he is playing. The actor shows that Sharp Copiers can grow with a businessman's needs along with implying a correlation of success with buying a Sharp Copier. The non-verbal communication is focused on the use of arm movements, type of clothes, and manner of walking. The latter correlates with both language and paralinguage.

"The Unspoken Language of Edward Driffield"

Katherine Fell
Texas A&M University

W. Somerset Maugham's speech impediment and interest in the theater prompted Maugham to focus on visual means of communication in his novels.

Maugham incorporates patterns of nonverbal behavior in his depiction of the characters in *Cakes and Ale*. These patterns are particularly apparent in Maugham's use of facial expressions to develop the central character, Edward Driffield. Paul Ekman and Wallace Friesen's study of facial gestures aids in the analysis of this character.

The nonverbal clues in *Cakes and Ale* develop the novel's theme—that literary fame forces a writer to repress his creativity. Driffield's nonverbal behavior reveals this repression.

The nonverbal messages serve both to unmark and to mask Driffield. Facial expressions—smiles, winks, looks, and gazes—unmask the usually well hidden Driffield; but the character usually hides behind his clothes and clean-shaven face—the only nonverbal clues which all the other characters, except the narrator, notice.

Maugham's skillful use of nonverbal communication makes *Cakes and Ale* an excellent portrayal of the difference between what is meant and what is said in conversation.

Do Older People Need More Personal Space?

Ann Heins and Elizabeth Keil
Vanderbilt University

This study investigates the claim that older people need more personal space. The results disproved this claim. Three age groups were investigated, young adult, the middle aged, and the elderly. Other aspects of nonverbal behavior are also reviewed, including level of gesticulation, interactional synchrony, and the use of illustrators, emblems, and adaptors. The results show that age groups have differences in these areas of nonverbal behavior, but the idea that older people need more space was not supported.

Linguistic Fluency vs. Verbal-Nonverbal Fluency, and Redundancy vs. Complementarity: Toward a Revision of Concepts

Fernando Poyatos
University of New Brunswick, Canada

Advances in Nonverbal Communication Studies have proven that verbal language is but one part of the Triple Structure language-paralinguage-kinesics and that the command of the linguistic structures (whether in the context of foreign-language learning, the therapist or business person/client interaction, or an ordinary conversation) represents only one aspect of what traditionally has been termed 'fluency' and 'being fluent'. It is only by being able to decode other people's verbal and nonverbal behaviors in their peculiar coconstruction, and by emitting these behaviors ourselves, that true total verbal-nonverbal fluency can be achieved, beyond which cultural fluency is indispensable in intercultural and intracultural communication, involving somatic as well as environmental and attitudinal systems. Fluency entails a series of conscious and unconscious

behavioral choices which depends on our *fluency quotient*, possessed by every individual in different degrees according, mainly, to age (an important developmental perspective), psychological configuration, and socioeconomic status. The understanding of the self-regulatory functions of behaviors within the person's own repertoire as well as between the behaviors of speaker and listener(s), and the recognition of the qualifiers of the interaction (costruction, intensity, and duration of behaviors) are essential for achieving true *interactive fluency*, for instance, with impaired persons. So it is the revision of the concept of *redundancy*, for what often is hastily labeled redundant is actually *complementary*, since it may add information about, for instance, the exact meaning or intention of the other concomitant behaviors, and because even where it seems to be redundant it may serve to define, for instance, the cultural or social background of the speaker.

Learning to Play the Parts: Language Development as Orchestration

Cecilia K. Yoder

South Oklahoma City Junior College

Language development is conceptualized as a process of increasing differentiation and integration of verbal and nonverbal, linguistic and nonlinguistic resources. Too narrow a conceptualization of the process fails to capture the flexibility in human communication, flexibility which is apparent and crucial even at early stages of language development. This study examines how one young child (boy), 16 months; 3 weeks and 17 months; 3 weeks of age exploits intonation, voice quality features, gaze, gesture and body movement for regulating interaction with his mother during the transition from idiosyncratic to conventionalized language. Regulation involves initiation and engagement as well as disengagement and defense. Significant developments include increased ability to shift gaze while vocalizing both toward and away from his hearer.

Working Group 27:

Generative Syntax

Organizer: Henk van Riemsdijk
Tilburg University

The papers in this working group are highly representative of work in generative grammar in recent years. They demonstrate in particular that despite the often highly technical aspects of the theories developed there is a great deal of flexibility. For one thing, two of the papers deal with questions of Japanese syntax. This is illustrative of the fact that generative grammar has ceased to be a theory for English only. On the contrary, work on quite a number of different languages has shown that increasing the scope of the languages studied does not necessarily imply that you have to sacrifice depth of analysis and insight.

Another fact emerges quite clearly. A theory is not something which you propose and then it will turn out to be either right or wrong. Quite the opposite is true. However interesting, promising, illuminating a theory may be, such as, e.g., Chomsky's recent government-binding theory, there are always many questions to be asked and hopefully answered, many problems to be solved, many principles to be improved, disproved, or explained away. There is a great liveliness in the theoretical and empirical discussions and an enormous flexibility in the possibility of debating whichever aspect of linguistic theory. What makes the debate a cooperative and fruitful one is not some theoretical dogma but a great sense of common purpose in striving for more and better insights into the nature of human grammar.

The talks presented in the working group are typical in this respect. On the purely theoretical side, for example, Kamio presents a wholesale alternative to the bounding theory, which restricts the domain of movement rules. Similarly, Hasegawa offers an alternative account for the domain restrictions on the relationship between reflexive anaphors and their antecedents. Imai reacts to recent proposals to the effect that there is an enormous bifurcation among the languages of the world, configurational languages vs. non-configurational languages. Perhaps the gap between the two is not so deep after all. My own contribution presents an example of how one can try to reduce the redundancy in the theory of grammar, thereby increasing its elegance, by deriving (the effect of) one principle from another one. Reuland and Marantz discuss ill-understood domains of data, in Germanic and Japanese respectively, trying to make sense out of them in light of recent theoretical proposals.

The comments by the discussants show impressively how well-developed pre-

sent-day generative theory is. Many of the components of the model of grammar have attained a rich internal structure and considerable deductive depth. At the same time more and more in-depth studies about widely diverging languages are becoming available. This means that whatever anybody says about some phenomena or some aspect of the theory, he says it not in a virtual vacuum, as used to be the case in the early days of generative grammar, but within the context of a richly articulated scientific field. Proposals cannot fail to have far-reaching consequences for the theory and for particular analyses of phenomena in various languages, and the discussants bring out this fact very clearly.

As a final comment along the same lines, I would like to stress the sense of common responsibility which is becoming more and more instrumental in discussions of this sort. If the field of generative grammar is to survive and reach a certain scientific maturity, it is absolutely essential that generative grammarians accept the fact that their contributions are part of a large enterprise. There is nothing wrong with presenting a new analysis of some phenomena in language X, but the analysis simply cannot and may not evade the responsibility of incorporating the results reached for similar phenomena in language Y nor can it ignore the relevance it has for various parts of the theory. This makes being a generative grammarian a much more difficult job than it used to be—but that is what becoming a mature science is all about. It means that the experts on English syntax must take into account results from Italian, German, Chinese, etc. if they are relevant. It also means that if you come up with some alternative (part of the) theory you have the doubly difficult task of arguing your position while avoiding the danger of dissociating yourself both scientifically and socially from the field. Both the dangers and the positive effects of this development are illustrated in the articles as well as in the discussions.

Stepping down, now, from the exalted level of the sociology of science, the main question will always be whether we have gained new insights into the nature of human grammar. It seems to me quite incontrovertibly the case that we have—in this respect—the articles and discussions will speak for themselves.

On deriving the correspondence filter from the Empty Category Principle

Henk van Riemsdijk
Tilburg University

In our article 'NP-structure' (The Linguistic Review I, 2), Williams and I propose an alternative to May's Quantifier Raising (QR) rule. Our rule, QI, does not move the quantified element, but just assigns a scope index to it and adjoins a phonetically null copy of this index to a containing S. The adjoined

copy is interpreted as the operator and the indexed quantifier as the variable. In our model, QI operates prior to *wh*-movement for various reasons. Hence we must make sure that the derived position of a moved *wh*-word corresponds to the position of the index marking its scope. In the article this is done by the 'correspondence filter', which says, essentially, that a scope-marker (such as a moved *wh*-word) must govern its own scope index. It was argued that this principle is the equivalent in our model of the principle which prohibits QR from applying to quantifiers in COMP in the standard model.

In the present paper I will attempt to derive the correspondence filter from an independently needed principle of grammar, the Empty Category Principle (ECP), which requires that empty categories must be properly governed. The central intuition is straightforward: the structural relationship between a *wh*-word in COMP and a scope index is virtually identical to that between a *wh*-word in COMP and its trace in subject position. The execution of the idea requires a slight reformulation of ECP in terms of chains.

The Notion of Induction and the Constraints on Movement Transformations

Akio Kamio
University of Tsukuba

Several attempts have been made to unify the island constraints proposed by Ross and attain a greater generalization. I would like to make one new attempt here (in collaboration with Naoki Fukui) to provide such a unification. Our proposal is based on the notion of "induction", which is approximately conceived of as the possibility of movement. An induction is provided by S for its two immediate constituents, subject N^3 and ADV directly dominated by S. This induction allows these two constituents to move unboundedly to the left. V^1 and V^2 also provide an induction for the constituents dominated by them except for V and V^1 , respectively. Similarly, N^1 also provides an induction for the constituents dominated by it except for N. Induction from V^1 or V^2 and induction from N^1 are effective within the domain delimited by their first cyclic categories above, i.e. S and N^3 , respectively and their first cyclic categories below, i.e. N^3 or S and N^3 , respectively. That is, constituents receiving a respective induction can move over the domain thus delimited. On these assumptions, the following constraint is proposed: The domain over which a constituent is moved must be covered by a continuous domain of induction.

It will be shown that a substantial portion of Ross's constraints and other major constraints on movement follow from our assumptions. Finally, the bounding theory of Chomsky and others, in particular the Subjacency Condition, is examined and will be shown to be inadequate for the movements in certain constructions. However, they can also follow from our constraint.

On Nonargument subjects

Eric J. Reuland

Groningen

The question to be addressed is why in sentences like (1) *there* walked into the room a man, *there* cannot be replaced by *it*, and in sentences like (2) *it* was ordered that everyone should leave, *it* cannot be replaced by *there*. Any analysis must explain why in a related language like Dutch, in the sentence corresponding to (2) the subject is *er* 'there', rather than *het* 'it'. This difference is matched by another difference: there are impersonal passives in Dutch, like (3) *er* wordt gedanst '*there is danced', but not in English. These differences will be shown to follow from the assumption that in Dutch, but not in English, AG can move into the VP and assign nominative Case in conjunction with some modification of the theory of thematic role assignment. It will be proposed that the presence of some position with either abstract or morphological Case in an NP-chain not only allows its chain to have a θ -role, but that it is rather such a position with Case features which induces the necessity for the chain to have a θ -role. A-chains with 2 or more positions with Case features will be ruled out, as well as A-chains that have Case, but lack a θ -role. Impersonal passives are impossible in English, since there is no way for the subject not to get Case in structures corresponding to (3), hence the subject must receive a θ -role. In Dutch, lowering AG into the VP causes the structural subject to escape from getting Case, hence it need not bear a θ -role. The standard analysis of (1) in which [there, a man] is a chain with *there* carrying the necessary abstract Case will be ruled out: since *a man* clearly has morphological Case such a chain would contain two Case positions in the extended sense. Neither *there* nor *er* has Case at any stage of the derivation, rather, they emerge wherever AG can assign its Case to some chain not containing the structural subject position, e.g. *a man* in (1). In (2) the only position to which AG can assign its Case is the subject position, hence *it*. In the Dutch equivalent, AG can move into the VP, assign Case to the chain containing the complement in that position. Hence *er* as a subject. **het* follows from the Avoid Pronoun Principle, Safir's Definiteness Effect follows from the Extended Projection Principle: [there, a man] is a chain at LF,

Raising and Category Types in Japanese

Alec Marantz

In his "Subject Raising" Kuno (1976) draws our attention to a construction in Japanese similar to "raising to object" structures in English. Three

peculiarities mark this construction: 1. It is basically limited to embedded clauses headed by adjectives or by a copula plus predicate, although the predicate adjective is superficially "verbal" in Japanese and the embedded clauses in these constructions are finite and contain a complementizer. 2. The passive versions of such constructions are restricted to what Kuno calls the "generic reading", but may occur with verb-headed complements. 3. Some verbs which do not participate in the raising to object-like constructions but do take sentential complements also exhibit what looks like raising to subject in the passive, as in English "John was said to have done that" (cf. *They said John to have done that). A new account of raising based on a theory of constituent types and on a transitivity condition on grammatical relations explains these "peculiarities" of raising-type constructions in Japanese, which actually have many parallels in the world's languages. The solution to 1., which also describes the situation in languages like Russian and French, lies in the basic difference between adjectives (and other statives) and true predicates, i.e., proposition producers. 2. only presents a problem if we wrongly think of sentences as passivizing, rather than verbs. Independently determinable properties of Japanese verbs (e.g., that there are no true raising to subject verbs in the language) taken with universal principles predict 2 as well as 3.

On the Significance of 'Move α ' and Empty Categories in Non-configurational Languages

Takashi Imai

Tsuru University, Yamanashi, Japan

In the recent generative literature, inspired by Hale, the syntax of an extremely word-order free language has been explored in detail and its theoretical contribution to generative grammar is remarkable. One assumes that there exists a parameter on the typological categories: configurational vs. non-configurational. Chomsky mentions Hale's observation that there are no empty categories in non-configurational languages. In other words, there are no transformational rules, assuming trace theory in such language.

It is my aim to discuss in some detail the existence of a transformational module in non-configurational languages, assuming empty categories. There have been some suggestions in the recent literature to the effect that "Move α " applies in the LF in non-configurational languages while it applies in the syntax in configurational languages.

In particular, I will argue that the existence of a new level of grammatical representation over LF is proposed, with special reference to Japanese, one of the non-configurational languages.

Syntactic Theory

Kinsuke Hasegawa

Tokyo

Close examination of Chomsky's theory (1981) shows that his leading ideas concerning the organization of grammar and the system of six principles are inadequate in crucial respects. Here I will be concerned with some of the properties of an adequate syntactic theory (i.e. one that can fully incorporate substantial and significant results hitherto achieved, at the same time providing a theoretical framework that can fruitfully guide future research). I argue that such a syntactic theory must incorporate the following properties:

I. Rule system

1. Phrase structure rules (not necessarily predictable from lexical properties)
2. Unbounded and bounded Movement and Deletion transformations
3. Cyclic interpretive rules

II. System of principles

1. Generalized A-over-A Principle
2. General Conditions on (Unbounded) Rules based on the notion 'Degree of Complexity'

In my talk, I will only have time to report on II 1 (which I proposed in 1974 (*Eigoseinen*, Vol 119, Nos 11, 12)), and on I 3 ('The Syntax of Reflexives,' *Eigoseinen*, Vol 124 (1978), Nos 7, 8) in which I argue that the Reflexive Rule applies cyclically and explain the behavior of Reflexives in terms of Generalized A-over-A Principle). I will also give an outline of II 2 for the sake of reference. I believe that it has been proved (by Bresnan and by myself) that unbounded transformations do exist, and that no amount of tinkering with Chomsky's Bounding Theory can adequately constrain these rules. The basic idea of II 2 is that we can isolate a few well-defined structural factors contributing to the degree of complexity in the application of unbounded rules. It has been found that any combination of two such factors leads to ungrammaticality, i.e. the output of an unbounded transformation becomes ungrammatical if the Degree of Complexity assigned to it is equal to or greater than 2.

Working Group 28:

Characteristics of Japanese Expressions in News Reporting

Organizer: Masatake Muraki

International Christian University, Tokyo

Discussants: Iyoko Hirata,¹⁾ Kazuko Inoue,²⁾ Keiichiro Okutsu,³⁾Masayoshi Sagawa,⁴⁾ Masayoshi Shibatani,⁵⁾¹⁾Tokyo, ²⁾Yokohama, ³⁾Tokyo, ⁴⁾Tokyo and ⁵⁾Kobe

With a grant from the Hosono Bunka Kikin (Broadcasting Research Foundation), our group (headed by K. Inoue) has been studying the peculiarities of the syntax, phonology, style, discourse structure of the Japanese language used in radio and TV newscasts and other similar programs. We had many radio and TV newscasts and related programs recorded on tape and transcribed. Standardization of transcription of spoken Japanese is not simple because our complex writing system allows us to represent each expression in different ways depending on which word/morphs are to be represented by *kanji* or *kana*. But consistency and standardization of transcription is necessary for statistical and quantificational comparison of different types of newscasts and other programs, for comparison of different stations, and for comparison of radio/TV and newspapers. In order to make it easier to compare the language of radio and TV newscasts with that of the newspaper, the transcription was standardized so that *kanji* is used in the same way as it is used in the newspaper.

The most important are the facts that newscasts on the radio and TV are in the *desu-masu* style (i.e., the polite colloquial style), in contrast to the *dearu* style (i.e., the non-polite expository style) of the newspaper, and that they are given orally (even though announcers read manuscripts and rarely compose their sentences before the microphone). It was found that radio newscasts and TV newscasts differ very little in their language though the visual aids (pictures, charts, etc.) of TV had been expected to affect the language in some way or another.

The language of the newscasts of NHK (Japan National Broadcasting Corporation) and that of commercial stations were found different in several ways, for example, in redundancy (amount of repetition), the number of adjectives used (Japanese true adjectives are mostly emotional or evaluative), the amount of background information provided as introduction to a news item (as reported by M. Shibatani). In general, NHK newscasts use a more formal language that gives the impression of objective attitude of the newscaster than those of the commercial stations. Comparison was also made between regular newscasts and various types of news commentary. The latter use more informal and

personal expressions.

In such studies, it is difficult to localize specific problems for comparison of discourses. K. Inoue focused on the discourse-initial sentences, especially on their initial parts. A "discourse" here means a single item of news in newscasts. It was found that discourse-initial sentences are often quite long, and contain many embedded clauses, and start with a big noun phrase containing a long non-restrictive relative clause. Such long non-restrictive relative clauses in the discourse-initial position are normally used to give and/or confirm the background information and to prepare the listeners for the following news. This may be a characteristic of verb-final languages. It contrasts sharply with newscasts in English. After the discussion, one of the audience suggested that we also study discourse-final sentences and see if they show any interesting feature, especially with respect to sentence-final elements of modality.

Okutsu examined the passive constructions in newscasts. It was found that indirect passives (adversative passives) are almost non-existent in newscasts, and that most of the passives are direct passives whose subject is inanimate and whose passive agent is deleted. This can be explained by the fact that Japanese transitive structures are most likely to require an animate subject and imply that the speaker's empathy is on the referent of the subject. Passive structures are often chosen in order to show that the newscaster is impartial and objective. Some questions were raised in the discussion period as to the division of the empathy focus Okutsu claims to be brought about by passivization when the passive has an inanimate subject and an overt animate agent. This clearly needs further investigation.

One of the findings of Sagawa's study was that an identical expression (NP or VP) is not always deletable just because it is identical to a preceding expression, but that there are crucial cases where identical expressions must be repeated. Their deletion at least requires that the semantic content of the two clauses that contain them be closely related. Hirata's study on the choice of *wa* and *ga* shows that it cannot be explained just by topic-comment and old-new information structure, but it also depends on whether and to what extent the event reported had been expected. Many of the audience responded to this. Some of them suggested that the types of the predicate should also be taken into account.

We had an audience of over 90, and there were no vacant seats. The discussion, which started at 18:00 and which was to close at 18:30, was very lively, and continued until 18:45. Many stimulating questions were raised, and valuable comments and suggestions were presented (including comments from the point of view of language typology by Mr. T. Tsunoda, comments by Mr. Yano. Ms. M. Ohso, Mr. K. Okuda, Mr. T. Hashiuchi, Mr. K. Fiala, Ms. M. Hubbard and many others).

"Ga or wa for new referents in a discourse"

Iyoko Hirata

Tokyo

Examining some TV and radio newscasts, we notice that the choice between *ga* and *wa* for a new referent which appears in the subject position of a sentence reveals the following stylistic peculiarities of news reporting.

1) The "discourse topic" of a news item is always "something happened," and the information structure of its discourse initial sentence is NEW NEW, where the first term designates the subject and the second the rest of the sentence. In such a case, we expect the subject to be marked by *ga*.

Nearly a half of the discourse initial sentences in our data, however, are found to have subjects followed by *wa*. The thematic *wa* is freely used to mark referents in the subject position if they are famous enough to be assumed to be known to everybody. If *ga* is used in such cases, the listener tends to feel that the speaker has underestimated the state of his general knowledge and imposes information on him. Thus, the function of *wa* in these cases is to decrease the mental distance between the speaker and the listener.

2) Nearly half the NEW referents which are introduced in the middle of a discourse are marked by *wa* if they are regarded to be related to the discourse topic.

Since these characteristics can also be found in news items in the newspaper, we may conclude that they are characteristics of public news reporting.

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"Sentences with multiple embedding in news reporting"

Kazuko Inoue

International Christian University

Various characteristics of Japanese expressions used in newspapers have been studied in terms of syntactic structures and their discourse functions (Inoue 1979, 1982). Almost the same characteristics are found in the NHK news reporting, although it is necessarily in the oral style. Use of long sentences with multiple embedding, one of the characteristics of the Japanese news broadcasting, is chosen for the topic of this paper and studied from the structural and functional points of view. Related topics, such as use of polite forms in

embedded clauses, are also discussed.

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"Why passive?—From the point of empathy"

Keiichiro Okutsu

Tokyo Metropolitan University

It has been widely accepted that the so-called *hizyoo-no-ukemi* (i.e. passive with an inanimate subject) does properly belong to the Japanese language, but has become common because of the influence of foreign (especially European) languages, and that it is better to avoid it.

However, it was found that about 80 percent of the passives in TV and radio newscasts are *hizyoo-no-ukemi*. It seems quite natural in Modern Japanese that when the empathy is on the object of a transitive verb, the sentence is passivized so that the object become the passive subject. Further discussion will be on the important function of *hizyoo-no-ukemi* in Modern Japanese.

"On Deletions and Repetitions in Discourse"

Masayoshi Sagawa

Hosei University

The purpose of the present paper is to observe the data taken from the broadcasting in Japan (not only NHK, but a number of private stations) and to elucidate some of the principles constraining deletions and repetitions.

The paper considers exclusively relations between two consecutive sentences, as a first step, disregarding deliberately larger units in discourse such as paragraphs or whole discourse. With respect to the data, the paper makes two fundamental distinctions: one between prepared (that is, read) announcements and spontaneous and free talking, and the other between literal (that is, unnecessary) repetitions and indispensable repetitions. Interesting observations can be made, from the viewpoint of discourse principles, in spontaneous talking and indispensable repetitions.

"On the stylistic differences between NHK and commercial broadcasting stations"

Masayoshi Shibatani

Kobe University

Comparing some of the 7 p.m. newscasts of NHK-TV (NHK: Japan National Broadcasting Corporation) with the corresponding newscasts of NTV and TBS (commercial broadcasting stations) of the same day, we notice the following differences in the style.

1. NHK is more redundant than commercial stations. NHK uses many more words in giving background information before giving new information.
2. The style of NHK is more formal, impersonal, and assertive. It uses very few adjectives. Commercial stations try to be concrete and vivid, and use more adjectives, onomatopoeic words, mimetic words, *syuzyosi* (sentence-final particles).
3. Nominalized constructions and non-restrictive relative clauses are much more common in NHK newscasts than in those of commercial stations. With some statistical data, these differences will be analyzed further and discussed.

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Their Imperial Highnesses the Crown Prince and Princess



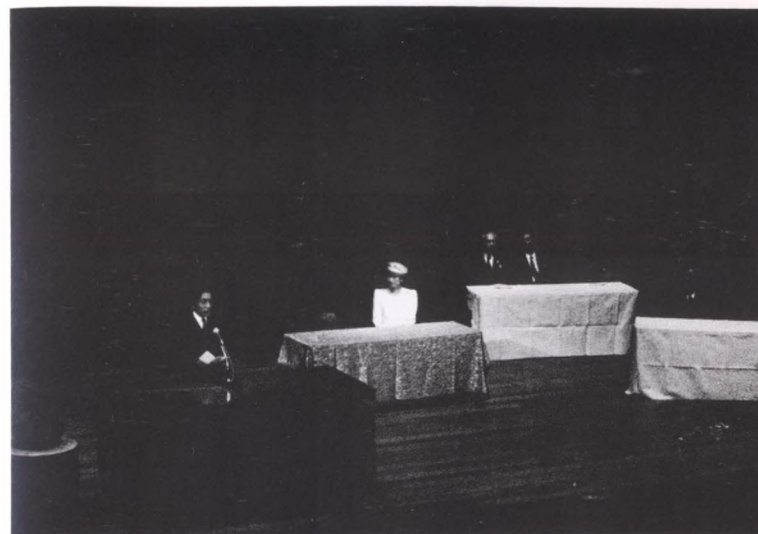
Office holders receiving the royal couple

The Crown Prince



The Crown Princess

Participants at Reception



The Crown Prince addressing at Opening Ceremony



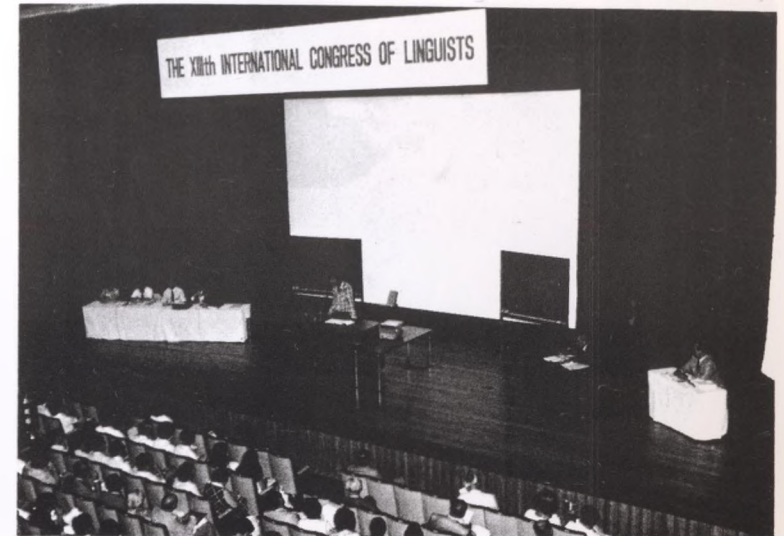
The Congress President at Opening Ceremony



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Registration



Plenary Session



Plenary Session



Section Meeting



Section Meeting



Working Group



Working Group



Banquet



Banquet

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Chairpersons at Closing Session



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The Comité International Permanent des Linguistes (CIPL) is pleased to announce that the 13th International Congress of Linguists will take place at the Nippon Toshiki Center, Tokyo, from August 29th to September 4th, 1982. The distinctive contribution of this Congress will be on the general theme of linguistics in the 1980s. It is intended that it should provide an opportunity to survey and discuss the diversity of theoretical viewpoints which characterizes linguistics at the present time. This aim will be reflected in the Plenary Sessions and in the Section Meetings.

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Organized under the Auspices of the CIPL
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Nippon Toshiki Center, Tokyo
August 29 - September 4, 1982

Congress Program

第13回国際言語学者会議

1982年（昭和57年）8月29日～9月4日
日本都市センター（東京）

会議プログラム (目録案内)

Tokyo, August 29, 1982

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The Japan Society of Chinese Language
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July 1—August 28, 1982: 03-988-9744 (ICL 1982 Office, Gakushuin University)

August 29—September 4, 1982: 03-263-9451 (ICL Congress Office, Nippon Tosh Center, Tokyo)

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Publications (pages; dates). 会議資料 (ページ数と発行時期)

1. First Circular (2 pp.; October 1980)
2. Second Circular (19 pp.; October 1981)
3. Preprints of the Plenary Session Papers (311 pp.; March 1982)
4. Third Circular (70 pp.; May 31, 1982)
5. Congress Program (60 pp.; August 29, 1982)
6. Abstracts of Section Papers and Working Groups (296 pp.; August 29, 1982)
7. List of Participants (60 pp.; August 29, 1982)
8. Proceedings of the XIIIth International Congress of Linguists (ca. 800 pp.; to be published in 1983)

ATTENTION

Each participant who has paid the registration fee receives a Congress bag containing the following items:

1. Your name plate (which replaces the participation card and badge announced in the third circular; you should wear your name plate visibly during the congress; access to the lecture halls and rooms are permitted only to participants wearing blue name plates);
2. Congress Program (which is the final circular containing the most up-to-date information);
3. List of Participants (carrying names and addresses of participants whose final registration cards reached the ICL 1982 Office before August 15, 1982; those arriving afterwards will be listed in the Proceedings);
4. Abstracts of Section Papers and Working Groups;
5. Preprints of the Plenary Session Papers (only for those people from overseas whose registration fee reached the Congress account after June 1, 1982; all the other people should have received a copy already; those who have lost it can obtain an extra copy for 1500 yen at the Congress Office).

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4. 部会およびワーキンググループの発表要旨;
5. 全体研究発表論文集 (海外からの参加者で1982年6月1日以後に参加費が届いた方々を除き、すべてお送りしてあります。紛失した方は1部1,500円でお分けしますので、会議場事務局にお申しください)

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問合せ先、組織、プログラム

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ICL 1982 Office, Gakushuin University, Mejiro 1-5-1, Toshima-ku, Tokyo, Japan-171.

TRAVEL and ACCOMMODATIONS: 旅行と宿泊の申込先

Japan Tourist Bureau, Foreign Tourist Department, Convention Center, 13-1, Nihonbashi 1-chome, Chuo-ku, Tokyo 103, JAPAN.

TIME AND PLACE OF THE CONGRESS:

The 13th International Congress of Linguists will be held at the Nippon Toshi Center (Hirakawa-cho 2-4-1, Chiyoda-ku, Tokyo/Japan-102; Tel: 03-265-8211) from August 29 to September 4, 1982, under the auspices of the CIPL (headquarters at the Hague).

RECEPTION:

A reception will be held for all participants and their accompanying persons from 18:00 to 20:00 on Sunday, August 29, 1982 at the Akasaka Prince Hotel, just opposite the Nippon Toshi Center.

会議開催期日と場所: 東京の日本橋区内。1982年8月29日(日)午後6時から日本橋市センターの向い側にある赤坂プリンスホテルでレセプションが開かれますので、御参加ください(ネームプレートを着用のこと)。

REGISTRATION FEE: 参加費納付

Those participants from overseas who have not been able to pay the fee beforehand are requested to pay 30,000 yen at the REGISTRATION FEE desk and receive the Congress bag. No travellers checks are accepted.

参加費未納者は30,000円(学生半額)を参加費デスクで支払ってコンgres・バッグをお受取りください。

ACCOMPANYING PERSONS: 同席者

who wish to have privileges to attend the Opening session and supplementary programs are requested to pay 3,000 yen at the ACCOMPANYING PERSONS desk and receive an orange name plate.

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同席者は3,000円を同席者デスクで支払ってオレンジ色のネーム・プレートをお受取りください。ただし会議ホールや会議室にはいることはできません。

BANQUET: バンケット

Those who wish to attend the Banquet on Wednesday, 19:00-21:00, September 1, 1982, are requested to pay 10,000 yen at the BANQUET desk on Sunday or Monday, August 29 or 30, 1982. The banquet will be held at Room B & C in the Nippon Toshi Center.

バンケットに御出席なさる方はバンケット・デスクで10,000円を8月29日(日)か8月30日(月)にお支払いください。バンケット日時は9月1日(水)19:00から21:00時まで、場所は日本橋市センターの中のRoom B & Cです。

OFFICE HOURS of the CONGRESS OFFICE:

会議場事務局の開室時間は下記のとおりです。

The ICL 1982 Office at Gakushuin University will move to the Congress Office at the Nippon Toshi Center on Saturday morning, August 28, 1982. The Congress Office is open:

10:00-20:00, Sunday, August 29, 1982;
9:00-20:00, Monday, August 30 through Friday, September 3, 1982;
9:00-14:00, Saturday, September 4, 1982.

EXCHANGE:

Only Japanese YEN (¥) is accepted at regular stores and restaurants. Since the banks are closed on Saturday afternoons and Sundays, you are advised to change foreign currency into Yen at the airport or in big hotels at least for the first few days.

REFERENCE ROOM which was announced in the 2nd circular to display the manuscripts of the speakers at section meetings will not be provided due to space limitations. So, the section speakers need not send us manuscripts. Instead, please prepare your manuscript according to instructions given under the item SECTION MEETINGS.

MAILING ADDRESS AFTER THE CONGRESS:

ICL 1982 Office, Gakushuin University,
Mejiro 1-5-1, Toshima-ku, Tokyo, JAPAN-171

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The ICL 1982 Office at Gakushuin University will function after the Tokyo Congress up to the time of publication of the *Proceedings*. Regarding mail coming after the Congress, only letters which do not require additional postage will be forwarded.

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PROGRAMS プログラムの概要

PLENARY SESSIONS 全体研究会議

The XIIIth International Congress of Linguists is highlighted by the following eight plenary sessions, supplemented by 20 section meetings and 25 working groups.

プログラムの主要部分を占めるのは8つの全体研究会議（プリーナリー・セッション）です。そこでは1980年代の言語研究の理論的多様性を反映する種々のテーマについて内外からの28名の専門家が意見を報告し問題を提起します。そのあと一般参加者との討論が行われます。

PLENARY REPORTERS are requested to submit their manuscripts (maximum 20 pages, double-spaced; revised, shortened or lengthened from the preprints, if necessary) to the Congress Office by noon, Saturday, September 4, 1982. If a revised version is not submitted, the text of the preprints will be published in the *Proceedings*.

Each plenary chairperson is assisted by a Japanese colleague (indicated in square brackets []).

Sunday, August 29, 1982

- 10:00—18:00 Registration at Rooms D & E, Nippon Toshu Center, Tokyo
Please receive your name plate and congress bag containing congress materials.
- 18:00—20:00 Reception at Royal Hall, Annex 5th floor, Akasaka Prince Hotel, just opposite the Nippon Toshu Center

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Monday, August 30, 1982

- 9:00—9:30 Opening Ceremony at Hall (President: Kazuko Inoue, Secretary-General of the Congress)

Opening of the Ceremony

Greetings Prof. Shiro Hattori, President of the Congress
Prof. Shigeo Kawamoto, President of the Linguistic Society of Japan

Prof. Robert H. Robins, President of the Comité International Permanent des Linguistes

Address His Imperial Highness the Crown Prince of Japan

Congratulatory Messages The Hon. Mr. Heiji Ogawa, Minister of Education, Science and Culture

Prof. Kôji Furukawa, President of the Science Council of Japan

Closing of the Ceremony

- 9:30—12:00 (1) Plenary Session on Syntax and Semantics, at Hall

Chairperson: Akira Ota, Tokyo [F. Lobo]

Reporters:

Tahmy Givón, Oregon

Typology and Functional Domains.

Summeo Kuno, Harvard

Principles of Discourse Deletion.

Masayoshi Shibasaki, Kobe

Toward an Understanding of the Typology and Function of Case-marking.

- 14:00—16:30 (2) Plenary Session on Syntax, at Hall

Chairperson: Henk van Riemsdijk, Tilburg [K. Hasegawa]

Reporters:

Antoine Culioli, Paris

Rôles des représentations métalinguistiques en syntaxe.

Simon C. Dik, Amsterdam

Some Basic Principles of Functional Grammar.

Richard Hudson, London

Word Grammar.

Thomas Wasow, Stanford

Idioms: An Interim Report.

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Tuesday, August 31, 1982

- 9:30—12:00 (3) Plenary Session on Semantics, at Hall

Chairperson: Frank R. Palmer, Reading [Y. Ikegami]

Reporters:

Oľga Akhmanova, Moscow

Semantics.

Manfred Bierwisch, Berlin (DDR)

Formal and Lexical Semantics.

Eugenio Coseriu, Tübingen

Pour et contre l'analyse sémique.

Roger Schunk, Yale

Integrating Semantics and Pragmatics.

- 14:00—16:30 (4) Plenary Session on Morphology, at Hall

Chairperson: Victoria Fromkin, Los Angeles [Y. Nagashima]

Reporters:

Mark Aronoff, New York

Potential Words, Actual Words, Productivity and Frequency.

Wolfgang U. Dressler, Vienna

On Word Formation in Natural Morphology.

Judith N. Levi, Illinois

Complex Nouns: New Discoveries, New Questions.

Arnold M. Zwicky, Ohio

An Expanded View of Morphology in the Syntax-Phonology Interface.

Wednesday, September 1, 1982

Free all day. Excursions.

- 19:00—21:00 Banquet (10,000 Yen per person)
at Rooms B & C, Nippon Toshu Center

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Thursday, September 2, 1982

- 9:30—12:00 (5) Plenary Session on Phonetics and Phonology, at Hall

Chairperson: Herbert Penzl, Berkeley [H. Fujisaki]

Reporters:

Englele J. A. Henderson, London

Phonetics and Phonology in the Eighties: Prospects and Problems.

Dee Leblanc, Ohio

The Role of Prosody in the Internal Structuring of a Sentence.

John J. Ohala, Berkeley

The Phonological End Justifies Any Means.

- 12:30—13:30 General Assembly of the CIPL

at Hall

- 14:00—16:30 (6) Plenary Session on Historical Linguistics, at Hall

Chairperson: Thomas V. Gamkrelidze, Tbilisi [K. Matsumoto]

Reporters:

Theodora Bynon, London

Syntactic Reconstruction: A Case Study and some Implications.

Antonio Torro, Madrid

Linguistic Similarity and its Significance: Comparative Procedures.

Calvert Watkins, Harvard

New Directions in Indo-European: Historical Comparative Linguistics and its Contribution to Typological Studies.

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Friday, September 3, 1982

- 9:30—12:00 (7) Plenary Session on Psycholinguistics, at Hall
 Chairperson: Walburga von Raffler-Engel, Nashville [T. Kunihiro]
 Reporters:
 Willem J. M. Levelt, Nijmegen
 The Speaker's Organization of Discourse.
 Els Oksaar, Hamburg
 Historical and Methodological Problems of Psycholinguistics
 and Selected Topics in the Field of Language Acquisition and
 Multilingualism
 Tetiana Slama-Cazacu, Bucuresti
 Psycholinguistics and Linguistics: Old Relationships and Pro-
 mising Prospects

- 14:00—16:30 (8) Plenary Session on Sociolinguistics, at Hall
 Chairperson: Werner Winter, Kiel [T. Shimomiya]
 Reporters:
 Einar Haugen, Harvard
 The Rationale of Language Choice
 Johann Knobloch, Bonn
 Soziolinguistische Probleme in der Bundesrepublik Deutsch-
 land
 William F. Mackey, Laval
 Sociolinguistics: The Past Decade

Saturday, September 4, 1982

- 9:30—12:00
 Closing Ceremony Presiders: Tsuyoshi Nara, Yuriko Ohtsuka
 Overviews of the Plenary Sessions: A. Ota, H. van Riemsdijk, F. R.
 Palmer, V. Fromkin, H. Penzl, T. V. Gamkrelidze, W. von Raffler-
 Engel, W. Winter
 Closing Addresses

12:00 Saturday, September 4, 1982, is the deadline for submitting the papers
 (to be printed in the Proceedings) to the Congress Office.

1. Plenary session papers (maximum 20 pages, double-spaced)
2. Section papers read at the Congress (maximum 5 pages, double spaced)
3. Summaries of working groups by organizers (maximum 5 pages, double-
 spaced)

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SECTION MEETINGS 一般研究部会

Each section chairperson is assisted by a Japanese colleague (indicated
 in square brackets []).

Please confirm the time of your presentation. There are minor changes
 in the time given in the third circular.

There are 20 Section Meetings. Each speaker should be strictly punctual,
 i.e. should finish his/her presentation in 18 minutes including discus-
 sion so that the next speaker can begin on time. If a principal speaker
 cannot attend or does not show up at the specific time indicated, the first
 alternate automatically takes his/her place. The following time-table is
 subject to minor changes.

All principals and alternates listed below for Section Meetings are
 requested to prepare a manuscript (double space, maximum 5 pages) and
 submit it to the Congress Office after you have read it, by noon Saturday,
 September 4, 1982. Please note that presenting a paper in absentia is not
 permitted, and that only the papers actually read at the Congress will be
 printed in the Proceedings. In order to minimize the cost of printing,
 graphs (if any) should be photo-ready beforehand and submitted together
 with the manuscript (graphs should be included within the 5 pages, with
 ample space).

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|---|--|
| 1. Syntax and Semantics
統語論と意味論 | Typology 言語普遍性と類型論 |
| 2. Syntax 統語論 | 12. Dialectology, Linguistic Geo-
graphy 方言学・言語地理学 |
| 3. Semantics 意味論 | 13. Textlinguistics (Discourse
Analysis) テキスト言語学 |
| 4. Morphology 形態論 | 14. Pragmatics 言語使用論 |
| 5. Phonetics and Phonology
音声学と音韻論 | 15. Language and Literature
言語と文学 |
| 6. Historical Linguistics
歴史言語学 | 16. Writing Systems 文字論 |
| 7. Psycholinguistic 心理言語学 | 17. Language Acquisition and
Language Learning
言語習得と言語学習 |
| 8. Sociolinguistics 社会言語学 | 18. Language Planning 言語政策 |
| 9. Recent History of Linguistics
(state of the art and future
perspectives) 言語学の近況(現
状と将来の見通し) | 19. Linguistics and the Computer
言語学とコンピュータ |
| 10. Word Semantics 語彙論 | 20. Semiotics 記号論 |
| 11. Linguistic Universals and | |

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Monday, August 30, 1982, 17:00—21:00

1. Recent History of Linguistics (Section 9) Room C
 Chairpersons: E. Haugen, K. Koerner [K. Inoue, M. J. Hashimoto]
 17:00 K. Koerner, Ottawa: A Critique of Recent Histories of Lingui-
 stics.
 17:20 W. Bahner, Berlin: "Paradigme" ou "courant" dans l'histoire des
 sciences du langage?
 17:40 J. Rischel, København: The Abstractness Paradox in Hjelmslevian
 Linguistics.
 18:00 T. Scharadenidze, Tbilisi: Jan Baudouin de Courtenay und die
 moderne Sprachwissenschaft.
 18:20 M. Mačavani, Tbilisi: Linguistics in the 1980's.
 18:40 R. Bugarski, Beograd: Towards Integration in Linguistics.
 19:00 R. Harris, Oxford: The Speech-Communication Model in 20th
 Century Linguistics and its Sources.
 19:20 W. T. Gordon, Halifax: Sausurean Structuralism and J. R. Firth.
 A Reassessment.
 19:40 W. A. Cook, Georgetown: Case Grammar Theory, 1982.
 20:00 L. Wald, Bucuresti: Aspects of the Relation between Logical and
 Historical Approaches in the History of Linguistics.
 20:20 K. L. Pike, Texas: The Future for Unit-in-Context: The Tag-
 meme
 20:40 V. P. Vomperskij, Moskva: From the History of Russian Linguis-
 tics.
 (alternates)
 1. C. R. Dominguez, Argentina: The Boundaries of Theoretical Lingui-
 stics.
 2. K. Ezawa, Tübingen: Gabelentz und Sekiguchi.
 3. T. Bungarten, Hamburg: Historische Perspektiven und künftige An-
 forderungen an die Fachsprachforschung.

Monday, August 30, 1982, 17:00—21:00

2. Syntax and Semantics (Section 1) Hall
 Chairpersons: R. E. Longacre, G. Bedell [T. Matsuda, K. Hasegawa]
 17:00 E. Erguvanli, Istanbul: Some Aspects of Negation in Turkish.
 17:20 Lee Kee-dong, Yonsei: Nominalizations in Korean.
 17:40 H. Kridalaksana, Indonesia: On Reciprocity.
 18:00 Lee Kiyong, Seoul: A Montague Grammar for Case Languages.

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- 18:20 M. Bily, Lund: The Remarks on the Government-binding Theory
 and Anaphors.
 18:40 S. Oshima, Kochi: On a Possible Extension of the Scope Theory.
 19:00 S. Ohye, Kyushu: Some Peculiar Uses of the Particle *no* in
 Japanese.
 19:20 Yang In-seok, Seoul: On the Interaction of *like*-expressions and
 Negation.
 19:40 D. Willems, Ghent: On the Search of Constants in Verbal Poly-
 semy.
 20:00 I. A. Mel'čuk, Montréal: Meaning-text Linguistic Models and the
 Role of the Dictionary in Linguistic Description.
 20:20 D. C. Ferris, Toronto: The Semantic Value of Syntactic Relations.
 20:40 R. Zuber, Paris: Semantic Restrictions on Certain Complemen-
 tizers.

(alternates)

1. A. Lemaréchal, Poitiers: Semantics of Parts of Speech and Semantics
 of Relations.
2. A. Boone, Brussel: Un problème d'équivalence sémantique: les nomi-
 nalisations.
3. Shin Hyon-sook, Sangmyeong: The Pragmatic Function of the so-
 called Object Marker *-ul* in Korean.
4. O. Nobuhara, Kyoto: Empathy and Postpositions: A Case Study
 from Japanese.
5. T. Yasutake, Nagoya: Givenness, Definiteness and Factivity.
6. M. Sano, Tsukuba: Wh-movement and Markedness.
7. Miao Chin-ab, Hong Kong: Towards a Description of Semantic
 Structure in Modern Standard Chinese.
8. W. van Langendonck, Louvain: Predicative Adnominal Adjuncts in
 a Semantic-Syntactic Dependency Network.
9. M. H. Klaiman, Arizona: Affectiveness and the Voice System of
 Japanese.

Monday, August 30, 1982, 17:00—20:20

3. Historical Linguistics (Section 6) Rooms D & E
 Chairpersons: G. Bonfante, W. Meid [R. Horii, T. Shimomiya]
 17:00 H. B. Rosén, Jerusalem: Diachronic Syntax and the Revival of
 Hebrew.
 17:20 H. Penzl, Berkeley: Graphemischer "Figuralismus" und die Metho-
 den der historischen Phonologie.

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- 17:40 E. C. Polomé, Texas: The Dialectal Position of Germanic within West-Indo-European.
 18:00 D. K. Nyländer, McGill: The Nature of Syntactic Change in a Creole Language.
 18:20 I. Dyen, Yale: Towards a History of the Sulawesi Languages.
 18:40 C. E. W. Jenewari, Nigeria: Toward a Diachronic Theory of Ijo Gender.
 19:00 R. Neuberger-Donath, Israel: Die Funktionen des Optativs in abhängigen Aussagesätzen.
 19:20 A. S. Mel'nikuk, Kiev: Some Theoretical Problems of Historical Linguistics.
 19:40 K. Noda, Nagoya: Ergativity in the History of Persian.
 20:00 M. Ogura, Berkeley: Word Frequency and Lexical Diffusion in ModE Shortening.

Monday, August 30, 1982, 17:00—20:20

4. Word Semantics (Section 10) Room B

Chairpersons: W. Bahner, D. Wunderlich [T. Kunihiko]

- 17:00 L. Lipka, München: A Multi-level Approach to Word-formation.
 17:20 M. G. de Boer, Utrecht: A Lexical Entry for Function Words.
 17:40 A. Newman, Jerusalem: Hebrew Verbs of Dress.
 18:00 B. Ralph, Göteborg: Ordinary Language as Metalanguage in Word Semantics.
 18:20 S. Kubo, Matsuyama: Lexical Semantics in Montague Grammar.
 18:40 L. Minajeva, Moskva: The Semasiological Analysis of a Word in terms of Lexicological Phonetics.
 19:00 R. Chatterjee, Singapore: Chomsky and Wittgenstein on Word Meaning.
 19:20 W. Kühlwein, Trier: A Sociosemiotic Approach to Contrastive Lexicology.
 19:40 K. Schildmann, Bonn: Envisaging an Historical Grammar of Sumerian.
 20:00 A. Burkhardt, Darmstadt: The Principles of Pragmatic Word-Semantics.

Monday, August 30, 1982, 17:00—19:40

5. Pragmatics (Section 14) Room A

Chairpersons: J. Hinds [M. Muraki]

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- 17:00 D. E. Cooper, Surrey: Metaphor.
 17:20 Th. R. Hofmann, Toyama: Why Must there be a Semantic Representation?
 17:40 H. Haverkate, Amsterdam: The Problem of Direct and Indirect Speech Acts.
 18:00 J. Verhaar, Spokane: Two Aspects of Pragmatics: Topicality and Iconicity.
 18:20 E. Parret, Louvain: Shifting In and Shifting Out.
 18:40 A. Ueda, Tokyo: The Co-operative Principle, Maxims and Language Specificity.
 19:00 A. Mittwoch, Jerusalem: Rules for the Ill-Mannered?
 19:20 D. R. Vanderveken, Quebec: Illocutionary Force and Meaning.

Monday, August 30, 1982, 17:00—18:20

6. Semiotics (Section 20) Room F

Chairperson: S. Kawamoto

- 17:00 B. Schlerath, Berlin: Language and Music.
 17:20 F. Poyatos, New Brunswick: Forms, Functions and Applications of Paralanguage as a New Multidisciplinary Area.
 17:40 Y. Tohyama, Tokyo: A Semiotic Analysis of Meeting and Parting Rituals in Japanese and English.
 18:00 J. Pesot, Quebec: The Genesis of Language in the List of Peircean Semiotics (in French).

Tuesday, August 31, 1982, 17:00—21:00

1. Syntax (Section 2) Hall

Chairpersons: A. Bangboose, W. O'Neil [Y. Ohtsuka]

- 17:00 M. Montalbetti and M. Saito, MIT: Tough Constructions and the O-Criterion.
 17:20 S. Starosta, Hawaii: Patient Centrality and English Verbal Derivation.
 17:40 E. J. Reuland, Groningen: On Mixing Configurational and Non-configurational Properties.
 18:00 K. Matsumura, Tokyo: Quasi-relative Clause in Mari.
 18:20 Henn-Memmesheimer, Duisburg: What Possibilities are Offered by Different Theories of Grammar for the Contrastive Description of Nonstandard Syntax?

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- 18:40 T. Imai, Yamanashi: ECP and the Distribution of Empty Categories.
 19:00 S. Miyara, Okinawa: Reordering in Japanese.
 19:20 K. S. Yadurajan, Hyderabad: The Open Path Condition.
 19:40 J. Bailard, UCLA: The Role of Word Formation Rules in the Causative Constructions of Non-Configurational Languages.
 20:00 Lee Chung-min, Seoul: Conditional Constructions in Korean.
 20:20 S. Tonoike and J. A. Bizassa, Tokyo: Multiple Argument Noun Phrases and Case in Japanese.
 20:40 M. Saltarelli, M. Azkarate, D. Farwell, J. Ortiz de Urbina, Illinois: On the Syntax of Free Word Order Languages: Evidence from Basque.

Tuesday, August 31, 1982, 17:00—20:20

2. Historical Linguistics (Section 6, continued) Rooms D & E

Chairpersons: I. Dyen, E. Pulgram [K. Matsumoto]

- 17:00 S. J. Keyser and W. O'Neil, MIT: The Reduction of Optionality and Language Change.
 17:20 B. de Chene, Tokyo: Morphological Segmentation and Resegmentation.
 17:40 P. M. Tiersma, San Diego: Rule Recession and Rule Loss.
 18:00 B. H. Bichakjian, Nijmegen: Natural Selection and Linguistic Change.
 18:20 W. Meid, Innsbruck: The Indo-European Lexicon and its Usage as a Problem in Reconstruction.
 18:40 G. Bonfante, Roma: The Tocharian Accent.
 19:00 B. Sundby, Bergen: The Codification of Prescriptive Grammar.
 19:20 O. N. Trubačev, Moskva: Slavs and Indo-Europeans from the Etymological Viewpoint.
 19:40 N. A. Syromjatnikov, Moskva: Proto-Japanese Phonetic Laws.
 20:00 S. R. Banerjee, Calcutta: Some Problems of Historical Linguistics.

Tuesday, August 31, 1982, 17:00—20:40

3. Semantics (Section 3) Room A

Chairpersons: H. Kučera, M. Radovanović [Y. Ikegami]

- 17:00 J. C. Choul, Dalhousie: Formal and Non-formal Conditions on Semantic Paraphrase.
 17:20 J. Schröpler, Heidelberg: Comparative Onomasiology and Semantics.

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- 17:40 Lee Ik-hwan, Seoul: Syntax and Semantics of Imperative Sentences.
 18:00 D. Zaefferer, München: Semantics of Sentence Mood in Typologically Differing Languages.
 18:20 M. S. Anwar, Kuwait: Intentional Semantic Adaptations.
 18:40 A. Costăchescu, Craiova: Remarks on Nominalization in a Montague Grammar.
 19:00 B. Rieger, Aachen: Generating Connotative Dependency Trees of Fuzzy Word Meanings.
 19:20 F. Heny and D. Wheeler, Groningen: A Categorical Analysis of Japanese Tone.
 19:40 P. Weingartner, Salzburg: Weak Relevant Logic for Natural Language.
 20:00 N. Danielsen, Odense: A Disseminational Analysis of Human Language Sentences.
 20:20 I. Bellert, Quebec: Lexical Features of Linguistic Quantifiers and Well-Formedness of Logical Forms.

Tuesday, August 31, 1982, 17:00—21:00

4. Sociolinguistics (Section 8) Room C

Chairpersons: G. Nickel, A. Elzaincin [F. Lobo]

- 17:00 A. Elzaincin, Montevideo: Variation/Variability within a Linguistic Contact Domain.
 17:20 J. Ornstein-Galicia, Texas: Linguistic and Social Aspects of Pachuco Caló.
 17:40 O. Uribe-Villegas, Mexico: Sociolinguistics in IISUNAM.
 18:00 Li Jen-luei, Taipei: Linguistic Variations of Different Age Groups as Mechanism of Linguistic Change.
 18:20 B. Hoffer, Trinity: Cross-Cultural Sociolinguistic Profiles.
 18:40 S. Ide, Tokyo: Two Functional Aspects of Politeness in Women's Language.
 19:00 R. Filipović, Zagreb: Pseudo-Anglicisms A Sociolinguistic Analysis.
 19:20 I. Broch and E. H. Jahr, Tromsø: Russenorsk, the Russo-Norwegian Pidgin: New Findings.
 19:40 G. Leitner, Berlin: Indian English, a Critique of Ethnographic Analysis.
 20:00 P. Nelde, Brussels: Three Issues on Languages in Contact.
 20:20 J. Penfield, Texas: A Socio-cultural View of Language Contact.

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- 20:40 N. F. Alieva, Moskva: Predicative (Shifter) Categories in Some Languages of Analytical Type.

Tuesday, August 31, 1982, 17:00—19:00; 19:00—21:00

5. Language and Literature (Section 15) Room F

Chairperson: R. R. Mehrotra [T. Nara]

- 17:00 J. Söderlind, Uppsala: The "Tone" of a Text in Linguistic Terms.
17:20 J. L. Heny, Groningen: Metricality and Complexity in Verse.
17:40 A. Betten, Regensburg: Language in Modern Drama, compared with spoken discourse.
18:00 G. P. Boguslavskaja, Saransk: Grammatical Unexpectedness in the Belles-Lettres Style.
18:20 M. K. Hiraga, Tokyo: Metaphor and Poetry.
18:40 V. Kukhareenko, Odessa: Linguostylistic Analysis of the Language of an Author.

5'. Linguistics and the Computer (Section 19) Room F

Chairperson: H. Spang-Hanssen [T. Ishiwata]

- 19:00 C. P. Browman, O. Fujimura, E. L. Ohira, Bell Labs: Demisyllabic Synthesis by Rule using Lingua.
19:20 O. Vollnhals, München: Utilization of a Commercialized Linguistic Data Bank System for the Electronic Storage and for the Automatic Production of Dictionaries.
19:40 H. Frank, Paderborn: Computer-based Information Retrieval by means of ILo-Summaries.
20:00 Y. Kusanagi, Tsukuba: A Model of Automatic Analysis of Time-related Expressions in Japanese.
20:20 J. Seppänen, Helsinki: Hierarchic Class Networks.
20:40 I. Bátor, Koblenz: Process Based Model of Language.
(alternate)
L. R. Smith, Newfoundland: An Electronic Linguistic Consultant.

Tuesday, August 31, 1982, 17:00—18:40; 19:20—21:00

6. Word Semantics (Section 10, continued) Room B

Chairperson: I.-S. Yang [K. Inoue]

- 17:00 A. Syrkin, Jerusalem: Some Notes on Upanishadic Etymologies.

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- 17:20 H. Kakehi, Kobe: Onomatopoeic Expressions in Japanese and English.
17:40 F. Bakos, Budapest: Denomination and Contrastivity: Semantic Typological Research of a Group of Compound Nouns in Romance, Germanic and Slavonic Languages.
18:00 L. Cserecsenyi, Szeged, and J. Hidasi, Budapest: Some Problems of Homonymy.
18:20 L. Warnant, Liège: Principes de cinéto-syntaxe.

6'. Recent History of Linguistics (Section 9, continued) Room B

Chairperson: J. Rischel [T. Matsunami]

- 19:20 J. Hewson, Newfoundland: Content and Expression from Sausure to the Present.
19:40 W. Moskovich, Jerusalem: Jewish Interlinguistics.
20:00 J. Kálmár, Toronto: The Phoenix of Linguistic Evolution.
20:20 V. Tauli, Uppsala: The Future 'Paradigm' of Linguistics.
20:40 U. Baichura, Leningrad: On the Contribution of Tatar Scholars to General Linguistics.

Wednesday, September 1, 1982

Free all day. Excursions.

19:00—21:00 Banquet (¥10,000.— per person)
at Rooms B & C, Nippon Toshi Center

Thursday, September 2, 1982, 17:00—21:00

1. Phonetics and Phonology (Section 5) Room A

Chairpersons: C. Hagege, P. Kiparsky [Y. Ohtsuka, S. Haraguchi]

- 17:00 Z. Simada, Kitazato, and J. Gauffin, Stockholm: An Examination of the Area of Tongue-Palate Contact in Swedish Dental Stop Production.
17:20 J. Gvozdanović, Amsterdam: Patterning of Distinctive Features in Relation to Variability.
17:40 D. C. Walker, Ottawa: Chain Shifts in Canadian French Phonology.
18:00 Y. C. Morin, Montréal: La (dé)nasalisation en français: phonologie ou morphologie?
18:20 H. Kusakabe, Tokyo: An Explanation of the Japanese Accentuation by the Dual-toneme Scheme.

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- 18:40 Lee Byung-gun, Seoul: The Contra-restructuring Constraint.
19:00 N. Davidsen-Nielsen, København: Phonological Neutralization.
19:20 G. Bedell, UCLA: Chukchi Vowel Harmony.
19:40 M. Ohala, San José: Acoustic and Perceptual Correlates of Stress in Hindi.
20:00 T. Matsushita, Gifu: Exceptionality of Norman-French Loanwords to Open Syllable Lengthening in Middle English.
20:20 T. de Graaf, Groningen: Vowel Duration and Vowel Quality.
20:40 Y. Homma, Osaka: Rhythm of Tanka, Short Japanese Poems.

Thursday, September 2, 1982, 17:00—20:20

2. Linguistic Universals and Typology (Section 11) Room D

Chairpersons: V. Jarceva, S. Wurm [J. Ikegami]

- 17:00 C. Lehmann, Köln: The Present State of Linguistic Typology.
17:20 L. Dezsó, Debrecen: Universal and Typological Studies. Differences in Methods.
17:40 C. Hagege, Paris: Linguistic Universals as General Tendencies.
18:00 S. Shaumyan, Yale: Ergativity, Syntactic Typology and Universal Grammar.
18:20 T. Tsunoda, Nagoya: A Redefinition of 'Ergative' and 'Accusative'.
18:40 E. Tiffo, Montréal: Les constructions passives en bourouchaaki.
19:00 K. J. Howell, Florida: Object-initial Languages and their Implications for Word Order Universals.
19:20 G. Lazard, Paris: A Possible Universal. A Dichotomy of Actance Constructions according to Categories of the Object.
19:40 M. V. Jankovili, Tbilisi: Basic Structure of the Sentence as Universal.
20:00 I. G. Melikishvili, Tbilisi: Universal Relations between the Resonance Features and Sonorant Phonemes.

Thursday, September 2, 1982, 17:00—20:20

3. Psycholinguistics (Section 7) Room E

Chairpersons: H. Goodluck, G. Pridcaux [A. Kamio]

- 17:00 G. P. Prideaux, Alberta: The Relative Accessibility of Cognitive Strategies.
17:20 J. F. Kess and R. A. Hoppe, Victoria: The Interaction of Bias and Context in Ambiguity Detection.

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- 17:40 V. Frontin, UCLA: On Models of Linguistic Performance.
18:00 L. Schiar-Cubral, Brazil: Development of Narrative Competence in Children.
18:20 T. M. Walsh and K. C. Diller, New Hampshire: Neurolinguistic Feature Detection in Wernicke's Area.
18:40 A. Kamio, Tsukuba, and K. I. Harada, Nagoya: A Repetition Experiment on Children's Comprehension of Complex Sentences in Japanese.
19:00 D. Gunzburger, Utrecht: Quantification of Linguistic Expectancy by means of Frequency Estimation Tests.
19:20 E. R. Gammon, Fresno: English Quantifiers and the Perception of Risk.
19:40 P. Sgall, Praha: On the Notion of the Meaning of the Sentence.
20:00 K. H. Hummel, Montréal: Bilingual Memory for Prose.

Thursday, September 2, 1982, 17:00—21:00

4. Textlinguistics, Discourse Analysis (Section 13) Room B

Chairpersons: G. Wienold, T. Slama-Cazacu [A. Kawashima, T. Kaneko]

- 17:00 B. L. Dubois, New Mexico: Function of Intonation Contours in Biomedical Speeches.
17:20 R. E. Longacre, Texas: Spectrum, Profile and Constituent Structure in Text Analysis.
17:40 T. H. Wilbur, UCLA: Grounding in Basque Sentence and Discourse Structure.
18:00 A. Neubert, Leipzig: Discourse Analysis of Translation.
18:20 U. Fries, Zürich: Diachronic Textlinguistics.
18:40 S. K. Maynard, Connecticut: Flow of Discourse and Linguistic Manipulation.
19:00 B. R. Lavandera, Argentina: The Harmonics of Discourse.
19:20 G. Wigen, Elverum: Spontaneous Dramatization in Semi-Formal Conversation.
19:40 Van de Velde, Antwerp: Coherence Relations in Texts and Inferential Processing.
20:00 K. Fiala, Praha: Sentence Delimitation and Sentence Order in Japanese.
20:20 M. Langleben, Jerusalem: On Dialogic Interaction.
20:40 P. Saukkonen, Oulu: What are the Main Semantic-Pragmatic Features of Stylistic Text Types?

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(alternates)

1. E. A. Berendi, Chiba: Strategies in Japanese and English Dyadic Discourse.
2. C. M. DeWolf, Chiba: Translation and Transmediation.
3. K. Facey, London: Japanese Reflexivization and Psychological Semantics.
4. G. Kolenskij, Moskva: Communicative Basis for Adequate Interpretation of Text Semantics.

Thursday, September 2, 1982, 17:00—18:00; 18:20—20:00

5. Language Planning (Section 18) Room C

Chairperson: H. Benediktsson [S. Morita]

- 17:00 A. Bamgbose, Nigeria: When is Language Planning not Planning?
- 17:20 G. Nickel, Stuttgart: Contrastive Linguistics, Error Analysis and their Relevance for Language Planning including Language Minimization.
- 17:40 F. Marcos-Marin, Madrid: The Problem of Language Reform in XVth Century Spanish.

5'. Sociolinguistics (Section 8, continued) Room C

Chairperson: H. Benediktsson [S. Morita]

- 18:20 S. Nagara, Michigan: Componential Analysis of Lexical Structure of Pidgin Languages and Sociolinguistic Factors Affecting their Acquisition.
- 18:40 C. Stieblich, McGill: Accommodation in Interpersonal Encounters.
- 19:00 R. R. Mehrotra, Banaras: Personal Names in Hindi.
- 19:20 Wang De-chun, Shanghai: Rules of Speech and Object of Stylistics.
- 19:40 M. Nishimura, Pennsylvania: On Intrasentential Code-Switching in Japanese/English.

Thursday, September 2, 1982, 17:00—18:20, 18:40—20:00

6. Syntax (Section 2, continued) Hall

Chairperson: S. Starosta [T. Matsuda]

- 17:00 L. Szabó, New Brunswick: Unmarked Values in Languages. The Example of Malecite.
- 17:20 K. Masunaga, Harvard: Bridging.
- 17:40 E. Pontes, Brazil: Anacoluthon and "Double Subject" Sentences.

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- 18:00 C. Y. Ning, Hcilongjiang: From Nonlinear Realistic Grammar to Linear Formal Grammar.

6'. Semantics (Section 3, continued) Hall

Chairperson: S. Starosta [T. Matsuda]

- 18:40 J. S. Gruber, Nigeria: Lexical and Conceptual Semantic Categories.
- 19:00 E. Vasiliu, Bucuresti: Modality and Existence.
- 19:20 L. Theban, Bucuresti: From Universal Semantax to Romanian and Japanese Syntax.
- 19:40 Tung Chao-hui, Taipei: Expansion and Identification of Reduced Nominals in English.

Friday, September 3, 1982, 17:00—20:40

1. Morphology (Section 4) Room B

Chairpersons: H. Rosén, L. Lipka [Y. Nagashima]

- 17:00 A. Marantz, Harvard: Restricting the Power of the Morphological Component: Reduplication in Southern Paiute.
- 17:20 D. Wunderlich, Düsseldorf: On the Compositionality of German Prefix Verbs.
- 17:40 P. Barbaud, Quebec: About a Tendency to Feminize in French Canadian.
- 18:00 E. D. Cook, Calgary: Phonologically Conditioned Alternation vs. Morphologically Conditioned Alternation.
- 18:20 I. R. Smith, Singapore: Morphological Operations, Historical Evidence and English *q*.
- 18:40 A. Jerejian, New York: The Role of the Morpheme in Armenian.
- 19:00 O. Schwarzwald, Israel: Gender Distinction and Feminine Formation in Modern Hebrew.
- 19:20 V. M. Alpatov, Moskva: On Linguistic and Psycholinguistic Meaning of the Notion of Word.
- 19:40 R. van Zonneveld, Groningen: Categorical Morphology.
- 20:00 J. van Marle, Amsterdam: On the Paradigmatic Dimension of Morphological Productivity.
- 20:20 M. A. Gabinskij, Kijinev: Allautemy and its Place in a Linguistic Structure.

Friday, September 3, 1982, 17:00—21:00

2. Phonetics and Phonology (Section 5, continued) Room A

Chairperson: O. Fujimura, G. Kolenskij [K. I. Harada, M. Sugito]

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- 17:00 M. Sudo, H. Yoshioka, S. Kiritani, M. Sawashima, Tokyo: On the Articulation of Japanese /r/.
- 17:20 M. R. Smith, Connecticut: A Complex Acoustic-Phonetic Description of Word Stress.
- 17:40 M. P. R. Van der Broecke and J. Stoop, Utrecht: A Posteriori Consonant Features in Inner Speech.
- 18:00 Lee Hyun-bok, Seoul: A Critical Appraisal of the IPA Cardinal Back Vowels by the X-ray Microbeam System.
- 18:20 U. Bortolini, G. Pini, G. Zilli, F. E. Ferrero, Padova: Dimensions of Perception for Italian Consonants: Multidimensional Analysis.
- 18:40 Yan Xue-qun, Huazhong: On the Chu People, the Chu Dialect, and the Chu Phonemic System.
- 19:00 Kim Suksan, Seoul: On the Compensatory Lengthening.
- 19:20 M. H. Ibrahim, Jordan: On the Contrast between /a/ and /æ/ in Modern Arabic.
- 19:40 M. Sherard, Kyoto: Phonological Diversity and Sound Change in Shanghai.
- 20:00 R. P. Dixit, Louisiana: On Defining Aspiration.
- 20:20 P. Martin, Toronto: Phonetics and Phonology. The Example of Intonation.
- 20:40 K. Shimizu, Nagoya, and M. Dantsuji, Kyoto: A Study on the Synthesis and Perception of /r/ and /l/.

Friday, September 3, 1982, 17:00—19:40; 20:00—21:00

3. Dialectology, Linguistic Geography (Section 12) Room C

Chairperson: B. A. Serébrennikov [T. Koizumi]

- 17:00 V. Panupong, Bangkok: Word Geography in Nakhon Ratchasima.
- 17:20 F. Inoue, Tokyo: New Dialect and Linguistic Change.
- 17:40 T. Ogino, Tokyo: Computer-aided Analysis of Field Survey Data.
- 18:00 W. Viereck, Bamberg: Presentation and Interpretation of English Dialects. Computer-assisted Projects.
- 18:20 Ting Pang-hsin, Taipei: Some Aspects of Tonal Development in Chinese Dialects.
- 18:40 R. D. Clement, Edinburgh: Scottish Gaelic Preaspiration.
- 19:00 B. A. Serébrennikov, Moskva: On Areas of Meanings.
- 19:20 M. A. Awwal, Bangladesh: The Chakma Dialect.

3'. Linguistic Universals and Typology (Section 11, continued) Room D

Chairperson: M. J. Hashimoto [N. Tsuji]

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- 20:00 V. Jarceva, Moskva: Typological Restrictions of Syntactic Ambiguity.
- 20:20 A. E. Backhouse, Monash: Lexical Stratification and Modern Japanese.
- 20:40 J. R. Wirth, Wisconsin: Toward Universal Principles of Word Formation.

Friday, September 3, 1982, 17:00—21:00

4. Language Acquisition and Language Learning (Section 17) Hall

Chairpersons: C. J. Bailey, D. Steinberg [F. Lobo, T. Kunihiro]

- 17:00 L. I. Soudek and M. Soudek, Illinois: Mental Lexicon in Second Language Learning.
- 17:20 B. Lust, T. Wakayama, H. Hiraide, W. Snyder, M. Bergman, Cornell and Yokohama: Comparative Studies on the First Language Acquisition of Japanese and English.
- 17:40 Li Shu-wen, Yanbian: The Development of Communication Skills.
- 18:00 S. Flynn, MIT: Comparison of Japanese and Spanish Speakers Acquisition of English Restrictive Relative Clauses.
- 18:20 J. S. Lin and L. M. Stanford, Alberta: Bilingual Children's Acquisition of Five English Derivational Processes.
- 18:40 G. Wienold, Konstanz: Linguistics-based Revision of Foreign Language Teaching Materials.
- 19:00 S. Miyagawa and G. Walker, Ohio: Self-sustaining Dialect.
- 19:20 D. R. Hall, Malaya: The Developmental Theory of Language Teaching.
- 19:40 A. Szentgyörgyvári, Budapest: Dual-plane Strategies in Foreign Language Learning.
- 20:00 G. Francescato, Trieste: Remarks on Children Bilingualism.
- 20:20 B. Lust, Y. C. Chien, L. Mangione, Cornell: First Language Acquisition of Mandarin Chinese.
- 20:40 Flores d'Arcais, G. B. Nijmegen: The Acquisition of Semantic Knowledge: The Connectives.

(alternates)

1. G. Gagné, Montréal: Learning the Standard Variety of a Mother Tongue in School.
2. K. Ohama, Tokyo: Development of Japanese Vocabulary in Bilingual Children.

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Friday, September 3, 1982, 17:00—19:40

5. Writing Systems (Section 16) Room E

Chairperson: G. Francescato [T. Nishida]

- 17:00 S. Morag, Jerusalem: Motivation and Methodology in Reforming Writing Systems.
 17:20 Gong Hwang-cherng, Taipei: Chinese Elements in the Tangut Script.
 17:40 Y. Yosimati, Hamamatsu: Proper Characters and Vowel System of the Old Japanese Language.
 18:00 D. Neriua, Rostock: On the Linguistic Foundations of the Use of Capital Letters.
 18:20 G. James, Exeter: Principles in Script Form: Applied Folk Linguistics.
 18:40 I. S. Mackey, Laval: Programming Incrementally Integrated Mini Alphabets.
 19:20 J. M. Wilding, England: Which Way to Jupiter?

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WORKING GROUPS 特別研究部会

ワーキンググループはオーガナイザーが特長なテーマについて数名の討論者を選定し討論するものです。

Working Groups differ from Section Meetings in that their content and structure are planned by a single person. The time available is five hours for each group. Discussants were chosen by the organizer. Please note that the schedule for working groups was changed from the previously announced evening sessions due to space limitations and the strong wish of the organizers to have more time for discussion.

ORGANIZERS of Working Groups are requested to submit a summary (double spaced, maximum 5 pages) to the Congress Office by noon Saturday, September 4, 1982. The already received half-page abstracts (in blue paper) by discussants will be printed in the *Proceedings* following the summary by the organizer. The organizers are encouraged to publish elsewhere longer papers by themselves and discussants.

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Monday, August 30, 1982, 10:30—15:30

- (1) New Interdisciplinary Perspectives in Linguistics through Nonverbal Communication Studies. (No. 2) Room B
 Organizer: Fernando Poyatos, New Brunswick
 Discussants: Thomas Bruneau, Guam; Hanne Martinet, København; Helmut Morsbach, Glasgow; Harvey Taylor, Beijing; Walburga von Raffler-Engel, Nashville; Fernando Poyatos.
- (2) Speech Production. (No. 15) Room D
 Organizers: James H. Abbs, Madison; Osamu Fujimura, Bell Labs
 Discussants: James H. Abbs; Osamu Fujimura; K. S. Harris, New York; Fredericka Bell-Berti, St. John's; Betty Tuller, New York; D. A. Dinnsen, Indiana; Hajime Hirose, Tokyo; Mark Y. Liberman, Bell Labs; S. E. G. Ohman, Uppsala; Peter F. MacNeilage, Texas; S. Kiritani, H. Hirose, M. Sawashima, Tokyo; John J. Ohala, Berkeley; Hiroya Fujisaki, Tokyo.
- (3) Patterns of Language Impairment in Aphasia (No. 20) Room C
 Organizer: Sumiko Sasanuma, Yokohama
 Discussants: Sumiko Sasanuma; John A. Bisazza, Tokyo; Florian Coulmas, Tokyo; Akio Kamio, Tsukuba; Hanna Ulatowska, Texas.
- (4) Origin of Japanese (No. 23) Room A
 Organizer: Kazuo Mabuchi, Tokyo
 Discussants: Lee Kimoon, Seoul; Shichiro Murayama, Tokyo; Susumu Ohno, Tokyo; Nikolaj A. Syromjainikov, Moskva; Karl H. Menges, Wien; Kim Kong-chil, Cheju; László Szabó, New Brunswick; P. Kothandaraman, Madras; Takao Kawamoto, Joetsu; Kōji Atarashiya, Sapporo; (Roy A. Miller, Seattle).
- (5) The Present State of Proto-Indo-European Studies (focus on phonology) (No. 25) Room F
 Organizer: Edgar C. Polomé, Texas
 Discussants: Edgar C. Polomé; Haiim Rosen, Jerusalem; Alexandr S. Melnikuk, Kiev; Thomas V. Gamkrelidze, Tbilisi; Vjačeslav V. Ivanov, Moskva; Antonio Tovar, Madrid; Bernfried Schlerath, Berlin; V. A. Dybo, Moskva; Robert Schmitt-Brandt, Heidelberg; F. Rodríguez Adrados, Madrid; Françoise Bader, Paris; Thomas Markey, Michigan; Wolfgang Meid, Innsbruck; Giuliano Bonfante, Roma.

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(6) Mood and Modality (No. 24) Room E

Organizer: Frank R. Palmer, Reading

Discussants: Frank R. Palmer; G. P. Boguslavskaja, Saransk; Walter A. Cook, Georgetown; Thomas R. Hofmann, Toyama; Beatriz R. Lavandera, Buenos Aires; Daniel Vanderveken, Quebec; Co Vet, Groningen.

Tuesday, August 31, 1982, 10:30—15:30

(1) Universals of Linguistic Action (No. 4) Room C

Organizers: Florian Coulmas, Tokyo; Hartmut Haberland, Roskilde; Jacob Mey, Odense; Jef Verschueren, Berkeley

Discussants: F. Coulmas; H. Haberland; J. Mey; J. Verschueren; Thomas Ballmer, Bochum; Steven Davis, Simon Fraser; Thorstein Fretheim, Trondheim; Henk Haverkate, Amsterdam; Thomas R. Hofmann, Toyama; Naomi Miyake, San Diego; Joyce Penfield, Texas; Danny D. Steinberg, Hawaii.

(2) Fachsprachen und Kommunikationskonflikte in der modernen Gesellschaft (No. 7) Room F

Organizer: Theo Bungarten, Hamburg

Discussants: Theo Bungarten; Helmar Frank, Paderborn; Tatsuo Miyajima, Tokyo; Herbert Penzl, Berkeley; Ingemar Persson, Lund; Inger Rosengren, Lund.

(3) Synchronic Processes in Language Contact Situations World-wide: A Focus on Generalizations and Universals (No. 11) Room D

Organizer: Jacob L. Ornstein-Galicia, Texas

Discussants: J. L. Ornstein-Galicia; Joyce Penfield, Texas; Miwa Nishimura, Pennsylvania; Adolfo Elizaincin, Montevideo; Marvin Herzog, Columbia; Walburga von Raffler-Engel, Nashville; Bates L. Hoffer, Texas; Edgar C. Polomé, Texas.

(4) Language Contact in Europe. Section A: Historical Stratalinguistic Aspects (No. 12) Room E

Organizer: P. Sture Ureland, Mannheim

Discussants: P. Sture Ureland; István Bátori, Koblenz; Hans Goebel, Regensburg; Horst Munske, Erlangen; Baldur Panzer, Heidelberg; Håkon Jahr, Tromsø.

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- (4) *Language Contact in Europe. Section B: Synchronic Aspects of Language Switching and Language Shifting* (No. 13) Room E
Organizer: P. H. Nelde, Bruxelles
Discussants: P. H. Nelde; Richard Trim, Bruxelles; Rudolf Kern, Chaumont-Gistoux; Walburga von Raffler-Engel, Nashville.
Please note: Language Contact in Europe: Section A (4) and Section B (4) are combined into one session. The time allowed is 10:30—16:30.
- (5) *Intonation* (No. 16) Room B. 9:30—16:30
Organizers: Hiroya Fujisaki, Tokyo; Eva Gårding, Lund
Speakers: H. Fujisaki; E. Gårding; Arthur S. Abramson, Connecticut; Gösta Bruce, Lund; Keiichi Hirose, Tokyo; Karen H. Kvakik, Wisconsin; Ilse Lehiste, Ohio; Mark Y. Liberman, Murray Hill; John J. Ohala, Berkeley; Sven Ohman, Uppsala; Miyoko Sugito, Osaka; John T. Hart, Eindhoven; Nina Thorsen, København; Zong-jii Wu, Beijing.
- (6) *Generative Syntax* (No. 27) Room A. 9:30—13:00
Organizer: Henk van Riemsdijk, Tilburg
Discussants: H. van Riemsdijk; Kinsuke Hasegawa, Tokyo; Alec Marantz, Harvard; Akio Kamio, Tsukuba; Takashi Imai, Yamanashi; Noriko Kawasaki, Tokyo; Ryuichi Washimi, Tokyo.

Wednesday, September 1, 1982, 16:00—18:30

- Characteristics of Japanese Expressions in News Reporting* (No. 28) Room D
Organizer: Masatake Muraki, Tokyo
Discussants: Kazuko Inoue, Keiichiro Okutsu, Masayoshi Sagawa, Masayoshi Shibata, Iyoko Hirata, Tokyo.

Thursday, September 2, 1982, 10:30—15:30

- (1) *Linguistic Theory and Language Acquisition* (No. 5) Room A
Organizer: Yukio Otsu, Tokyo. 9:30—16:00
Speakers: Helen Goodluck, Madison; Kazuko I. Harada, Nagoya; Barbara Lust, Cornell; Alec Marantz, Harvard; Thomas Roeper, Amherst; other speakers will be announced later.
Discussants: Masayuki Ike-uchi, Nagoya; (Thomas Wasow, Stanford); (Kenneth Wexler, California); Henry Hamburger, Naval Research Laboratory.

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Papers will be distributed before the Congress. They will become available in July for a nominal fee. It will be assumed at the Congress that participants have read the papers. Interested people should write to: Yukio Otsu, 1-16-17 Omori Minami, Ota-ku, Tokyo 143 JAPAN.

- (2) *The Use of Script Frame in Linguistic Semantics* (No. 6) Room B
Organizer: Victor Raskin, Purdue
Presenters: Ferenc Kiefer, Budapest; Victor Raskin, Purdue; Roger C. Schank, Yale; Deborah F. Tannen, Georgetown.
Participants: above and Renate Bartsch, Amsterdam; Charles J. Fillmore, Berkeley; Jerry L. Morgan, Illinois; Yonck Wilks, Essex; Vladimir A. Zvegincev, Moskva.
- (3) *Sociolinguistic Surveys in Asia* (No. 10) Room C
Organizer: Raja Ram Mehrotra, Varanasi
Discussants: R. R. Mehrotra; Takeshi Sibata, Tokyo; Bh. Krishnamurti, Hyderabad; S. V. Shanmugam, Annamalai; K. Karunakaran, Annamalai; Rodney Moag, Ann Arbor; Anwar S. Dil, San Diego; Toemskadi Krishnamara, Chulalongkorn.
- (4) *Shared Knowledge in Language Use* (No. 17) Room D
Organizers: John Hinds, Pennsylvania; Jeannette K. Gundel, Minnesota
Discussants: J. Hinds; J. K. Gundel; Ellen Prince, Pennsylvania; Dieter Wunderlich, Düsseldorf; Joseph Kess and Ronald A. Hoppe, Victoria; Florian Coulmas, Tokyo; Jerry R. Hobbs; G. P. Boguslavskaja, Saransk.
- (5) *Structural Transitions and Typological Diversities in Sino-Tibetan* (No. 19) Room E
Organizer: Mantaro J. Hashimoto, Tokyo
Discussants: M. J. Hashimoto, Søren C. Egerod, København; William J. Gedney, Michigan; Eugénie J. A. Henderson, London; James A. Matisoff, California; Tatsuo Nishida, Kyoto; Jerry L. Norman, Washington; Ting Pang-hsin, Taipei; Suriya Ratanakul, Mahidol.
- (6) *Nonverbal Behavior for Linguists* (No. 26) Room F
Organizer: Walburga von Raffler-Engel, Nashville
Discussants: W. von Raffler-Engel; Isidoro Blickstein, São Paulo; Forrest Burr, Texas; Bates Hoffer and Jeanne Callihan, Trinity; Nicholas Clark, Nashville; Katherine Fell, Texas; Ann Heins and Elizabeth Keil, Nashville; Fernando Poyatos, New Brunswick.

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Friday, September 3, 1982, 10:30—15:30

- (1) *History (and Philosophy) of Science and Linguistics* (No. 1) Room A
Organizer: E. F. Konrad Koerner, Ottawa
Discussants: E. F. K. Koerner; Mario Bunge, McGill; Terence H. Wilbur, Los Angeles; Jon D. Ringen, Indiana; Stephen O. Murray, Berkeley; James A. Bell, South Florida; Kurt R. Jankowsky, Georgetown; John M. W. Verhaar, Spokane; Ranjit Chatterjee, Singapore.
- (2) *Developmental Linguistics* (No. 3) Room E
Organizer: Charles-James Bailey, Berlin (BRD)
Discussants: Charles-James Bailey; Roy Harris; Heng-hsiung Jeng; Bea de Gelder; Jerold Edmondson; Hermann Parret; Talmy Givón; Kim Sterelny.
- (3) *Functional Grammar* (No. 9) Room B
Organizer: Simon C. Dik, Amsterdam
Discussants: S. S. Dik; A. Machteit Bolkestein, Amsterdam; Mike Hannay, Amsterdam; Jan de Jong and Yuri Okabe, Amsterdam; J. Lachlan Mackenzie, Amsterdam; Nobuya Itagaki and Gary D. Prideaux, Alberta; Ger P. Reesink, Papua New Guinea; Co Vet, Groningen.
- (4) *Non-linear Phonology* (No. 14) Room D
Organizer: Mark Y. Liberman, Bell Labs
Discussants: M. Y. Liberman; Paul Kiparsky, MIT; Shosuke Hara-guchi, Tsukuba.
- (5) *The State of the Art in Language Typology* (No. 18) Room C
Organizer: Christian Lehmann, Köln
Discussants: C. Lehmann; Eugenio Coseriu, Tübingen; Wolfgang U. Dressler, Wien; Talmy Givón, Colorado; Edward L. Keenan; Gilbert Lazard, Paris.
- (6) *Morphosyntax or Seniosyntax?* (No. 22) Room F
Organizer: Claude Hagege, Paris
Discussants: C. Hagege; Susumu Kuno, Harvard; Alain Lemaréchal, Poitiers; G. P. Boguslavskaja, Saransk.

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GENERAL ASSEMBLY OF THE CIPL

On Thursday, September 2, 1982, at 12:30—13:30, the General Assembly of the CIPL will take place at Hall, Nippon Toshi Center. The agenda for the meeting will be announced at the beginning of the Congress. All participants of the Congress are cordially invited to attend the General Assembly, but only the official representative of each country is allowed to vote.

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Prof. E. Vasiliu, Intr. Lucaci 3/11/5, București IV, Rumania.
Prof. S. A. Wurm, The Research School of Pacific Studies, Dept. of Linguistics, Box 4 P.O., Canberra A.C.T. 2600, Australia.

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Prof. V. N. Yartseva, Academy of Sciences of the USSR, Leninsky Prospekt 14, Moscow V-71, U.S.S.R.

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Prof. Chr. A. E. Mohrmann, St. Annastraat 40, 6524 GD Nijmegen, The Netherlands

REPRESENTATIVES AT THE GENERAL ASSEMBLY (1982)

Argentina	Prof. A. Elizaincin, Dept. de Linguistica, Universidad de la Republica, Facultad de Humanidades y Ciencias, Lindolfo Cuestas 1525, Montevideo (Uruguay).
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Bulgaria	Prof. V. Georgiev, University of Sofia, Ruski 15, Sofia.
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Finland	Prof. F. Karlsson, University of Helsinki, Dept. of Gen-

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eral Linguistics, Hallituskatu 11-13, SF-00100 Helsinki
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France	Prof. Jean Perrot, 23 Rue Pierre-Brossolette, 78350 Jouy-en-Josas.
Ghana	Prof. A. S. Duthie, Linguistic Circle of Accra, Dept. of Linguistics, P.O. Box 61, Legon.
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India	Prof. A. R. Kelkar, Deccan College, A-2 Perimal, 1239-Apte Road, Pune 411004 India.
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Kuwait	Prof. M. S. Anwar, Department of English, Kuwait University, P.O. Box 23558, Kuwait.
Libya	Dr. Othmann Hadeili, Faculty of Education, University of Libya, P.O. Box 2558, Tripolis L.A.R.
Netherlands	Prof. B. P. F. Al, Regentesselaan 31, 2341 KN Oegstgeest.
Nigeria	Prof. A. Bamgboye, University of Ibadan, Dept. of Linguistics and Nigerian Languages, Ibadan.
Norway	Prof. E. Hovdhaugen, Linguvistisk Institutt, Universitet i Oslo, Postboks 1012, Blindern, Oslo 3.
Pakistan	Prof. A. S. Dil, United States International Univ., San Diego, Calif. 92131, U.S.A.
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Sweden	Prof. B. Malmberg, Lunds Universitet, Östervångsvägen 42, 223 65 Lund.

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Switzerland	Prof. G. Redard, Jctizikoferstrasse 4, CH 3012 Kirchlin- dach.
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Uruguay	Prof. A. Elizaincin, Dept. de Linguística, Universidad de la Republica, Facultad de Humanidades y Ciencias, Lin- dolfo Cuestas 1525, Montevideo.
U.S.A.	Prof. Arthur S. Abramson, Dept. of Linguistics, U-145, University of Connecticut, Storrs, Connecticut 06268.
U.S.S.R.	Prof. B. Serebrennikov, Academy of Sciences, Marx- Engelsstrasse 1/14, Moscow.
Yugoslavia	Prof. Pavle Ivić, Ohridska 7/IV, Yu-11000 Beograd.

SOCIETAS LINGUISTICA EUROPAEA (SLE)

is going to hold its 15th annual meeting in Athens (at the University of Athens, Greece) from 8 to 11 September 1982.

Main theme: Language Universals and Typology (to be considered from every possible point of view). There will be plenary sessions, section meetings, working groups and round tables.

Participation fees will be 20 dollars (ca. 1200 drachmas).


Correspondence concerning the Meeting should be directed to: Prof. G. Babinotis, Linguistic Department, The University of Athens, Hippokratous 33, Athens 144, Greece.

7th WORLD CONGRESS OF APPLIED LINGUISTICS
Brussels, Belgium, August 5-10, 1984.

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*We are exhibiting our books during the Congress
Participants are kindly invited to pay us a visit at our booth in the exhibition
hall.*

PART 2: LOCATION OF THE ICL 1982

(See Map toward the end of this booklet)

The city of Tokyo is a metropolitan complex covering 832 square miles (2,155 km²) with a population of over 11.6 million. However, the area of Tokyo which concerns most of the Congress participants would be the central portion marked by the loop train system called YAMANOTE Line [ya-ma-no-té-sen] 山手線 and by the CHUO Line [chu-o-sen] 中央線 cutting across the Yamanote loop in a general east-west direction. Both of these lines are elevated surface trains belonging to the Japan National Railways 国鉄 (henceforth JNR).

Nippon Toshi Center 日本都市センター (henceforth NTC), where the Congress will be held, is located around the centre of the Yamanote loop, between the Yotsuya [yo-tsu-ya] 四ツ谷 station of the JNR Chuo line and the Imperial Palace. See the map below. Within the vicinity of NTC are Hotel New Otani, Akasaka Tokyu Hotel and Akasaka Prince Hotel. For those taking public transportation to NTC, the use of subway lines is recommended.

General information on the Tokyo traffic system is available at the airport and hotels. The following description on the access to NTC via subway is meant only as a supplementary guide for the Congress participants.

1. Tokyo Subway

The Tokyo subway is a complex network of ten different underground lines crossing each other and JNR lines in many places. Hence, the major stations are a complex of platform and extended concourses with many exits to the ground level. You may often find more than two alternative routes for same destinations. Before using the subway system, therefore, inquiry or study of maps in advance is recommended. Proper choice of lines, stations and exits may save a great deal of walking distance and time. Some useful information to remember when using the Tokyo subway are:

- ① is the general symbol for the subway 地下鉄 on maps and on the street as signs for entrances to subway stations.
- ② Each subway line has its own color for identification. The identification color is used in subway maps, on subway car exteriors, and as direction guides (circular symbol) in stations and terminals.

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- ③ Most stations provide detailed direction boards and maps of the vicinity. Although they are written mostly in Japanese, those who are familiar with the Chinese script would also find them useful.

2. Subway Lines & Stations to the Congress

There are three subway stations in the vicinity of NTC and they are served by four lines (routes) of the subway as summarized below. For each line, the identification color is indicated in []. Choice of lines and stations for individual participants is to be determined by consulting maps and/or by inquiry.

- a) AKASAKA-MITSUKE (a-ka-sa-ka-mi-tsu-ke) 赤坂見附 Station.
Served by: i) Marunouchi (ma-ru-no-u-chi) Line 丸の内線 [red], and
ii) Ginza (gin(g as in "geese")-za) Line 銀座線 [orange].
- b) KOJIMACHI (kô-ji-ma-chi) 麹町 Station.
Served by: Yurakuchô ("you"-ra-ku-chô) line 有楽町線 [pale yellow]. This station and the following Nagatacho are adjacent stops on the Yurakucho line.
- c) NAGATACHO (na-ga-ta-chô) 永田町 Station.
Served by: i) Yurakuchô Line (see above), and
Served by: ii) Hanzômon [han-zô-mon] Line 半蔵門 [purple].

Please note that Akasaka-mitsuke is the busiest of the three stations, and the walk from there to NTC is uphill and a little longer than the other two.

3. Walk from Subway Stations to the Congress

Use Map above along with the following guide on the walk from any of the three subway stations. General landmarks for NTC within the vicinity of these stations are Akasaka Prince Hotel high-rise and an adjacent steel-frame tower. In the description of the stations, an "exit" refers to a stairway leading to the street level while a "ticket gate" is an area where tickets are punched and collected.

a) Akasaka-mitsuke Station

Since two lines cross here, there are two levels of platforms. Note that the Marunouchi Line cars are red while the Ginza Line cars are orange.

- ① As there is only one ticket gate in this station, just follow the exit signs to the ticket gate.
- ② After the ticket gate, go straight and follow the signs marked "Exit for Akasaka-mitsuke Crossing" and use this exit.
- ③ On reaching the street level, you should see on your left another stairway for a pedestrians' bridge crossing. Use this bridge.

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- ④ The 2nd floor of "Akasaka Tokyu Plaza" terrace (part of Akasaka Tokyo Hotel building) is at the end of the bridge. On reaching the terrace turn left and follow the terrace which will merge into the sidewalk of an uphill street.
- ⑤ Follow the sidewalk uphill and you should see another pedestrians' bridge. Use it. As you cross the bridge, you should be facing a tall building. This high-rise is a part of the Akasaka Prince Hotel complex.
- ⑥ At the end of the bridge, go uphill again.
- ⑦ Turn left at the first corner, and walk alongside the hotel.
- ⑧ After the hotel complex you should see a steel structure tower on your left and the NTC on your right.

b) Kojimachi Station

Unlike the case of the other two stations, individual exits of this station bear no names or codes.

- ① Getting off a Shintomicho bound train from Ikebukuro, follow the direction of the train on the platform to the end and use the ticket gate marked "Exit for Kojimachi".
- ② Those on an Ikebukuro bound train from Shintomicho would normally get off at Nagatacho. But if they happen to choose Kojimachi, walk against the train direction on the platform and follow the rest of ① above.
- ③ After the ticket gate, turn left to follow the sign "Exit for Hanzômon & Yotsuya" and take an escalator (or stairs).
- ④ Turn left again to take stairs to the street level. At the exit, you should see a bank with a three-diamond trade mark. (Mitsubishi Bank 三菱銀行. They handle foreign currency exchange)
- ⑤ Turn right along the bank corner and go straight.
- ⑥ You will soon see a slim steel tower ahead. Just before reaching this tower is NTC (on your left).

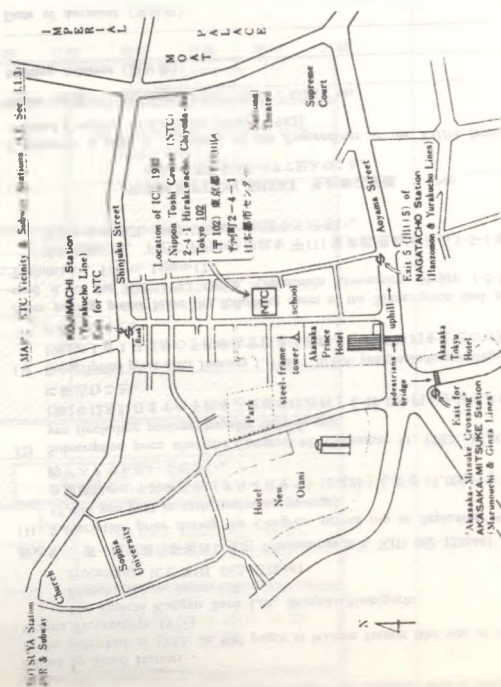
c) Nagatacho Station

All the exits of this station are numbered in Arabic numerals.

- ① Direct yourself to "Exit 5" (出口 5) as soon as you get off the train, by boards and maps or by asking.
- ② Use "Exit 5" to the street level, go downhill for a block and turn right at the corner of a high-rise in the Akasaka Prince Hotel complex.
- ③ After the hotel complex you should see a slim steel structure tower on your left and NTC on the right.

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LOCATION of the ICL 1982



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SOME USEFUL JAPANESE EXPRESSIONS

(Pronounce each word as it is written; *ch* as in *church*, *sh* as in *sure*, *j* as in *Japan*.)

Konnichiwa, Tanaka-san. Hello, Mr. (Mrs. Miss) Tanaka.
Konnichiwa, Smith-san. Hello, Mr. (Mrs. Miss) Smith.
Ogenki desuka? How are you? (*o* honorific; *genki* fine, well, healthy; *desu* am, are, is; *ka* interrogative particle)

Arigatō, genki desu. I'm fine, thank you. (*arigatō* < *arigatai* [your kindness] is precious; *aru* to be; *gatai*, *katai* difficult, hard)

Arigatō. (informal) Thank you.
Arigatō gozaimasu. (polite) Thank you.
Dōmo arigatō. (informal) Thank you very much. (*dōmo* anyhow)
Dōmo arigatō gozaimasu. (polite) Thank you very much.
Dō itashimashite. You're welcome. (*lit.* I haven't done anything special)

Ohayō. (informal) Good morning. (*hayai* early, quick)
Ohayō gozaimasu. (polite) Good morning.
Konnichiwa. Good afternoon; Guten Tag; Bon jour.
Konbanwa. Good evening. (*kon* this; *ban* evening)
Oyasumi nasai. Good night. (*lit.*: have a rest; *yasumu* take a rest)

Asa gohan. Breakfast. (*asa* morning; *gohan* rice)
Hiru gohan. Lunch. (*hiru* daytime, midday, noon)
Ban gohan. Supper. (*ban* evening)
Shokuji. Meal. (*shoku* eat, eating)
Shokuji ni ikimashō. Let's go and eat. (*ni* to, for; *ikimashō* let's go; *iku*, *yuku* to go)

Nippongo. Japanese language
Gengo. Language
Gengogaku. Linguistics
Gengogakusha. Linguist. (*inter*)
Kokusai. International. (*koku* country, nation; *sai* Congress. (*kai* meeting; *gi* discussion)
Kaisi. Congress. (*kai* meeting; *gi* discussion)
Sayōnara. Goodbye
Mata oai shimashō. Auf Wiedersehen, au revoir, hasta la vista, do svidaniya. (*lit.*: Let's meet again)

Gomen go ni dokode? Where in five years? (*again*)
Ogenki de! (*lit.*: Be fine) Wish you good health. Alles Gute!

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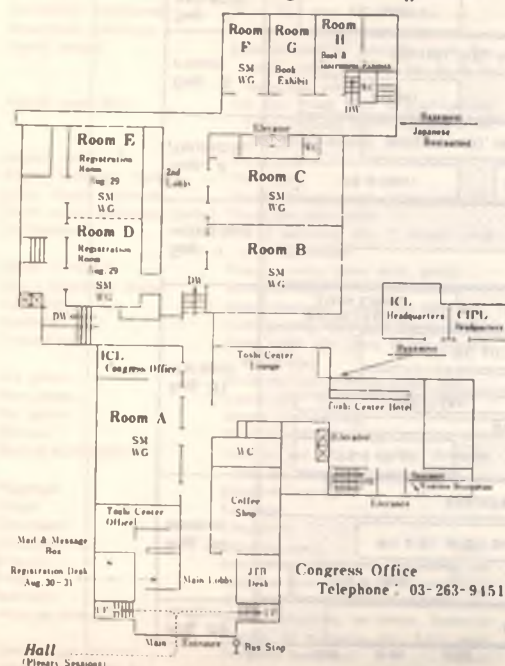
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	9:00	10:00	11:00	12:00	13:00	14:00	15:00	16:00	17:00	18:00	19:00	20:00	21:00
Aug. 29 SUNDAY													RECEPTION (Akanaka Prince Hotel)
Aug. 30 MONDAY													
Aug. 31 TUESDAY													
Sep. 1 WEDNESDAY													
Sep. 2 THURSDAY													
Sep. 3 FRIDAY													
Sep. 4 SATURDAY													

Note: OC=Opening Ceremony; PS=Plenary Session; SM=Section Meeting; WG=Working Group; GA=General Assembly of CIPI; CC=Closing Ceremony. Capital letters in parentheses indicate room code (See layout of the Congress Building of this program on the inside of the back cover).

Layout of the Congress Building



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TOKYO, AUGUST 29—SEPTEMBER 4, 1982

Editor: SHIRŌ HATTORI

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This reproduction of the title pages of previous *Proceedings* (1930–1978) is intended to give a bird's-eye view of the history of the International Congress of Linguists (1928–1982). See also p. iii and pp. viii–ix of this volume.

1. Actes du premier congrès (La Haye 1928), Leyde 1930 1431, 1432
2. Actes du deuxième (Genève 1931), Paris 1933 1433
3. Atti del III congresso (Roma 1933), Firenze 1935 1434
4. Actes du quatrième congrès (Copenhague 1936), Copenhague 1938 1435, 1436 bottom
5. V^{me} congrès (Bruxelles 1939, annulé) 1436 top, 1437 bottom
6. Actes du sixième congrès (Paris 1948), Paris 1949 1437 top, 1438
7. Proceedings of the Seventh Congress (London 1952), London 1956 1439, 1440
8. Actes du huitième congrès (Oslo 1957), Oslo 1958 1441, 1442 bottom
9. Proceedings of the Ninth Congress (Cambridge, Mass. 1962), The Hague 1964
..... 1442 top, 1443 bottom
10. Actes du X^e congrès (Bucarest 1967), 4 tomes, Bucarest 1969–1970
..... 1443 top, 1444, 1445 bottom
11. Proceedings of the Eleventh Congress (Bologna-Florence 1972), 2 vols. Bologna 1974
..... 1445 top, 1446
12. Proceedings of the Twelfth Congress (Vienna 1977), Innsbruck 1978 1447, 1448
13. Proceedings of the XIIIth Congress (Tokyo 1982), Tokyo 183 See this volume.

ACTES DU PREMIER
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A LA HAYE DU 10—15 AVRIL 1928



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ACTES DU DEUXIÈME CONGRÈS INTERNATIONAL DE LINGUISTES

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(ROMA, 19-26 SETTEMBRE 1933-XI)

A CURA DI
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FRANCE
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Reposés

auxquelles auront donné lieu les documents publiés ici et éventuellement des résumés des communications de sections qui seraient parvenues au comité avant la fin du mois de mai.

Le présent recueil n'a donc qu'un caractère provisoire. Néanmoins, le comité a cru que le lecteur lui saurait gré d'avoir tenté un classement rationnel des notes qu'il a reçues. Celles-ci sont groupées d'abord d'après les sujets proposés. Dans chaque série, on trouvera d'abord les réponses qui étudient les problèmes d'un point de vue général, ensuite celles qui en examinent un aspect particulier sans se rapporter spécialement à certaines langues ou à certains groupes de langues, puis successivement les avis concernant un seul groupe linguistique, ceux qui traitent de quelques langues d'une famille, ceux qui ne considèrent qu'une seule langue, enfin les réponses qui envisagent les questions d'un point de vue comparatif. L'ordre alphabétique des noms des auteurs a été adopté pour le classement des diverses notes d'une même catégorie. Certaines communications nous étant parvenues alors que ce recueil était déjà en épreuves, nous avons dû parfois, pour des raisons de mise en page, déroger à cet ordre de principe. Une table des matières et un index figurent à la fin du volume.

Presque par le temps, le comité n'a pu soumettre aux intéressés les épreuves de leurs réponses. Il a dû les corriger lui-même assez rapidement: bien qu'il ait apporté un grand soin au respect scrupuleux du texte des auteurs, il s'excuse auprès de ceux-ci, comme auprès des lecteurs, des fautes qu'ils pourraient relever dans cette publication.

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